The authors respond:

I sincerely thank Dr McCaughey and his collaborators for their interest in our work.[1] With respect to your comments, I agree that it is problematic to adequately estimate the risks of favorable or unfavorable events when the different outcomes are related and, rather, when the probabilities of their occurrence compete with each other, as you have well exemplified with respect to mortality and liberation from mechanical ventilation. Furthermore, I agree that our results are applicable only to the population that finally achieved liberation from invasive mechanical ventilation, which does not mean that such estimations of the difference in mechanical ventilation duration between the group of subjects who were critically ill and who received neuromuscular electrical stimulation and the control or sham neuromuscular electrical stimulation group is biased but rather responds to a different and more specific research question.

It would be interesting to consider using a shared parameter model[2] to estimate the risk of "suffering" of a specific event through a meta-analysis, as you suggest, as opposed to an intervention research question, which involves a "risk competition." For this, randomized clinical trials included in a meta-analysis should consider among their outcomes the evaluation of the incidence of these events at a fixed follow-up time, such as mortality versus liberation from invasive ventilation at day 28 from the start of ventilatory support. In this way, the relative risks of dying and achieving liberation from invasive ventilation could be estimated for a given follow-up. Therefore, future systematic reviews that seek and can conduct meta-analyses according to the data available in the included randomized clinical trials could adequately estimate the risk of different outcomes at a given time[2] (categorical variables, such as death, liberation of mechanical ventilation, discharge from the ICU at a given time), and also establish the effect of an intervention on quantitative outcomes, such as the ICU length of stay or the duration of invasive ventilation, thus providing a more complete picture of the landscape studied.[2]

However, it should be noted that the determination of a single quantitative estimator, such as "time on mechanical ventilation," is not free of problems that can introduce biases in this estimation, as was detected in our work.[1] This is the case of the study by Routsi et al[3] due to the large loss of participants, as you rightly point out, and which we did consider because we rated the risk of bias arising from attrition as high.[4] In any case, it should be considered that, although the losses were high in the study by Routsi et al,[3] they were balanced between the groups (66% in the intervention group and 61% in the control group), which could have an impact on the estimation of the risk of discontinuation of mechanical ventilation within the groups rather than on the difference in risk between the groups, thanks to the random assignment of the participants to the different groups.

Finally, and regretting the inaccuracy in the data extracted from the study by Dall'Acqua et al[5] and in the reporting of the statistical significance value of the study by McCaughey et al[6] (not included in our meta-analysis), I reaffirm our conclusion of the effect of neuromuscular electrical stimulation on the duration of mechanical ventilation,[1] because, although the mean difference changes slightly when all studies are considered (mean difference –2.83 [95% CI –3.88 to –1.78] d), the "low certainty of evidence" rating for the totality of studies was not determined by such mean difference, together with its 95% CI (input to assess imprecision and inconsistency within the GRADE (Grading of Recommendations Assessment, Development, and Evaluation) approach,[7] but, due to the very serious risk of bias of the included studies, mainly derived from problems in the generation and concealment of the random sequence.

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