Author's response to: *Pre-Exhaustion exercise and neuromuscular adaptations: an inefficient method?*

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Dear Editor, we appreciate being able to respond to the discussion raised by Prestes, et al. in their letter to the editor *“Pre-Exhaustion exercise and neuromuscular adaptations: an inefficient method?”* We thank the authors for their commentary and agree that raising discussion of methodological issues is a step towards resolution. However, we believe the Prestes, et al. may have misinterpreted and misrepresented our study. The primary themes within the authors’ letter appear to dispute volume of resistance training (single- vs multiple- sets) and hypertrophic adaptations. For clarity; our original paper (Fisher, et al., 2014) neither compared nor measured either of these variables. However, in the interests of open dialogue we feel readers might benefit from our responding to the letter to clarify any misinterpretations.

A concern regarding the comments by Prestes, et al. arises in their statement that the disparity between Jones’ (1970) original hypothesis and our results might be explained by a *“lack of gold standard”* methods to measure muscle hypertrophy. We reiterate that we did not measure, and made no claims regarding hypertrophy within our study (Fisher, et al., 2014). In addition the rejection of empirical data in favour of a pre-conceived hypothesis without identifying genuine methodological issues which might limit the extent to which said data can be respected appears demonstrative of considerable bias.

Prestes, et al. continue stating *“for trained subjects, it is important to note that resistance training volume can increase magnitude of muscle strength improvements”.* To support this statement Prestes, et al. cite a meta-analysis relating to muscular hypertrophy *not* strength (Krieger, 2010). Interestingly this meta-analysis (Krieger, 2010) has been critiqued in detail for a lack of control over the numerous variables disparate between the studies included within said meta-analysis (Fisher, 2012). In the interests of clarity; Krieger did in fact publish a meta-regression comparing single and multiple sets for strength (Krieger, 2009), which was later critiqued by Carpinelli (2012). Furthermore, we should be cautious to validate a belief citing only meta-analyses, which in their very process have considerable limitations (Shapiro, 1994; Egger & Smith, 1997), without consideration of the studies included. In addition, since the publication of these meta-analyses, further empirical research has examined set volume within RT for both trained and untrained persons with some support for multiple set approaches (Marshall et al., 2011; Radaelli et al., 2014[b]), and yet the majority of studies finding no differences between single and multiple set routines (Radaelli et al., 2014[c]; 2013[a]; 2013[b]; Kadir et al., 2014; Adnan et al., 2014; Correa et al., 2014; Baker et al., 2013; Steele et al., 2015). However, irrespective of this, we urge Prestes, et al. to re-read our paper which in no way compared single and multiple set training but rather considers the use of PreEx training and exercise order for equated volume groups; further evidence that our paper (Fisher, et al., 2014) may have been misinterpreted and/or misunderstood by the authors of the letter. Prestes et al. may argue that our results might have differed with greater set volumes and indeed this may be true. However, in the absence of evidence to support that PreEx produces greater strength gains with multiple sets this remains speculative. It should be noted that, in the absence of evidence regarding a particular training approach (i.e. PreEx) the most logical direction for research is to begin with a simple intervention which is the approach we took.

The authors then discuss PreEx training and cite multiple studies considering acute muscle activation measured by electromyography (EMG), and indeed we thank Prestes, et al. for bringing to our attention the study by Júnior, et al., (2010). However, as previously stated (Fisher, et al. 2011) the use of acute EMG at best only infers hypotheses regarding training adaptations or provides evidence regarding the potential role of motor unit recruitment to adaptations evidenced from training intervention study. In fact the only scientific method to measure a chronic response is with a controlled intervention study, such as our PreEx article. Indeed Prestes et al. note themselves that *"…muscle strength can increase even without a significant increase in muscle electromyographic activity…"*. It would appear Prestes et al. are suggesting that PreEx may produce greater adaptatinon when not performed to momentary muscular failure (MMF) and we do concede that there may indeed be some benefit of performing PreEx typein this context. As Prestes et al. note, it has been proposed that PreEx may allow for greater fatigue related stimuli to be induced and it seems likely that when not training to MMF the use of PreEx would enhance responses related to metabolic stress as well as motor unit activation. The results of Junior et al. (2010) might allow us to hypothesis this and this, along with the potential for PreEx to manifest in greater adaptations when applied using multiple sets, remain future avenues for research regarding this technique. Our study though suggests when training to MMF using single sets per exercise PreEx offers no further benefits (Fisher et al., 2014).

