Author's response to reviews

Title: The 2016 HIGh Heels Health effects and psychosexual benefits (HIGH HABITS) study: systematic review of reviews and additional primary studies

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Author’s response to reviews:

Dear Editor,

Many thanks to you and the reviewers for your consideration of our work and the invitation to submit a revised version of our manuscript for publication. We have provided this with tracked changes to show how we have responded to reviewer comments. For clarity of response, we have also grouped comments by theme below and detailed our replies (providing continuous line numbers for reference).

TYPOGRAPHICAL ISSUES

Point 1.
Reviewer 2 (Neil Cronin): Page 9, line 49: Should be '…is shown in…'
Response: We have made this correction as requested (line 269)

Point 2.
Reviewer 2: p10, l46: Inconclusive instead of inclusive
Reviewer 3 (Cylie Williams): Page 10 ln 46 - Russel found inclusive results...should this be conclusive?
Response: We have corrected this to ‘inconclusive’ (line 293)

TERMINOLOGY ISSUES
Reviewer 1 (Nachiappan Chockalingam): Within the methods section you make a reference to a study (High heels). What's this study and was it published or is this paper (which is under review) is one of the outputs? You need to make this clear.

Response: “The 2016 HIGh Heels: Health effects And psychosexual Benefits (HIGH HABITS) study” is the name of our systematic review study. In order to introduce this earlier and avoid doubt as to what this refers to, we have incorporated our study name into the title of our revised manuscript, which now reads: “The 2016 HIGh Heels: Health effects and psychosexual benefits (HIGH HABITS) study: systematic review of reviews and additional primary studies” (lines 1-4)

Reviewer 2: p11, l27-32: “…whereas helping behaviours were only influenced by the confederate’s heel height in male participants”. I think I understand the intended meaning here, but the authors might consider rewording for clarity. For example, the word 'confederate' is not commonly used to describe a wearer of high heels! The term 'helping behaviours' may also need some explanation. p11, l34: Similarly, the term 'point-light task' could perhaps be clarified.

Response: We accept that ‘confederate’ is a term mainly used in the psychological literature. It is used by reports on the psychosexual studies included in our review. We have added an explanation of this term at first mention (lines 313-315): “a person who participates in a psychological experiment pretending to be a participant but is working for the researcher”. We have changed “helping behaviours” to “whether or not a man was willing to help a woman” (line 312). The technical term is likely unnecessary in this article. We have added “in light-based silhouette form” before ‘in a point-light task’ (lines 316-317) in order to clarify what the task involved.

Reviewer 2: p13, last paragraph: The authors might reconsider the use of the word 'beneficial' when stating that high heels increase attractiveness in the eyes of men. I suspect some women may object to the suggestion that they wear high heels for this reason or that male attention is inherently beneficial.

Response: We have changed this to “increases women’s attractiveness to men” (line 380). The psychosexual studies are experimental so such an inference can be made.

Reviewer 3: I would also defer to the journal preference in removal of first person language. It is not my preference to see language such as "we" etc in an article but if this is something the journal allows then that should be conveyed through the editor.

Response: We have decided to retain the first-person writing style, as we think it is the most engaging way to tell the story. We have found this style in other papers published in BMC Public Health. Webb (J Adv Nurs 1992; 17: 747-52) writes that “use of the neutral, anonymous third person is deceptive when applied to quantitative research because it obliterates the social elements of the research process. With regard to research in the qualitative, critical and feminist paradigms, it is further argued that the use of the first person is required in keeping with the epistemologies of the research and in the pursuit of reflexivity”. We agree with Webb.
Reviewer 3: I also remain unconvinced that the literature supports the subsequent statement of an serious first person injury being related to high heel use given the literature within the review. "Serious" generally is a term relating to risk of death and the article relating to first injury data primarily describes fractures, sprains and strains. The authors almost state this in the second part of the paragraph and therefore discount their initial strong condemnation of footwear. This paragraph is not cohesive.

Response: The reviewer has interpreted serious in a very different way than we were using it. We used the meaning that we considered most common in public health and public policy discourse, which is a meaning that closely parallels the lay meaning. In this meaning, a fracture would be considered serious. However, ‘serious’ can also in other clinical settings be used to refer specifically to risk of death or hospitalisation, which is clearly not how we intended the term. After consideration, in light of the potential broad readership of this article, we decided to avoid the term ‘serious’ in contexts where it related to high heel related injury. We instead used “injury/injuries requiring ED attention” at each mention.

