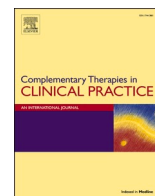




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## Letter to the editor

**Letter to the editor: Respiratory rehabilitation in elderly patients with COVID-19: A randomized controlled study**


## ARTICLE INFO

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SARS-CoV-2

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## Dear Editor,

We have read with great interest the study by Liu et al., which investigated the effects of respiratory rehabilitation on respiratory function, quality of life, mobility, and psychological function in elderly patients with COVID-19, in the May 2020 issue of *Complementary Therapies in Clinical Practice* [1]. We commend the authors' effort to quickly provide evidence-based data on the effects of respiratory rehabilitation in patients with COVID-19, with the aim of treating the effects of this new pathology that has radically changed our existence [2–4]. However, the need to quickly produce data cannot be at the expense of reporting quality.

Indeed, we believe that the reporting of this study suffered from some methodological pitfalls that may have biased its results and conclusions. Further, the report did not indicate the trial registration number for this protocol, which would have helped us understand what was planned and what was actually done. Thus, based on the reporting of this study, we here raise some methodological and conceptual concerns, which in turn have implications for the study's internal and external validity.

Firstly, while the title and the study design section clearly state that this was randomized controlled trial, the abstract refers to it as “an observational, prospective, quasi-experimental study”. Further, it seems that the local ethics committees approved the implementation of a survey. We therefore feel that a clear, transparent definition of the study design, of pivotal importance as readers base their interpretation of the research data on such information, is lacking [5].

Moreover, to correctly estimate the effects of an experimental intervention, especially when the intervention is complex (as rehabilitation treatments usually are), it is important to provide a description of the components of both the experimental intervention and its control [6]. However, while Liu and collaborators state in the Abstract that the control group did not participate in any rehabilitation programme, in the Results section it appears that patients who dropped out from the control group were actually involved in rehabilitation. Further, this programme is not described. In addition, although the components of the experimental rehabilitation intervention are quite well described, we do not feel the dosage is, thus complicating any replication of this trial, which is recommended to confirm its results [7].

Along with these methodological issues, we, as rehabilitation professionals, would have expected to read the theoretical background on which the experimental intervention was grounded. Yet this information was lacking. For example, we could not identify the rationale for testing cough exercise in all the patients involved, considering that sputum production seems to be a clinical feature in only about 30% of individuals with COVID-19 [8,9]. Liu and collaborators based the background of their study on research conducted in individuals with Parkinson's disease hospitalized for pneumonia [10] and on research conducted in individuals with musculoskeletal disorders, without respiratory diseases [11]. The authors seem to hypothesize that respiratory rehabilitation useful to individuals with chronic obstructive pulmonary disease might also be of help to elderly patients discharged from hospital after SARS-CoV-2 infection. However, in our opinion, they fail to elucidate why the components of the respiratory rehabilitation they tested could help these patients.

Moreover, we struggled to identify the population under study, which is described simply as individuals “discharged with satisfying results”. We assume the authors meant that the patients had adequate respiratory function, but a more complete description of the patients studied would have helped us readers determine the generalizability of the results. In fact, while rehabilitation interventions frequently need to be carefully tailored to individual characteristics, the participants in Liu et al.'s study are not well described [4], thus not allowing professionals to do this. Hence, a more detailed description of the patients investigated, including their symptoms, clinical history, devices in use, and so on, would have been beneficial [6].

Finally, we believe that some citations are not the most appropriate to support the authors' arguments (e.g. Maki, 2018 [11]; Levi, 2018 [12]), and we could not help but notice that the reporting is littered with English language mistakes, which can lead to potential misunderstandings (e.g. “chronic obstructive non-disease”).

Despite these important issues, however, this study has been cited numerous times as it is the first to assess the efficacy of respiratory rehabilitation in COVID-19. Though, based on this report, we are unwilling to subscribe to this statement.

We agree with the authors that a randomized controlled trial represents the highest level of evidence to assess the efficacy of respiratory

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rehabilitation in patients with COVID-19, and we appreciate the acknowledgement that the small cohort studied suggests implementing further double-blind studies with a larger sample size to collect more evidence. Nevertheless, we think that it is equally important to report studies accurately, as suggested by current guidelines [6], as speed cannot be at the expense of the quality of data reporting, so that even small studies can be of help for clinicians, especially when dealing with new, destabilizing pathologies.

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### Declaration of competing interest

None.

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Barbara Bressi\*

*PhD Program in Clinical and Experimental Medicine, Department of Biomedical, Metabolic and Neural Sciences, University of Modena and Reggio Emilia, Reggio Emilia, Italy*  
*Physical Medicine and Rehabilitation Unit, Azienda Unità Sanitaria Locale-IRCCS di Reggio Emilia, Viale Risorgimento 80, 42123, Reggio Emilia, Italy*

Sara Paltrinieri, Stefania Fugazzaro  
*Physical Medicine and Rehabilitation Unit, Azienda Unità Sanitaria Locale-IRCCS di Reggio Emilia, Viale Risorgimento 80, 42123, Reggio Emilia, Italy*  
 E-mail addresses: [sara.paltrinieri@ausl.re.it](mailto:sara.paltrinieri@ausl.re.it) (S. Paltrinieri), [stefania.fugazzaro@ausl.re.it](mailto:stefania.fugazzaro@ausl.re.it) (S. Fugazzaro).

Stefania Costi  
*Scientific Directorate, Azienda Unità Sanitaria Locale-IRCCS di Reggio Emilia, Viale Umberto I 50, 42123, Reggio Emilia, Italy*  
*Department of Medicine, Dentistry and Morphological Sciences, University of Modena and Reggio Emilia, Modena, Italy*  
 E-mail address: [stefania.costi@unimore.it](mailto:stefania.costi@unimore.it).

\* Corresponding author. Viale Risorgimento 80, 42123, Reggio Emilia, Italy.  
 E-mail address: [barbara.bressi@ausl.re.it](mailto:barbara.bressi@ausl.re.it) (B. Bressi).