Prestes et al. proceed to discuss muscle hypertrophy mediated through mechanisms of metabolic stress which, unfortunately, is not directly relevant to our study since we didn’t measure, discuss or even infer hypertrophic adaptation or measures of metabolic stress in conjunction with PreEx training. However, we agree that studies which are indeed designed to investigate hypertrophy should utilise adequate outcome measures such as ultrasound or magnetic resonance imaging and that perhaps future research should investigate PreEx using these outcomes.

Prestes, et al. do highlight limitations of our study which are worthy of discussion. They suggest that greater details of previous training experience would have added to the quality of the paper and might help explain our results. We agree; research articles considering trained participants should make greater effort to detail previous experiences and background and thus highlight differences between pre-existing training routines and those performed for research purposes. We failed to provide this information in the original study and here clarify that all participants previous resistance training experience included employing a single-set to muscular failure, full body routine ~2 x / week (similar to the intervention protocol for the CON group) for at least 6 months at the facility were the study was conducted.

Prestes, et al. also comment that we failed to report on high- and low-responders within our study, which is accurate. However, we draw attention to the fact that the change data for all groups within our study was in fact normally distributed and that the 95% confidence intervals shown in Figure 2 (Fisher, et al., 2014) relating to absolute strength changes for the respective exercises and intervention groups are similar between groups. This would indicate that the range over which the true population mean might exist was similar for all groups suggesting a roughly similar spread of data.

We further agree that we failed to cite test-retest reliability for our measurement of muscular strength. However, the use of repetitions to MMF with sub-maximal loads is well evidenced as appropriate and reliable and that this has a relatively fixed relationship to 1RM addressing the comment regarding reporting 1RM also as unnecessary (Carpinelli, 2011). The fixed order of the strength test however, which was standardised pre- to post-intervention for all groups, should be consider a scientifically rigorous protocol which avoids the potential for uncontrolled confounding factors from a variable order impacting the outcomes.

The participants were also not University students; however we are intrigued as to whether this is recognised to impact muscular adaptations from resistance exercise? Indeed participants are stated as members of a fitness facility in the methods. Also, and as further clarification, participants were not involved in any other structured exercise programmes throughout the intervention.

Finally Prestes et al. suggest that we could have used effect sizes as proposed by Rhea (2004) for trained participants. Indeed though we did not interpret our results in light of Rhea's proposed scale for determining magnitude of effect sizes this can easily be done by anyone reading our paper as we used Cohens *d* (Cohen, 1992) to calculate our effect sizes. Using Rhea's scale and classifications our results would still be considered as ranging moderate to large (1.15 to 1.89) with no clear indication that one group over another obtained consistently larger effect sizes.

Prestes, et al. further suggest that *"…it is very difficult to suppose that trained subjects will perform a single set resistance training limited to pec-fly, chest press, leg extension, leg press, pull-over and pull-down…"* and note the ACSM (Ratamess, et al. 2009) recommendations to include *"split body multiple-set routines, usually used by advanced trainers and bodybuilders"*. However, we are unclear as to the exact point raised by this comment. Prestes et al. are perhaps correct if they are suggesting that most advanced trainees do not follow the training program utilised in our study. However, this only speaks to the relative popularity of the approach in comparison to the more popular recommendations of the ACSM and not to the empirical evidence supporting it. Irrespective, further investigation of the ACSM article and its citation for this statement reveals that these specific recommendations are based on observations rather than empirical evidence (Häkkinen, et al., 1988). We urge caution to Prestes, et al. in their use of secondary citations and anecdote. Without reading the original research there is substantial possibility of misinterpretation, misrepresentation and in this case inappropriate referencing. It is perhaps noteworthy that the ACSM resistance training recommendations (Ratamess, et al. 2009) received considerable criticism for the same publishing misconducts (Carpinelli, 2009).

We thank the editors for the opportunity to respond to this letter, and we hope that our response has provided some clarification regarding the points raised by Prestes et al. and in areas where they may have misinterpreted our study. We agree that open debate of methodological issues can only serve to enhance understanding and future research, however we reiterate earlier comments that authors and researchers should be careful not to misinterpret and/or misrepresent their own or others' research articles and/or data.

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