CONCEPTUAL ISSUES

Reviewer 2: In particular, the authors' discussion of high heels in relation to children, as well as the compulsory nature of high heel wear for women in some professions, offer important insights and may serve to raise awareness among these vulnerable groups. I have relatively few suggestions for improvement, which are listed below.

Response: The societal aspects of the review are very important to us as authors, and also appear to form an important aspect of why this work and the wider topic is interesting and important. We bear your comment in mind when revising the introduction and discussion

Reviewer 2: p12, l34-42: "However, there is still no clear epidemiological evidence of an association between high heel wear and OA, despite a large volume of biomechanical evidence dating back to the seminal paper in the Lancet in 1998 by Kerrigan et al".

I think the wording could be a bit more cautious here. I interpret this sentence to mean that the authors believe firmly that high heels increase the risk of OA, and that Kerrigan's paper offers strong evidence in favour of this. I would disagree with this interpretation, since Kerrigan simply showed that the moment about the knee is larger when walking in heels compared to barefoot. In their conclusion they state that "The altered forces at the knee caused by walking in high heels may predispose to degenerative changes in the joint". Whilst this may be true, their paper does not offer evidence to confirm it.

Response: We did not intend to imply this and have revised it to “However, there is still no clear epidemiological evidence of an association between high heel wear and OA. Therefore, the suggestion from a large volume of biomechanical evidence dating back to the seminal paper in the Lancet in 1998 by Kerrigan et al that high heel wear may predispose women to OA has not been confirmed in population-based studies.” (lines 345-349)
Reviewer 3: Overall comments; I would encourage the authors to consider the following changes that promote a negative instead of neutral connotation when discussing the impact of footwear.

Reviewer 3: An example of this is:

Previous studies and reviews have provided evidence that high heels are detrimental to health.....etc

Yet in other parts, authors have conceded that there is an association between footwear and health. Therefore association does not equal causation. There is a continual flavour of this throughout the article and the authors should take care to ensure this is not the case.

Reviewer 3: Again, the authors must be reminded that association does not lead to causation. Pg 12 ln 24-26 - OA and HAV has additional risk factors including genetic and therefore this statement should be association and not so definitive.

Response: We address these three comments together since they are related in theme. We contend that our review article is far more positive in tone than prior reviews on this topic, which have solely focused on negative health outcomes either using biomechanical or epidemiological outcomes. In contrast, we present information about psychosexual benefits. Indeed, it is the tension between these psychosexual benefits and the negative musculoskeletal health outcomes, as we already state in our manuscript, that this this such an interesting and complicated topic. As McKee and colleagues (Eur J Public Health 2015; 25: 1-2) succinctly explain, public health research and practice is never ‘neutral’. Indeed, to be effective it has to be informed and guided by contemporary perspectives and debates. Addressing the comments specifically about causality, it is important to note that prior reviews have largely been of experimental biomechanical literature. As discussed in Barnish and Barnish (2016, as cited in this manuscript), one major advantage of the experimental biomechanical literature is its greater ability to infer causation. Therefore, the statement “Previous studies and reviews have provided evidence that high heels are detrimental to health.....etc” is valid. On the other hand, we have used “association” appropriately to discuss epidemiological studies. We have gone through the first paragraph of the discussion and confirmed that causation is used only in relation to experimental biomechanical/psychosexual studies, and association is used relation to epidemiological studies. In response to your third comment and also a suggestion by Reviewer 2, we have amended the sentence about Kerrigan, to which we believe you were alluding, to read “However, there is still no clear epidemiological evidence of an association between high heel wear and OA. Therefore, the suggestion from a large volume of biomechanical evidence dating back to the seminal paper in the Lancet in 1998 by Kerrigan et al, that high heel wear may predispose women to OA has not been confirmed in population-based studies” (lines 345-349).

Reviewer 2: "Public health is closely linked to politics and some political features, for example Left-of-centre rather than Right-of-centre, have been demonstrated to be more health promoting [28]".
It is not clear to me how this sentence logically follows from the previous, which refers to the potential of high heels to cause physical harm. In any case, it is a little ambiguous, and does not add any concrete information to the 'story' being told.

Response: We have restructured the introduction as requested by Reviewer 1 and this sentence has been removed.

Reviewer 2: p10, l44 onwards: "Looking specifically at lumbar lordosis, considering literature up to 2010, Russell [33] found inclusive results, a result that has been since superseded by Cronin [21] in his findings of qualitatively consistent alterations in kinetics and kinematics from the spine to the toes".

