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Letter to editor

Contralateral Effects after Unilateral Strength Training: A Meta-Analysis Comparing Training Loads

Dear Editor-in-chief

I read with interest the recent publication considering cross-education by Cirer-Sastre et al. (2017). The authors should be commended for attempting to review this area, however, a number of concerns arose in reading through the manuscript that warrant discussion and clarification to avoid misrepresentation.

The use of a meta-analysis to review this body of literature raises methodological concerns. Meta-analyses have increased in popularity due to the relative time efficiency of collecting and analysing accessible data to produce a comprehensive review, rather than the recruitment and provision of an exercise-based intervention for participants, as well as the efforts required for participant retention. Meta-analyses are a tool for combining data from homogenous empirical studies which might not have produced statistically significant differences as a result of a type-2 error. Combining studies in this way serves to consider the data across a larger population. However, as stated by Haidich (2010); *"Meta-analyses should be conducted when a group of studies is sufficiently homogenous in terms of subjects involved, interventions, and outcomes to provide a meaningful summary"* and continues *"Combining studies that differ substantially in design and other factors can yield a meaningless result"*. In this sense, and since resistance training studies notoriously control and assess differing variables from participant demographic and training history through to methodological intricacies of the intervention employed, there exist very few homogenous studies that could be included in a robust meta-analysis considering cross-education. Furthermore, as the following discussion will highlight, the detail within the heterogeneity of the studies included might have made a narrative review of this area more worthwhile.

The authors report in their abstract and discussion that *fast-eccentric* exercise produces comparatively favourable adaptations in cross-education. However, of the 8 studies included, the only study to use eccentric only exercise was that of Lepley and Palmieri-Smith (2014). The data certainly show that large effect sizes (d = 1.28 and 1.35) are attained for contralateral eccentric strength by the use of eccentric training. However, considering the Lepley and Palmieri-Smith (2014) study in greater detail highlights that the participants were trained using an isokinetic dynamometer. It's important to appreciate that, when using isokinetic training, an eccentric muscle action might actually be better thought of as an intended concentric action. The computer controlled lever arm of the dynamometer is forcefully being lowered causing an eccentric muscle action, however the limb is maximally resisting this downward movement (Blazevich et al., 2007). As such with increased velocity there is increased effort to resist the downward movement.

The use of eccentric isokinetic training should also be considered in relation to the authors' (Cirer-Sastre et al. 2017) comments regarding effort. In their discussion, Cirer-Sastre et al. state that *"studies conducted on programs involving the most strenuous training - organized into single sets to fatigue or muscle failure - were those that produced the lowest contralateral strength increases"* (page 184). Their discussion of the training interventions (page 182) states that it was in the Munn et al. (2005) and Shields et al. (1999) studies in which participants performed *fatigue protocols*, whilst the other studies were organized based on sets and repetitions (the authors report mean values of 4.4 ± 1.7 and 10 ± 3.5, respectively). In fact, multiple studies within the article required participants to perform maximal effort exercise based on either repetitions to momentary failure or maximal force contractions. For example, the aforementioned study by Lepley and Palmieri-Smith (2014) used a training protocol where *"participants performed 4 sets of 10 maximal eccentric isokinetic actions"*. Since this study also produced some of the largest effect sizes (see above) it is incorrect to suggest that the most strenuous training produced the lowest contralateral strength increases. Indeed, Moore et al., (2012) suggested that, due to the nature of eccentric isokinetic training (e.g. resisting a load by attempting to perform a concentric muscle action) there is a greater muscular force than concentric training. Furthermore, the included study by Farthing et al., (2005) which produced the largest contralateral effect size (d = 2.09) also included maximal repetitions; the authors report *"Strength training was 6 wk of maximal isometric ulnar deviation"* (Farthing et al., 2005; page 1594). Finally, the included studies by Shaver (1970; 1975) used a protocol whereby participants performed 3 sets of 10 repetitions at 50%, 75% and 100% of their 10-repetition maximum (RM) progressively. In this sense, the final set of each exercise session was performed to maximal effort (e.g. 10 repetitions using their 10RM).

