Author's response to reviews

Title: Impact of physical fitness and biometric data on the quality of external chest compressions: a randomised, crossover trial

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Title: Impact of physical fitness and biometric data on the quality of external chest compression - a randomized, cross-over trial

Reviewer's report and point-by-point

Dear Deesha Majithia,

Thank you very much for the opportunity to revise our manuscript. We also would like to thank the reviewers for their helpful comments.

We have revised our manuscript according to the reviewers comments and suggestions. Please find our point-by-point response to the assessors’ concerns.

We hope to meet your and the reviewers expectations and looking forward to your comments.

Best regards

Sebastian Russo
Report #1

Abstract Line 2 In before 2005 Done.

Background Line 3 remove with and add in. Done.

The hypothesis is missing. Added

Last paragraph remove and before on (last line). Done.

Methods Specify oral and written consent. Done.

Remove the before formal. Please rephrase first paragraph Ergospirometry (please specify which endurance tests) Done. first line. last line were (typed we only). Done.


Subjective assessment. there is no scale or open label answers provided. Clarified

Statistical analysis. this is probably the most and only confusing paragraph.

Please consider revision. in particular the investigators needs to provide the power analysis or explain why this study was a pilot. They provided informations that this type of study is not new, but they are providing the first of its kind.

There have been previous some studies addressing the impact of physical fitness on ECC. However, to the best of our knowledge none of the existing studies investigated physical fitness as objectively as we did in the presented manuscript. Hence, it was difficult to estimate the
sample size. We screened the recent literature dealing with similar topics and methodologies (Field et al., Resuscitation, 2011; Yannopouls et al., Crit Care Med., 2006; Bjorshol et al., Resuscitation, 2008) and found that the power analyses were not provided (probably for similar reasons) without claiming these studies to be pilot studies. However, we added “exploratory study” to the method section and would be grateful if you and the editor would accept this section as submitted.

Please consider revision of figures with SEM. SEM is acceptable only if number of observation are provided, but is not overall a measure of statistical difference between two comparison groups. We agree. The numbers of observations (subjects per group) are now explained in the manuscript. Furthermore, all statistical tests and presentations refer to SD and CI. Only in the figures, for reasons of illustration and overview, have we chosen to use SEM (see Field et al., Resuscitation, 2011).

Order the presentation in a. Two way Anova, post hoc, linear correlation. Done. I would remove the sentence about normally distributed data. Done

**Results.** please consider removing all the subtitle and rephrase the first sentence of each accordingly. i.e. second paragraph Nine minutes of ECC. Last sentence not clear (about compression depth) Page 2 all participants minute by minute.

Based on a minute by minute... Done, if appropriate and helpful for reading the manuscript.

What was the amount decrease? can you provide a percentage of change if minute 1 is 100% (or 1) what would be the other? (it is easier for the reader to
see the percentage relative change and decrease). Please apply to all sections

that analyze the minutes into ECC and performance decrease. We understand that the relative
decrease of, for example, the depth of ECC is probably easier to understand if the data were
normalised to the first minute. Indeed, during manuscript preparation, we thought to do so.
However, we decided to present the absolute values in order to give the readers an overview
about the absolute compression depth without presenting additional tables. Nevertheless, in the
main text we added the relevant percentage. We hope this is acceptable.

Subjective need to provide classes or scores or assessments.

We have clarified in the manuscript that candidates only had to check the mark; thus, we are
unable to provide scores or classes. We have to acknowledge this as a weakness regarding the
data on the basis of subjective perceptions.

Discussion

3rd paragraph Important...(don't understand) What is the clinical relevance of
addressing BMI for ECC?

Effective chest compression requires physical force. It seems as the required force can be
generated by muscular strength or by “using” an individual’s weight of the upper body to
passively compress the chest.

During data analyses, we were afraid that deeper chest compression found in subjects with higher
BMI may be misinterpreted due to insufficient chest recoil based on the subjects leaning on the
patients’ thorax. The Skill-Reporting software does not provide decompression depth; therefore,
we used MatLab analyses to evaluate the decompression depth and the compression amplitude
(difference between compression and decompression depth). As shown in Tables 2 and 3, males
and subjects with higher BMI tended to lean on the patient’s chest, causing less chest recoil than lighter subjects represented by a deeper de-compression depth. Nevertheless, the compression amplitude was still higher in subjects with higher BMI as compared to those with lower BMI. Therefore, the increased BMI of healthcare providers performing ECC seems to have more positive than negative effects.

We speculated that an increased BMI might have been associated with increased muscle mass and higher physical fitness. No significant correlation of BMI and physical fitness was found for male and female subjects analysed separately. Therefore, BMI and physical fitness do independently influence the quality of ECC.

Influencing factors within the sentence addressing fatigue... which are these influencing factors? are the one in the paragraph following? Then they should be mentioned only later. Thank you for that advice. We deleted “influencing factors”, as it does not add any additional information in that context.

**Limitations** please discuss further the sample size and the composition of the investigational cohort. Done

**Tables**, please provide number of patients and relative numbers for the sub groups (how many were BMI male low and high etc?) Done and specified in the manuscript.

Figures consider SD instead of SEM. Please find our comment above.
Report #2

No major revisions are necessary prior to publication but a lot of minor and discretionary revisions that are listed below and in the additional pdf of the draft paper uploaded with my remarks.

MINOR REVISIONS:

1. Please explain shortly, why you have chosen 9 min of CPR. Because the guideline recommendation to change the rescuer every 2 min during compressions due to fatigue was included in 2005. The fact that a lone single rescuer has to do more minutes of CPR while waiting for an ambulance might be a good reason. Thank you for this comment. We have added this point to the manuscript. Indeed, approximately eight minutes is the average time needed by the professional healthcare provider to arrive on scene in the case of an out-of-hospital cardiac arrest. Another reason is related to human physiology during endurance tests. Physiological equilibration for a defined physical strain takes approximately three minutes (see also physical fitness tests). Therefore, we set the ECC sequence to nine (3x3) instead of eight minutes.

