Letter to the editor

Response to Comment On: “Injury Profile in Women’s Football: A Systematic Review and Meta-Analysis”

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1 INTRODUCTION

First, we would like to thank the editor of *Sports Medicine* for giving us the opportunity to respond to the comments made by Mayhew et al. in their letter to the editor [1] with regard to our published paper titled “Injury Profile in Women’s Football: A Systematic Review and Meta-Analysis” [2]. As researchers, we welcome the fact that all our work is subjected to criticism. This type of interaction/discussion among researchers strengthens the sports medicine field of knowledge and provides transparency and credibility to the findings reported in published studies.

For the sake of clarity, the comments made by Mayhew et al. in their letter to the editor [1] have been organised into five different blocks or categories: (1) injury incidence outcomes in primary epidemiological studies; (2) main outcome of the meta-analysis (i.e., time loss injury incidence rates); (3) Fig. 5 design; (4) approach followed to make inferences; and (5) degree of heterogeneity of the pooled injury incidences. Below, our category per category responses to the comments made by Mayhew et al. can be found.

1.1 A) Injury Incidence Outcomes in Primary Epidemiological Studies

First, we would like to thank Mayhew et al. [1] for informing us regarding the typographical error presented in the example displayed in the article to describe how to use Eq. 2 (i.e., match incidence = no. of injuries/[no. of matches x 11 players x match duration] x 1000), which was suggested in previous studies to calculate match injury incidence when the hours of exposure (unlike the number of matches) were not provided in primary epidemiological studies [3, 4]. Undeniably, this was a typographical error that was not identified, and the true match incidence value of the example is the one documented in the commentary by Mayhew et al. [1].

Equation 2 was not applied to estimate soccer match injury incidence in either of the 22 studies that were finally included in the meta-analysis. Consequently, Eq. 2 was not used in any of the six studies that Mayhew et al. [1] pointed out in their letter to the editor because the data provided in those papers permitted the use of Eq. 1 to describe all epidemiological outcomes. Thus, match injury incidence was not estimated/imputed 'at a gross level' through the use of Eq. 2 in any primary study.

With regard to Eq. 1 (injury incidence = 1000 x [Σinjuries/Σexposure hours]), this is widely used by the scientific community to estimate injury incidence. Furthermore, it has been considered by consensus statements on epidemiology of injuries in team sports (including the one mentioned in the letter to the editor) as a valid equation to estimate injury incidence [5, 6]. Therefore, we are very confident that Mayhew et al. [1] have no objections to its use.

Given the above, it is difficult to understand why Mayhew et al. [1] have suggested that "*we did not accurately estimate injury incidences*", (thus questioning the precision/rigor of the results reported in our meta-analysis) without knowing whether or not we had used Eq. 2 to estimate match injury incidence. We hope this now provides clarity to the approach we have used, which is in line with consensus statements.

1.2 B) Main Outcome of the Meta-Analysis (i.e., Time Loss Injury Incidence Rates)

Medical attention (i.e., referring to "an assessment of a player's medical condition by a qualified medical practitioner" [5]) and urogenital/gynecological female/women illnesses have been poorly documented in primary studies on the epidemiology of injuries in women's soccer. Thus, the very limited number of studies that accurately report these data in women's soccer led us to exclusively focus the main outcome of this meta-analysis on time loss (soccer-related) injuries (see the first inclusion criterion). Therefore, medical attention and urogenital/gynecological illness symptom clusters were excluded from this meta-analysis. A time loss injury is a soccer-related injury that results in a player being unable to take a full part in future soccer training or match play. The fact that we had exclusively analysed time loss injury incidence rates rigorously following the definitions of injury described by Fuller et al. [5] does not bias the findings in relation to either the quality or the usefulness of the injury profile described in this meta-analysis for women soccer players. We clearly stated this in the study selection subheading: "*Injury must be defined in terms of time loss (i.e., injury that results in a player being unable to take a full part in future football training or match play)*".