This takes my statement a little out of context. Indeed in my review article I discussed the inconsistencies in the literature regarding lordosis, and with the above-cited statement, I certainly did not intend to imply that all studies are actually in agreement on this issue. I suggest revising.

Response: We have revised accordingly. This now reads: “Looking specifically at lumbar lordosis, considering literature up to 2010, Russell [27] found inconclusive results. Meanwhile, Cronin [22] found qualitatively consistent alterations in kinetics and kinematics from the spine to the toes, although some studies were not in agreement on lumbar lordosis.” (lines 294-297)

Reviewer 3: Comment: Page 13 Ln 51 - First sentence - what about High heels is a challenging topic? There also continues to be a "flavour" of how to get women to stop wearing high heels through the discussion. In spite of feminist theories introduced into this discussion, where does one draw the line at preaching on choice, in the presence of only moderate evidence supporting detrimental choices but strong impact on psychosocial health benefits. Which is better, I'm not sure one is better than the other. The authors should strong consider reducing much of the discussion while remaining factual. For e.g.: Inclusion of reference 34 - whilst a systematic review, this article was built on many assumptions and therefore not conclusive that high heel use directly is dangerous to children's posture.

Response: After the sentence “High heels are a challenging topic” (line) we have added “due to the tension between health and psychosexual considerations”. We have gone through the discussion and made revisions to ensure it keeps on focus. Freedom of choice in light of debates over compulsion is our key framing context. However, we all have an interest in child health and Reviewer 2 agrees with us that the perspective about children is valuable. We have cut discussion that we felt was off-topic. The role of society is usually considered to be different for children (as legally defined) and adults, and it is reasonable to consider issues of child protection. The Silva et al review meets our inclusion criteria and we have checked that our data extraction is correct. The social discussion is a key feature and strength of this topic and of our work.

METHODOLOGICAL ISSUES

Reviewer 1: You seem to have included overview papers - which is not an issue; but I would like to see some information on how you maintained the quality. After reviews you have added
several primary publications. These seem to be from various countries - that's OK. But were these English texts? If not did you translate them? Again how did you maintain quality? What data did you get from the meta data?

Reviewer 1: One of your aim is to provide a commentary on psychosexual literature. This is interesting but a very few studies of questionable quality? This plus the case studies doesn't really make a systematic review?

In terms of discussion, again, it is like the introduction - you are trying to review and critically analyse some evidence which is either of low quality or non existent. This in turn turns your discussion to be descriptive.

Reviewer 1:

Overall, This topic is certainly of interest and I am happy if this paper is presented as another review. I am not sure if this a structured systematic review. I would like to see a clear conclusion and directions for other authors. This is missing from the main text. You do have one within the abstract but I am not sure about the conclusion section of the abstract.

Response: We shall answer these comments together since they address the same principal question. A systematic review is defined by a structured and reproducible methodology. We have presented our methods in a clear and structured way, following PRISMA. Whether an article is a systematic review or not is not defined by the number of studies identified. The psychosexual literature is an emergent field but is an interesting aspect of this topic for the reasons we discuss elsewhere, so it makes a valuable contribution to the article. As we state in our manuscript, we decided to include review papers to order to provide better evidence synthesis since we were aware that there were a number of published review papers on aspects of this topic. We added additional primary studies in areas where prior reviews were lacking, and in the case of second-party injury, the evidence is at such an early stage that we considered case reports to offer a valuable perspective. The type of studies that can be included in a systematic review is no restricted, as long as the article provides clear inclusion criteria. Our language criteria, following the language skills of the team, are stated in the methods section. It emerged that all articles that met the inclusion criteria were available in English. The country of study need not correspond to the language of publication, as English is considered the predominant language of scientific communication. We have classified this comment as conceptual, but it does also touch on the structure of the discussion. We have introduced sub-headings and, in light of your comment, and a comment from Reviewer 2, sought to provide a clearer conclusion. We, do, however already provide guidance for future research.

Reviewer 3: Why have only articles reported as having a negative impact on musculoskeletal health been included within the review, were articles with a positive impact? Why not just include articles with an impact, again this goes back to the flavour of negativity.

Response: Our health outcomes are musculoskeletal. Evidence and societal debates point in the direction of negative musculoskeletal health outcomes. In a systematic review, it is important to have clearly defined criteria. There is no evidence, or hypothesis, to suggest a positive effect of
high heels on musculoskeletal health. We do not have a ‘flavour of negativity’ – we are summarizing the evidence as it stands. We, unlike other reviews, actually do consider what benefits high heels may offer. This is not a health outcome. However, it is a relevant social outcome in terms of perceived attractiveness and related concepts that is we believe of interest to the public health community, so we have included this outcome to provide balance of the positive and negative features.

