

PEER REVIEW HISTORY

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ARTICLE DETAILS

TITLE (PROVISIONAL)	High-heeled shoes and musculoskeletal injuries: a narrative systematic review
AUTHORS	Barnish, Maxwell; Barnish, Jean

VERSION 1 - REVIEW

REVIEWER	Cylie Williams Peninsula Health, Victoria, Australia
REVIEW RETURNED	06-Oct-2015

GENERAL COMMENTS	<p>Thank you for the opportunity to review this paper. I commend the authors for tackling a tricky topic that is of interest to many health professionals.</p> <p>Due to the complexity of lower limb conditions, I suspect this is where many of my comments have stemmed due to assumptions of associations made without comment or balance of their limitation.</p> <p>Major revisions:</p> <p>One of the major issues throughout the paper is the link with HAV and high heel shoes, that while there may have been this association established, there is no consideration to the fact that there is also a high association of HAV with foot type (flatter, shorter first ray, higher metatarsal angle), gender (female), ethnicity (Asian, African) as well as particular biomechanical changes with age ie: ankle equinus. Therefore by reporting these strong associations without the balanced view of confounding factors, there is the risk that the reader will not have the whole picture and highly weight high heel footwear as the sole culprit.</p> <p>Additionally within this review of the literature, particular with respect to OA and HAV, there was no information provided or assessed within the threads to validity on the standardization of diagnosis of OA and HAV. There are grades to OA and to HAV and where possible this is of interest to the reader to better establish understanding of the high heel shoe and medical condition relationship. If these grades and diagnosis of conditions were not reports/standardized then this is a limitation of the quality of the included paper and limitation to the strength of association.</p> <p>Consideration should be given to the outcome within Menz study - where low heels had a higher association than high heels in regards to the presence of HAV given all pressure data reports that higher heels exerts a greater force on the forefoot potentially providing a deforming force. This challenges these outcomes and again highlights the confounding associative factors in foot change and footwear correlates.</p> <p>It is this lack of biomechanical and contextual knowledge that I feel strongly lets this paper down. I would encourage the author to gain</p>
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	<p>expert support for the interpretation of some of the results as it has been presented within an epidemiological framework, the whole picture is not clear.</p> <p>Minor revisions There are a small number of minor revisions within the paper that may be more personal preference but also may defer to different journal preferences. The use of the HHS is not a standard acronym in any health profession and I would discourage it's use throughout the paper. There are a number of references that are presented out of order ie: pg 9, ln 34 and ln 36 Page 10 ln 18 – semi-high heels – this is not a commonly used term. Low, mid or high heel may be more appropriate or just report the heel height in cm/inches Page 11, ln 3 – When read out of context this sentence does not make sense – suggested change to: The estimated cost of the first-party injury presentations to the Victorian public hospital system was estimated to be \$71.579.62 over a five year period. Page 12, ln 41. Substantial risk – I would avoid making quantifiable statement where there is no evidence that the risk is that great. For example riding a motorbike without a helmet is a substantial risk of head injury however given the population of women wearing high heels every day and not presenting to ED does not make it a substantial risk Page 14 lns 12-14 Again, structural damage has not been well associated with high heel use without confounding factors. Women over the age of 40 who are pre-diabetic or diabetic have the greatest risk to achillies tendons of which heels/lifts may be a recommended treatment. I would temper this sentence. Legal adulthood is not a well used term within the medical profession – I would change to adulthood or skeletal maturity.</p>
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REVIEWER	Neil Cronin University of Jyvaskyla, Finland
REVIEW RETURNED	21-Oct-2015