The authors' of the meta-analysis (Cirer-Sastre et al., 2017) suggest that larger volumes of training (E.g. multiple sets compared to single sets) produced greater contralateral increases in strength. And further, use this data to provide recommendations on cross-education exercise protocols. Since the mechanisms are largely dependent upon neural practice of a movement rather than comprehensive recruitment of muscle fibres, this seems logical. However, the Shields et al., (1999) article used

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what might be considered the largest volume of training of all the studies included; 1 second concentric: 1 second eccentric contractions using either 30% or 0.005% of maximal voluntary isometric contraction (MVIC) to fatigue or for a maximum of 2 hours. The authors reported an average of 22 minutes training time, which equates to an average of 660 repetitions. Shields et al. report *"The subjects received a 5-minute rest at the conclusion of each day's first training bout, followed by a second training bout."* This means that following an average of 660 repetitions/22 minutes of exercise the participants performed a second set! The authors continue *"The second bout was always considerably shorter than the first bout due to the fatigue developed during the first test. The subjects repeated the training sessions 5 days per week for a total of 6 consecutive weeks."* This is evidently a high volume and frequency of training, and yet the effect sizes for contralateral strength increases were some of the smallest included in the meta-analysis (d = 0.6 and d = 0.05 for 30% and 0.005% MVIC, respectively). The authors (Cirer-Sastre et al., 2017) present this article as evidence that fatigue protocols produced some of the lowest contralateral strength increases. However, they fail to mention this article in respect to the supposed efficacy of larger training volumes. Exposure of the methodological details of these studies shows that we cannot be so conclusive as to the role which volume and effort play in cross-education strength adaptations.

In summary, authors should be cautious of the use of meta-analyses both in research and application. As highlighted herein, numerous methodological inconsistencies are present and demonstrate the heterogeneity within the studies included in this meta-analysis. For a meta-analysis to be valid there needs to be a large number of homogenous studies, something which appears to be lacking within resistance training and notably, within the specialised area of cross-education. A more qualitative review of the literature might have better discussed the methodological details as well as highlighting areas for replication studies and progressive research, rather than attempt to draw general conclusions from considering only the numerical values. Furthermore a practical understanding and explanation of the training and testing methods is essential to ensure that studies are fairly represented.

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Authors' response

We appreciate the comments received in a letter to the editor regarding our recent manuscript (Cirer-Sastre et al., 2017) and thank the Journal for giving us an opportunity to participate in this discussion.

First, the authors introduced the letter stating that meta-analyses have increased in popularity due to their time efficiency. In this regard, we must emphasize that a meta-analysis should be understood as a statistical method, rather than a time-efficient strategy to be published. Readers must note that a meta-analysis is not efficient, neither in time nor effort, although it might require, indeed, less resources than the original clinical trials. Our colleagues were concerned about the typical lack of homogeneity across resistance training studies, which generally differ in their intervention characteristics. They refer to Haidich (2010) when she states: *"Combining studies that differ substantially in design and other factors can yield a meaningless result"*. However, the authors missed the second and important clause when paraphrasing that sentence. In the original text, that sentence continues: *"(…), but the evaluation of reasons for heterogeneity among studies can be very insightful"* (Haidich, 2010, p. 33). Indeed, to estimate a single, accurate, and robust effect size, studies need to be homogeneous in terms of participants, intervention, comparison, outcome, and study design. Probably for this reason, the PRISMA statement for systematic reviews and meta-analyses encourages authors to describe inclusion criteria by mentioning such characteristics, under the well-known acronym of PICOS (Moher et al., 2009). However, a moderator analysis proceeds only when the main effect size across studies results with statistically significant heterogeneity. This means that, a certain degree of heterogeneity is needed to investigate the sources that could be associated with that variability (Pigott, 2012). Moreover, even the cited font (Haidich, 2010, p. 32) provides an approach to control such discrepancies by using random-effects models, which was the method described in our manuscript (Cirer-Sastre et al., 2017, p. 182).