Reviewer 3: The authors discuss in the abstract and aim they wish to synthesis injury in first party injury but exclude reference 46 based on second party injury. This article is primarily first party injury related therefore is it in or out?

Response: In the phrase “Only one descriptive study [40] had presented second-party injury data, the toll was relatively low and these were excluded from further analysis”; ‘excluded from further analysis’ referred to being excluded by Williams and Haines from further analysis in their article rather than about exclusion from our review. We have clarified by adding “in the paper by Williams and Haines” (lines 282-283)

Reviewer 3: Risk of bias - there are many tools that allow a generic risk of bias or assessment of quality of the article- I would urge the authors to consider this to allow the reader to understand the quality of the evidence. It is not acceptable to miss this step. This is especially relevant when the authors describe “good evidence” in support of the findings

Response: One of the three reviewers raised an issue about the lack of a formal risk of bias assessment in our systematic review. In our original manuscript, we stated clearly why we could not undertake this and acknowledge that this is a limitation. The comment that “It is not acceptable to miss this step” is not in accordance with the realities of conducting a systematic review of this nature. Not all systematic reviews can include a formal risk of bias assessment, and several that we are aware of do not, for example the systematic review by Muntaner et al as cited in our introduction. While formal risk of bias assessment can be useful, it is only useful if used appropriately. It is not appropriate in cases such as ours when studies are greatly heterogeneous, since a tool needs to be applicable across all the study types. This situation is very similar to that of Muntaner et al. The reviewer mentions ‘generic risk of bias’ tools. However, these are for assessing clinical intervention studies using a wide range of methodologies, and as such are not suitable for our review. We consider that the comments in the methods and limitations sections that we already have regarding this matter are sufficient. ‘Good evidence’ does not imply formal risk of bias assessment, but rather the judgement of the authors.

STRUCTURAL ISSUES

Reviewer 1: Your introduction section has appropriate content but I am not sure if it has a clear focus. You start of with the history then go on to talk about the dress codes in the film festivals and then about the petition to the UK parliament. All these are relevant but it is not setting the scene to the question you are trying to answer. I started reading this with interest and when I finished the introduction section, I am not sure where you want to go. I would like to you provide clear subheadings which will improve the manuscript. Start with the historical perspective, then
provide an overview of the biomechanical and clinical papers. You then need to highlight the gaps in knowledge and why are you are trying to complete this review. As such I am not getting this information in a structured way.

Response: We have restructured the introduction, resulting in major text edits. We have introduced sub-headings. We consider a three-section approach to be most appropriate. Now, we firstly set the scene by looking at the cultural and policy aspects and then move on to discuss the evidence base and in turn proceed to set out what the knowledge gap is and what our aims for the review were. Hopefully, this now presents the required information in a more structured way. Please see tracked changes for details. We have updated reference numbers accordingly.

Reviewer 2: The paper lacks a concrete conclusion, ending instead with a brief discussion of the paper’s limitations. The authors might consider adding a short concluding statement to close the paper.

Response: We have added a short concluding paragraph. Please see tracked changes.

Reviewer 3: I found the results section particular difficult to follow and suggest a restructure of findings. As no meta-analysis is possible, the authors should consider presenting this against each of the aims with clear linking of the result to the tables. Removal of additional primary studies section and including this all into a succinct summary of findings against each part of the aim is needed. A suggestion would instead be of results focusing on each of the outcomes i.e.: Biomechanics factors, MSK factors, etc and the articles supporting each of these impacts. I found continual referral to different parts of tables distracting to the flow of the results and would encourage the authors to consider incorporating this into the result section to minimise this.

Response: There are different ways that the evidence synthesis could be conducted. We conducted the evidence synthesis by study design, as can be seen in our methods section and in our approach to the results section and tables. An alternative could have been to conduct the synthesis by outcome. However, we do think there are marked disadvantages to that approach. What is important, however, in a systematic review is that the analysis is conducted according to a reproducible plan that is agreed in advance. Therefore, in a systematic review, the evidence synthesis approach should not be altered post hoc. Moreover, neither of Reviewers 1 and 2 commented on the structure of the results and the way the evidence synthesis was conducted. We propose therefore to follow our pre-specified approach, which appears to correspond to the perspective of the majority of the reviewers.

We trust that these revisions are in order. Many thanks for considering our resubmission. We look forward to hearing from you in due course.

Dr Maxwell Barnish, on behalf of the authors