GENERAL COMMENTS	<p>This paper deals with a pertinent issue and offers a novel synthesis of the area. In general I think the paper is well written and comprehensive. However, my main suggestion is to acknowledge the importance of age somewhere in the discussion. I realise that this is a complex issue, but from the studies that were included in this review, the age range of the participants varies from about 20 right up to 85. Given the known effects of ageing on relevant parameters such as balance and gait, I think some mention needs to be made of this issue. Other, minor suggestions are given below.</p> <p>Page 4, line 19: ‘...occasions than non-occasions’. Could this be clarified somehow?</p> <p>Page 5, sentence on lines 26-30 (Beginning ‘Although typically...’): Is this relevant here?</p> <p>Page 6, last bullet point: How were these non-English articles assessed? By the authors, or were other assistants used? This could be mentioned somewhere because the ability to review papers in several languages is mentioned as a study strength later.</p>
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	<p>Page 7, first bullet point: Were authors contacted if full text was not available online?</p> <p>Page 8, lines 16-21 (beginning 'Some studies...'): Could this sentence be reworded? The meaning is not clear to me.</p> <p>Page 9, line 28: Could a more useful metric be given than F value? Do these refer to ANOVA F values or some other F? Perhaps some context could be given.</p> <p>Page 10, lines 8-12: This finding of Gabell et al is mentioned a few times in the paper (also at the bottom of page 13). I think some kind of explanation needs to be given for how the habit of wearing high heels can influence falls likelihood, even when not wearing the shoes. It is known that regular high heel use is associated with various structural adaptations, and perhaps these contribute to the higher falls risk (see point below re page 14, line 8).</p> <p>Page 12, lines 8-10: The fact that there is little evidence regarding second-party injury is not really a major finding in my opinion.</p> <p>Page 12, lines 43-45: The wording here is a little awkward. Instead of 'can demonstrate that the effect is due to heel height rather than heel width', could something like this be clearer: can separate/distinguish the effects of heel height and heel width.</p> <p>Page 14, line 8: The wording here is too strong. Csapo et al did not demonstrate damage to the tendon, but rather a shortening/stiffening. Although this has negative consequences for movement, it does not imply damage as such. Nonetheless, the increase in stiffening could support the point being made here, because even when removing the high heels, there is likely to be much higher passive stiffness in the muscle-tendon unit, and this may have implications for balance control and thus falls risk.</p> <p>Page 14, lines 19-21: I agree that this is striking. Could it even be a major finding of this review?</p> <p>Page 15, last line: There seems to be a word missing at the end of the sentence.</p> <p>Supp table 2: From this summary it seems that the definition of heel height or what constitutes a high heel shoe differs quite markedly. Perhaps more could be made of this issue in the discussion?</p> <p>Also for this table, is it possible to add the age of the included participants for the Moore, Nagata and Tencer studies?</p> <p>Finally, for the Yung-Hui reference, what is meant by 'normal' women? Healthy?</p>
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VERSION 1 – AUTHOR RESPONSE

Reviewer 1 (Cylie Williams, Peninsula Health, Victoria, Australia)

• Comment: Thank you for the opportunity to review this paper. I commend the authors for tackling a tricky topic that is of interest to many health professionals. Due to the complexity of lower limb conditions, I suspect this is where many of my comments have stemmed due to assumptions of associations made without comment or balance of their limitation.

- Response: We thank the reviewer for her interest in the topic and our paper and her appreciation of our efforts. We acknowledge this is a very complex topic both scientifically and societally
- Comment: One of the major issues throughout the paper is the link with HAV and high heel shoes, that while there may have been this association established, there is no consideration to the fact that there is also a high association of HAV with foot type (flatter, shorter first ray, higher metatarsal angle), gender (female), ethnicity (Asian, African) as well as particular biomechanical changes with age ie: ankle equinus. Therefore by reporting these strong associations without the balanced view of confounding factors, there is the risk that the reader will not have the whole picture and highly weight high heel footwear as the sole culprit. Consideration should be given to the outcome within Menz study - where low heels had a higher association than high heels in regards to the presence of HAV given all pressure data reports that higher heels exerts a greater force on the forefoot potentially providing a deforming force. This challenges these outcomes and again highlights the confounding associative factors in foot change and footwear correlates. It is this lack of biomechanical and contextual knowledge that I feel strongly lets this paper down. I would encourage the author to gain expert support for the interpretation of some of the results as it has been presented within an epidemiological framework, the whole picture is not clear
- Response: We consider that the best response to offer here is a justification of taking an epidemiological approach. The explicit aim and scope of the systematic review we present is to assess epidemiological evidence with regard to the link between high heels and a series of musculoskeletal health outcomes. As we explore in the discussion, there are 2 main approaches that can be taken – the biomechanical approach and the epidemiological approach – each with their associated strengths and limitations. The biomechanical approach has hitherto been dominant, although one of its major limitations is that it relies on proxy measures of clinical conditions. There have been a number of reviews published from a biomechanical perspective, most recently Neil Cronin’s excellent review in 2014, which we cite in the introduction of our manuscript. The biomechanical approach has been well addressed and we do not feel that another biomechanical review so soon after Cronin’s publication would be justified. Indeed, Marc Linder, as cited in our manuscript, stated that the lack of epidemiological evidence was a major barrier to the introduction of measures to prevent the wear of high heels being compulsory, for example at work. We therefore strongly believe that the field requires a review from an epidemiological perspective and that is what we offer here. We do seek to relate our findings to biomechanical findings in the discussion, however we cannot be exhaustive in that regard and must seek to characterise the body of evidence rather than focus to a great extent on single studies given the very large number of biomechanical studies that have been published. In addition, the Editor has requested minor revisions within 2 weeks and any major refocusing of this nature would require a much longer timescale. This suggests that the Editor agrees that our focus is appropriate and makes a novel contribution to the literature. However, we have added an additional point regarding confounding in our comparison of the biomechanical and epidemiological approaches. The relevant sentence now reads ‘However, they tend to be observational, may be subject to recall bias regarding past exposure, may be affected by residual confounding and may not be able to separate the effects of different shoe parameters’. While clearly beyond the scope of the current review, we do consider that a review integrating biomechanical and epidemiological evidence may be of future interest. Therefore, we add the following concluding remark to our manuscript ‘Here we present a systematic review of epidemiological evidence relating the use of HHS to musculoskeletal health outcomes. A future review on this topic could seek to integrate the hitherto relatively separate bodies of biomechanical and epidemiological literature.’
- Comment: Additionally within this review of the literature, particular with respect to OA and HAV, there was no information provided or assessed within the threads to validity on the standardization of diagnosis of OA and HAV. There are grades to OA and to HAV and where possible this is of interest to the reader to better establish understanding of the high heel shoe and medical condition relationship. If these grades and diagnosis of conditions were not reports/standardized then this is a limitation of the quality of the included paper and limitation to the strength of association
- Response: We have added the following sentence to the end of our comparison of the

