Reviewer’s report

Title: Do knee abduction kinematics and kinetics predict future anterior cruciate ligament injury risk? A systematic review and meta-analysis of prospective studies

Version: 0 Date: 27 Jan 2020

Reviewer: Nathan Schilaty

Reviewer’s report:

In this manuscript, the authors performed a systematic review and meta-analysis of knee abduction kinematics and kinetics of ACL injury risk. The manuscript is well written and was a pleasure to read and critique. I thank the authors for their diligent efforts on this work and feel that it will contribute well to the literature pending revision. Although this manuscript is timely and an important step in the progress of research in this arena, my overall evaluation is that 1) the current title is misleading, 2) the current scope of the literature causes this study to be underpowered, and 3) the conclusions are too strong for what the data demonstrates.

I hope that the authors will consider the following revisions for improvement of this manuscript and to support current research prerogatives:

Minor:
*Page 5, Line 91 - Alignment has a misplaced capital

*Page 7, Line 126 - Be sure to remain consistent in terminology throughout the manuscript. 'Sex' was allocated, not gender. Sex is binomial and determined by genetics - gender is self-selected and is a large spectrum. In Line 129, the term 'sex' is appropriately utilized. Need to change on line 205.

*Page 7, Line 143 - Change to, "Of those, 10 articles…"

Major:

*The title needs to include 'risk' as only injury risk can be predicted… not actual injury.

*Line 236 - it needs to be made clear that a non-linear relationship may exist. It is alluded to with mention of a "linear relationship," but it could be more clearly stated.

*Although it may be construed as promoting my own work, I think that the authors need to consider recent cadaveric publications that are highly controlled, randomized loading laboratory studies that clearly demonstrate knee abduction as a major contributing factor to ACL strain and rupture. Thus, there is not just clinical evidence to support the concept of knee abduction (as is the goal of this meta-analysis), but there is also basic and translational science that support knee abduction as a key contributor to ACL injury. ACL injury prediction is difficult as it is likely non-
linear and will likely be multifactorial, but we cannot discount the high degree of evidence that continues to support that knee abduction angle and moment are key in loading the ACL, especially in a meta-analysis that will drive future research and funding. As a reviewer on this study, I am passionate about these topics and need to speak to the research that has been performed here. These articles could potentially be discussed (at the discretion of the authors) near line 257 and further in the Discussion near line 301. Also, this information should be considered when addressing the conclusion of 312-314.


*Table 1 - Smeets et al. 2019 Quality Score is incorrect. It should be 11/19.

*It is not clear if the Forest plot was adequately weighted based on the numerical values of the studies. This should be made clear.

*I have concerns for the Goerger et al. study data as presented in the manuscript (Forest plot). It really is the only study that is an outlier of the other 3D studies that trend in favor of 'increased knee abduction' as a ACL injury risk predictor. This is the study that disagrees with all of the other studies on the Forest Plot. However, Figure 3D of this study and the conclusions counteract what has been summarized by the authors of this current manuscript. In fact, Figure 3D demonstrates a baseline IC abduction angle of 11 +/- 4 deg. I would advise the authors to revisit their data extraction and ensure that it is accurate in their analyses.

*Supplemental Data (mentioned with Funnel plots in the abstract): The Funnel plots only have 3-4 data points and clearly are not adequately powered to determine publication bias. However, observation of the Forest plot from the current manuscript demonstrates that knee abduction is clearly factor in ACL injury risk prediction. This needs to be specifically mentioned in the limitations of the study as it is currently overlooked with being supplemental material.
I think that there needs to be caution with the term 'normal range.' This needs to be carefully defined as there is a spectrum of knee motion that could be considered 'normal', yet there can be high risk in the 'normal' motion. For example, people present with disease even when their biometrics are in the 'normal' range. Usually, there are multifactorial presentations that amount to the disease / risk even though one could be within 'normal' range.

In the opinion of the reviewer, the conclusion (in abstract and text) is currently too strong for the data presented. Although this current publication is warranted, the conclusion and title are quite misleading to the current amount of data present in the literature. There is a definitive clinically relevant trend (Figure 1 - Forest plot), although there is currently not statistical significance. The lack of statistical significance may not show as the limited literature causes the study to be 'underpowered,' but the trends are clearly favoring that knee abduction does predict ACL injury risk based on the Forest plot provided.

Are the methods appropriate and well described?
If not, please specify what is required in your comments to the authors.

Yes

Does the work include the necessary controls?
If not, please specify which controls are required in your comments to the authors.

Yes

Are the conclusions drawn adequately supported by the data shown?
If not, please explain in your comments to the authors.

No

Are you able to assess any statistics in the manuscript or would you recommend an additional statistical review?
If an additional statistical review is recommended, please specify what aspects require further assessment in your comments to the editors.

I recommend additional statistical review

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Please indicate the quality of language in the manuscript:

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