2. Please mention how your randomisation was carried out? Done

3. Every time you mention that a “high(er) BMI” is a factor for better ECC performance, you should clearly define what “high/higher” stand for in this context. Otherwise this might implicate, that a reader misinterprets it as a correlation between real high BMI (>30) and good quality CPR, which was not
your finding, because almost no participant in your study had an obese BMI. Thank you for that point. We clarified this in the abstract as well as in the main text.

4. There should be real data and p-levels presented for the main findings in the abstract. You just report the quality of your findings, like “performed better” etc. We understand this point. However, we believed the Abstract section would become too long for presenting all the available data (15:2 vs. 30:2, male vs. female, higher vs. lower BMI and higher vs. lower HR75). We added real data and $p$-levels where these seemed possible without lengthening the Abstract too much.

5. The square brackets with the citation numbers must be inside the sentence they belong to, not on the beginning of the next one. This has to be readjusted in the whole paper. Done

6. In the methods section you are reporting about recruitment of 40 participants in total and about your exclusion criteria, but in the results section, you did not report about excluded candidates. So either nobody of the 40 people asked to take part met exclusion criteria or the 40 in total are the result of the amount 40+$n$ asked and $n$ persons excluded because of the previous given criteria. This should be reported or clarified respectively. Clarified

7. It should be mentioned in the limitations section, that the participants did not a complete CPR, because the ventilation on your ALS manikin (without the possibility to give mouth-to mouth rescue breathing I suppose) was therefore just
imitated. The fatigue during CPR might be different with real and correct rescue
breaths. Done

8. Please add a short overview of the mixed professions in your candidate group
(how many paramedics, nurses, physicians). Done

9. Do you have an explanation for the finding that all participants didn’t allow full
chest recoil through leaning on the patient? I know it is a common problem, but I
don’t remember a study that found every candidate doing it. Maybe these very
new publications are helpful to discuss this:

a. Field RA, Resuscitation 2011 [Epub ahead of print]

b. Yeung J, Resuscitation 2011

c. Fried DA, Resuscitation 2011

This is an interesting point. We believe it may be attributed to the different definitions and
technical abilities for detecting an incomplete chest recoil. We addressed this important
discussion in our manuscript.

10. Please exchange the term “frequency” with “rate” in the whole paper and in
tables if you are talking about the speed of compressions, because compression
rate is the common term that is used in the guidelines and other publications.

You used both terms equivalent in your paper. Done.

11. You mention the correct compression depth as 38-50 mm. In the metric
system it has been 40-50 mm and if you take the inches of 1.5-2.0 you have

38-51 mm. Done

12. The link included in citation [20] should be renewed, because it doesn’t route
to an article about females in EMS. Besides this doesn’t seem to be a scientific
citation at all and you might delete it completely. Deleted

13. I marked and changed some obvious mistakes in spelling in the paper, which
should be corrected. You find my corrections in the attached pdf of your draft
version. Thanks a lot.

14. The figure count in purple on the lower left side of the sheets in the pdf
document is wrong but the As and Bs are forgotten and therefore you numbered
7 instead of 4 figures. The figures it self seem to be numbered correctly. It seems as the
numeration you are revering to has been created during the online-submission process?

DISCRETIONARY REVISIONS:

1. I suppose it is more common and rational to use the plural “chest
compressions” throughout the whole paper instead of “chest compression” like
you do. The plural is used constantly in the guidelines as well and this is helpful,
because the focus in your paper isn’t a single compression. Corrected.

2. Consider to delete figure 1, because it is very easy to understand the meaning
and it is already mentioned in the text. There should be no problems if you cut
the paper down for this redundant information. Due to the discussion regarding leaning, we added information based on the threshold as defined by Fried et al. We would prefer to keep the figure in the manuscript.

3. I recommend not mentioning a tendency towards significance, especially not in such a small group. No finding needs that debatable definition for your conclusion, if you have a p-level between 0.1 and 0.05. We understand your point, but we may want to disagree. Indeed, a $p$-level of 0.05 (5% error probability) is the definition of significance commonly used. However, this does not automatically mean that an effect or a finding with a $p$-level of, for example, $p = 0.075$ and a corresponding error probability of 7.5%, is not existent. If these findings are in line with other results, the contrary seems to be the case: based on the small number of female participants, a $p$-level below 0.1 usually indicates a huge effect, which could easily show a $p < 0.05$ just by increasing the sample size. Therefore, we would appreciate being able to present $p$-levels between 0.1 and 0.05 as “tendencies towards significance” in this context.

4. There is a very new publication about your topic that you should consider to mention and discuss shortly:


5. The term “caregiver” seems to be uncommon for this topic. Discuss to exchange it by rescuer, provider, responder…. Caregiver changed to rescuer.

6. Consider to add a sentence about feedback devices (stand alone or included
in a defibrillator/AED) in your discussion. Feedback is an important option in providing high quality compressions, for example if someone is constantly compressing to shallow, like the female candidates in your study. This is clearly pointed out in the 2010 guidelines as well. Thank you very much for this good point. We added this.

Level of interest: An article of importance in its field

Quality of written English: Acceptable

Statistical review: No, the manuscript does not need to be seen by a statistician.

Declaration of competing interests:

There are no competing interests in conjunction with my review. I published about chest compressions, especially with CPR feedback devices, in the ERC-Journal "Resuscitation" and national Journals in Germany but I never was financially involved and never got a salary from any institution or company.