



Letter to Editor

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Letter to the Editor; Resistance training and its impact on psychological health in participants of corporate wellness programs

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Dear Editor,

We read with interest the recent article titled “Resistance training and its impact on psychological health in participants of corporate wellness programs” by Serra, et al. [1].

We would firstly like to thank the authors for their continued work on frequency of resistance training programs, and notably, in that of a corporate wellness program. This is something that appears to have profound positive influence on general health. However, our examination of the article has produced some questions, and whilst the word constraints of the publication possibly limited the authors, we felt that raising these questions in a public forum allows the authors to elaborate on some specific details in a published response, should they see fit.

Firstly, we wanted to ask regarding the significant increases in the general health questionnaire (GHQ) scores for the control group. The data suggests that GHQ scores are not increased to any greater degree for persons engaging in 12-weeks of resistance exercise (RE; pre = 16.5, post = 11.3; $p < 0.001$) compared to participants of a non-exercising control group (CON; pre = 14.0, post = 9.4; $p = 0.018$). The authors report small differences in effect size (ES) in favour of the RE groups; RE = -0.96 vs. CON = -0.74, without statistically significant between-group differences ($p = 0.727$). However, we felt little attention is given to this in the manuscript. The authors state that “a possible justification could be that the possibility of participating in the study voluntarily motivated these individuals to practice physical exercise due to a common work environment” continuing “in this context, the psychological condition of the control group could have been affected by the interaction with their colleagues who trained at the corporate gym.” This is certainly an interesting comment – that they spontaneously decided to begin RE because they knew their peers were engaged in a RE program. However, if participants might have engaged in RE or were impacted in any way by the activities of the intervention group, or the occurrence of the study then at a simple scientific level this ceases to be a true control group as they were not unaffected by the training intervention.

Secondly, we would like to ask how the corporate wellness program was implemented. There are no details provided within the publication and we believe that this might be impactful to the outcomes. For example, if employees were provided time from their working day as a ‘break’ to engage in the RE then was a break of equal duration provided for all the other RE groups to match frequency (e.g. did the group only training 2x per week also receive 2 breaks from their work, and did the control group receive 4 such breaks from work to frequency-match the intervention groups, etc.)? If this is the case then having 4 additional breaks from work in the week might be sufficient to improve GHQ scores as the control group did in the present study. However, if the requirement was to participate in RE in a company environment outside of normal office hours then any positive outcomes might have been negated by a feeling of still being at work. We also ask the authors to clarify the nature of the job roles of the participants from this “specific company”. We believe there would likely be differences between blue- and white-collar

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employees and disparity between both initial and change in GHQ scores between, for example; lawyers, bankers, debt collectors, insurance sales persons, secretaries, etc.

Thirdly, we are interested in the more specific points as to how the RE program was implemented. For example, were the RE sessions supervised? If so; at what ratio of trainer: client? Research has consistently shown that supervised sessions result in greater strength and power adaptations than unsupervised sessions [2] and more so, that favourable trainer: client supervision ratios (e.g. 1:5vs. 1:25) result in greater strength adaptations [3]. Since research has also previously considered the affiliation of a participant to their resistance training programme (notably including 1:1 and small group ratios [4]), we think it is important to consider whether supervision, and if so; the ratio, occurred since it might have impacted physiological and psychological adaptations; including those in the GHQ. In addition - to what intensity of effort did the participants within the RE groups exercise? A growing body of evidence is suggesting that training to a high intensity of effort (e.g. to muscular failure) might be more impactful for strength adaptations than training volume, and furthermore; might affect perception of outcomes and motivation to continue training [5]. In this sense, we would be interested to know the intensity of effort applied by participants performing RE within the study, perhaps using the following set endpoints definitions [6].

Fourthly, we were surprised to see that the authors did not report completing a post-intervention strength assessment. Notably, research supports increases in strength are linked to improved psychological health such as reduced anxiety [7], reduced depression [8], and improved self-esteem [9]; all factors in the GHQ (questions 2; 9; 10 & 11, respectively). It would be interesting to know if there is a cause and effect relationship between participation, or specifically strength increases. However, upon searching, we believe the authors have reported elsewhere that “All [training] groups showed increases in loads of 10RM for all exercises after three months of training ($p<0.05$)” in a separate publication [10]. Perhaps the authors can confirm that these data are from the same study, and might elaborate as to these strength increases (e.g. whether testing occurred or whether this was simply an increase in training load, and to what degree for each group, etc.).

Finally, since the article repeatedly discusses the benefits of RE for depression and anxiety and even considers the individual analysis of question 9 independently, we wonder why the authors did not consider the use of a 3-factor analysis as suggested by Gao, *et al.* [11] whom the authors cite. Gao, *et al.* [11] suggest that the 12-item scale, whilst appropriate for unifactorial analysis, can also be divided in to 3 separate factors; Anxiety and Depression (questions 2, 5, 6, and 9), Social Dysfunction (questions 1, 3, 4, 7, 8, and 12), and Loss of Confidence (questions 10 and 11) should it be important to consider these factors individually. Perhaps the authors would care to present this more detailed data analysis in a response to this letter.

We hope the authors see this letter in the context of science and can see the value in answering these questions. The use of RE programs within corporate wellness is an exciting research area but the details can help both future research design and practical application.

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