We were surprised to read that Mayhew et al. [1] were critical of the fact that injury burden was not provided in our meta-analysis, as we outlined that this could not be calculated due to the fact that there was insufficient information available in the literature to build a risk matrix (only three studies provided some data in this regard [7–9]). We highlight this limitation of the available data in numerous sections of the text (Discussion, Limitations, and Future Directions):

"*As previous studies exploring the location and type of football-related injuries have only reported incidence rates and not the average number of days lost from football (time loss), it was not possible for us to calculate the injury burden (the cross-product of severity [consequences] and incidence [likelihood]) to build a risk matrix. The risk matrix would have helped to identify the importance (i.e., burden) of each football-related injury and may provide information to help prioritize injury prevention measures used in applied football environments*."

In the text, and regarding the injury burden, only the following is mentioned:

"*Based on the findings shown in previous studies [17, 62, 99, 116], the most burdensome injuries in women's football may be quadriceps and hamstring muscle injuries and knee (ACL mainly) and ankle ligament injuries. For example, Larruskain et al. [17] reported that, in an elite football team, 43% and*

10% of the days that female players were away from playing football were due to ACL and quadriceps muscle injuries, respectively."

Consequently, this information should not be considered either irrelevant or inaccurate.

1.3 C) Fig. 5 Design

With regard to the comments on Fig. 5, we acknowledge the presence of a typographical error. In the legend of this figure, it is stated that the confidence intervals (CIs) of each estimate are shown. However, these data were removed from the figure to avoid repetition as the CIs of all estimate incidences are reported in the text, as highlighted by Mayhew et al. [1]. However, we do not think that not showing the Cis in Fig. 5 is a fatal flaw as the purpose of the figures was to show the main findings of the respective articles in an attractive way to catch the reader's attention. In the figure legend, we do not mention the presence of differences among injury incidences by location; therefore, all key epidemiological data obtained from this meta-analysis are provided in either the text or figures.

1.4 D) Approach Followed to Make Inferences

First, we would like to remind the reader that the main purpose of this study was to perform a systematic review and meta-analysis of epidemiological data of injuries in women's football; thus, all the conclusions of this study are based on the rigorous methodology used. In order to contextualize the results found in this study (similar to that which has been conducted in previous meta-analyses [10–13]), in the Discussion section some 'indirect comparisons' were carried out between the results of our study for female soccer players and the findings reported mainly in a meta-analysis on the epidemiology of injuries in professional soccer players in which a very similar methodology was applied.

We agree with Mayhew et al. [1] that the optimal approach to compare the pooled injury incidence between male and female soccer players would have been through the selection of primary studies where direct sex-related comparisons of this variable have been carried out. However, due to the very limited number of studies available, we could not conduct a specific subanalysis in which papers reported sex-related comparisons of pooled injury incidence. Alternatively, Mayhew et al. [1] consider that these indirect comparisons between sex should have been made through the use of the CIs of the injury incidence estimates reported in our meta-analysis [2] and in the study by Lopez-Valenciano et al. [10]. In particular, these authors consider that if the CIs of the pooled estimates are overlapped, then the differences between their mean scores should not be graded as statistically significant. However, this approach may be suboptimal in this context in order to make indirect sex-related comparisons, as the variables do not show a normal distribution and there is also a high imbalance (in favor of the professional male soccer players) with regard to the number of players (20,493 vs. 7733), total injuries (29,991 vs. 4161), and hours of exposure (4,429,568 vs. 892,534) included in both meta-analyses (for more information about this issue please read the article by Poole [14]). In addition, some authors suggest that this frequentist approach could be too strict in certain settings to make inferences, as it may not be sensitive enough to identify small (statistically not significant) but clinically meaningful differences [15, 16].

