We would like to thank Professor Knudson for his interest in our article. Four main points of concern were raised in his letter, each of which we have addressed below.

With reference to the first point regarding the choice of measured variables, of course we agree that there are direct associations between mechanical variables and performance within an activity. However, the aim of this study was to establish how well force-velocity (F-V) variables obtained from one extremely common physical training and testing activity (the vertical jump) (Petrigna et al., 2019) related to performance in a separate activity which is fundamental to performance in many sports (sprint acceleration) (Simperingham et al., 2016). The motivation was to provide information which might allow athletes to tailor their F-V profiles in vertical jumping via resistance training (Jiménez-Reyes et al., 2017), with a view to improving their sprint performance.

Regarding the second and third points relating to the use of regression and extrapolation to derive the linear F-V profiles. Whilst we appreciate the concerns raised, the methods used have been employed extensively in peer-reviewed work over many years (see Jaric, 2015 for review), so it is perhaps surprising that Professor Knudson chose this article as a focus for expressing these concerns. As previously stated, the aim of our study was to use these well-established and reliable methods (Jaric, 2015) to inform strength and conditioning training, not to assess the validity of the linear F-V model. Nevertheless, the comments regarding the extrapolation of the data in Figure 2 misinterpret the results. An apparent discrepancy was highlighted between the point where the regression line intersects the horizontal axis (~1.35 $m.s^{-1}$) - which represents V_0 - in Figure 2, and the V_{max} value of 8.3 $m.s^{-1}$ reported in Table 1. This discrepancy is unsurprising, since V_0 was the mean vertical velocity during the jump takeoff, and V_{max} was the measured horizontal sprint velocity. Additionally, it was asserted

that an extrapolated mean vertical ground force of ~7.5 $N.kg^{-1}$ during an unweighted jump takeoff was unreasonably low since, 'a person in this unweighted condition could still likely create a dynamic lower extremity force of 9.8 N/kg'. But no mechanical rationale was provided for this assertion, and it is not clear what would govern this apparent lower limit.

Lastly, it was proposed that the relationship between sample size and the number of correlations performed 'posed a serious risk of statistical errors'. Multiple comparisons carry with them an increased risk of type-1 errors; however the experiment-wise error rate is a function of the number of comparisons and the alpha level, and therefore is independent of sample size (Ryan, 1959). It was acknowledged in the article that the sample size was a limitation of the study, and that it carried with it the risk of type-2 errors, and confidence intervals and p-values were reported for each comparison to allow readers to draw their own inferences from the results of the correlations.

In summary, although we sympathise with some of the concerns raised by Professor Knudson, we also believe others to be unfounded, and that none undermines the study conclusions.

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