Second, our colleagues pointed out the scarcity of included studies which trained solely eccentric contractions. This is true and we share this concern. More original research is needed to achieve more powerful comparisons. In the same paragraph, the authors also were concerned about whether the eccentric contractions would be *"better thought of as an intended concentric action"* when performing exercises in isokinetic dynamometers. Although both, participant's intention and tendon forces, are concentric in all contraction types, such a statement could mislead readers and deserves clarification. We may need to consider the distinction between intentionality and mechanics since eccentric contractions (either in isokinetic, constant load, constant resistance or accommodating resistance exercises) remain characterized by the mechanical behavior when an external load, resisting the action, is greater than the internal force, generated by the muscle, rather than participants' intentionality (Hessel et al., 2017). Curiously, the authors referred to Blazevich et al. (2007, p. 1566), who defined an eccentric contraction of the knee extensors as a mechanical-induced flexion regardless the intention of extending the joint.

Third, our colleagues suggested that in eccentric isokinetic exercises, *"with increased velocity there is increased effort (…)"*. This statement evokes the following reflection: On the one hand, the force-velocity relationship is negative, so the higher force outputs are observed in the fast-eccentric contractions (Lieber, 2002). This implies that the same absolute load might represent less relative intensity in eccentric actions than in concentric. On the other hand, effort and exertion are constructs far more complex to measure than intensity (Peñailillo et al., 2017). In this sense, the relationship stated by the authors, remains currently controversial since very few studies have investigated the association between velocity and exertion, and indeed there exists research reporting higher RPE values in the concentric exercises (Hollander et al., 2003).

Fourth, the authors highlighted some misleading terms we used in the meta-analysis that we agree must be clarified. We divided the sample of studies depending on whether participants were prescribed to perform an undefined number of repetitions up to failure (RF) or, conversely, were prescribed to perform a defined load of sets and repetitions, without exercising until failure (SR). In our meta-analysis discussion (Cirer-Sastre et al., 2017, p. 184), we were imprecise referring to RF as *"the programs involving the most strenuous training"*, and we agree with our colleagues that this definition should be clarified. We considered RF studies when the resistance interventions were organized in sets to failure, regardless the level of effort given to each repetition. This implies that contractions at maximum effort (for example those required in Lepley and Palmieri-Smith, 2014), could be not considered RF, since participants might not necessarily exercise until failure. The authors mention the study of Farthing et al. (2005). In this study, participants performed maximal isometric ulnar deviations (2 sec. contraction, 2 sec. relaxation) organized into 2- 6 sets of 8 repetitions. According to our distinction, this program was considered as an SR program since participants performed a defined volume of sets and repetitions, without achieving failure. Finally, the authors cited Shaver (1975), whose participants trained 3 sets of 10 repetitions of arm curls at 50%, 75% and 100% of 10-RM. In this study, the calculated level of effort in each session corresponds to 75% of 10-RM [(1x10x50% + 1x10x75% + 1x10x100%)/30], and it was considered SR because training volume was predefined and participants were not required to exercise until failure in all sets.

A final consideration refers to when we mentioned that RF referred to *"single sets to fatigue or muscle failure"* (Cirer-Sastre et al., 2017, p. 184). We agree with the authors that our original statement was incorrect since, as clarified above, a RF organization could be structured with multiple sets rather than single sets. Furthermore, due to this inaccuracy, readers might confound our distinction between training organizations with the training volume in each intervention, which do not necessarily match. Indeed, our meta-analysis detected differences between training organizations, whereas it did not find any conclusive correlations with training volume. In this regard, we agree with our colleagues that our exposure of the methodological details should have not been so conclusive, specifically considering that volume was calculated from the total amount of repetitions, and not from the total working time.

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