Therefore, considering all of the above (and also with the support and expertise of the reviewers of the article), we made the decision to base our sex-related comparisons on what might be defined as a clinically relevant approach according to expert knowledge. Of course, this approach is not without limitations, but it may be more informative to put into context the findings of this meta-analysis than the approach based on CIs. Nevertheless, if the approach proposed to make indirect sex-related comparisons by Mayhew et al. had been employed, the conclusions would not significantly differ since the overlap of the CI would be 'very slight', allowing us to conclude that at least there was a strong tendency towards significance.

Based on this, we strongly think that Mayhew et al. have been too hasty in stating that "*we wish to caution inferences supported from single-point estimates (e.g., in Fig. 5) rather than CIs in pooled*

injury incidence statistics. It may be prudent for practitioners, researchers, policy makers and funders to hesitate before using injury incidence data generated from meta-analyses to support "introducing and evaluating preventative measures that target the most common diagnoses".

1.5 E) Degree of Heterogeneity of the Pooled Injury Incidence

In their letter to the editor, Mayhew et al. [1] suggest that the high I^2 values reported in our meta-analysis "*reflect substantial heterogeneity and reduce confidence in statements of single-point estimates (and CIs) even further. When high I^2 values arise, the Cochrane collaboration recommends that in some circumstances it is not advisable to pool data using meta-analysis because of the likelihood of misleading results, and if data are pooled for analysis then sources of heterogeneity need to be carefully explored and reported".*

We agree with Mayhew et al. [1] on the fact that the degree of heterogeneity of our pooled injury incidences were very high. High I^2 scores have also been documented in most of the previously published meta-analyses on the epidemiology of injury in different sports [10–12, 17]. Although we are aware that the high heterogeneity is currently an inherent limitation in the epidemiology of sport-related injury, we strongly think that the findings from these meta-analyses can help practitioners to better understand the injury profile of the sports explored. It is perhaps not surprising that Mayhew et al. [1] support this statement since they have registered in PROSPERO (code: CRD42019130407) a meta-analysis on the epidemiology of injuries in professional women soccer players using a very similar methodology to that used in our study.

Consequently, we may assume that Mayhew et al. [1] considered that we have not carefully explored and reported potential sources of heterogeneity. We are not in agreement with that suggestion because, as stated in the meta-analysis (statistical analysis subheading), *the possible influence of the following variables on the model outcomes (including the statistic I^2) was analyzed independently through univariate and multivariate analyses: registration period, year of the study publication, age of the players, STROBE score, NOS stars, Downs & Black and number of teams included in the study*. The results of these analyses reported that none of these variables had an impact on model outcomes. Furthermore, in the limitation sections of our meta-analysis, this high heterogeneity is clearly mentioned as a limitation and some potential sources of inconsistences that were not explored in here were suggested.

"Nevertheless, even when our inclusion criteria for subanalysis and standardized formulas were applied, the degree of inconsistency of the results across studies was still very high ($I^2 > 90\%$). Similar to men's football, other potential sources of inconsistency may have been the differences existing among the national leagues in terms of climatic regions (cooler and warmer areas) [126], periods of fixed match congestion [79, 127], numbers of matches and in-season breaks [128], as well as the level of professionalism [8, 129]."

2 CONCLUSIONS

Considering the aforementioned in each of the categories, we strongly believe that both the methodology followed and the results provided in our meta-analysis comply with the quality standards and provide a precise and rigorous fixed picture of the epidemiology of injuries in women's soccer.

Declarations

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Conflicts of interest/competing interests Alejandro Lopez-Valenciano, Javier Raya-Gonzalez, Jose Alberto Garcia-Gomez, Alba Aparicio-Sarmiento, Pilar Sainz de Baranda, Mark De Ste Croix, and Francisco Ayala declare they have no conflicts of interest relevant to the contents of this response.

Availability of data and materials Not applicable.

Code availability Not applicable.

Author contributions All authors contributed to the conception and design of this response to the letter to the editor by Mayhew et al. [1]. The first draft of the response was written by FA and all authors commented on subsequent versions. All authors read and approved the final response.

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