biomechanical and epidemiological approaches 'Moreover, although the epidemiological approach offers greater direct insight into clinical features than the biomechanical approach, differences in diagnostic criteria, grading systems and classifications may present a further limitation.'

- Comment: The use of the HHS is not a standard acronym in any health profession and I would discourage its use throughout the paper
- Response: From our extensive reading, papers across the scholarly literature regarding high heels tend to use an acronym. Some authors use 'HHS' and others use 'HH'. We consider that 'HHS' is most appropriate for us since we are using the term 'high-heeled shoes' rather than the more informal 'high heels' in our manuscript. We have defined the acronym at first use and leave the final decision regarding this matter to the Editor
- Comment: There are a number of references that are presented out of order ie: pg 9 ln 34 and ln 36
- Response: Reference numbers have changed during this revision. We have conducted a thorough re-check of all reference numbers following the completion of this revision.
- Comment: Page 10 ln 18 – semi-high heels – this is not a commonly used term. Low, mid or high heel may be more appropriate or just report the heel height in cm/inches
- Response: While we agree that 'semi-high' is not the most commonly used term, it was a term used to categorise heels in the study that we are citing here. We wish to retain authors' original conceptualisation schemes in order to retain fidelity and avoid introducing bias in the interpretation of the results
- Comment: Page 11, ln 3 – When read out of context this sentence does not make sense – suggested change to: The estimated cost of the first-party injury presentations to the Victorian public hospital system was estimated to be \$71.579.62 over a five year period
- Response: We thank the reviewer for her suggested phrasing and have made the suggested change
- Comment: Page 12, ln 41. Substantial risk – I would avoid making quantifiable statement where there is no evidence that the risk is that great. For example riding a motorbike without a helmet is a substantial risk of head injury however given the population of women wearing high heels every day and not presenting to ED does not make it a substantial risk
- Response: We have changed the phrasing from 'substantial risk' to 'substantial toll' since the evidence provided this review is in terms of the number of presentations (i.e. a toll count) rather than a risk estimate per se. We do contend that the toll presented especially in the Moore et al study is substantial and Reviewer 2 agrees that the injury toll among children is particularly striking. We have also made the corresponding change in phrasing in the abstract
- Comment: Page 14 lns 12-14 Again, structural damage has not been well associated with high heel use without confounding factors. Women over the age of 40 who are pre-diabetic or diabetic have the greatest risk to achilles tendons of which heels/lifts may be a recommended treatment. I would temper this sentence
- Response: This sentence has been revised in light of this comment and comments from Reviewer 2. It now reads 'This finding should be interpreted in combination with evidence of stiffening and shortening of the Achilles tendon and radiographic evidence of alterations to the spino-pelvic structure associated with HHS use.'
- Comment: Legal adulthood is not a well used term within the medical profession – I would change to adulthood or skeletal maturity
- Response: We have rearranged the sentences in question to now refer to the 'legal age of adulthood' rather than the 'age of legal adulthood'. The legal aspect is important here with regard to societal and parental responsibility

Reviewer 2 (Neil Cronin, University of Jyväskylä, Finland)

- Comment: This paper deals with a pertinent issue and offers a novel synthesis of the area. In general I think the paper is well written and comprehensive
- Response: We thank Professor Cronin for his appreciation of our work, both in terms of its pertinence, by which we expect he is referring in part to the contemporary public debates we seek to reference, and also the novelty of our synthesis. We owe a great debt to Professor Cronin's 2014 review article on the biomechanical aspects of this field of study and seek our epidemiological

perspective to produce a useful complementary perspective

- Comment: However, my main suggestion is to acknowledge the importance of age somewhere in the discussion. I realise that this is a complex issue, but from the studies that were included in this review, the age range of the participants varies from about 20 right up to 85. Given the known effects of ageing on relevant parameters such as balance and gait, I think some mention needs to be made of this issue
- Response: We agree and have added the following sentence on p.13 'With regard to confounding, for example, studies included in the current review have a wide variety of age ranges and the effects of HHS and the ageing process on outcomes may not be entirely separable'
- Comment: Page 4, line 19: '...occasions than non-occasions'. Could this be clarified somehow?
- Response: These are the terms used by Dilley et al whom we are citing here. We are careful to not alter these terms as they reflect the underlying theoretical framework in the cited work – we consider that they are readily understandable
- Comment: Page 5, sentence on lines 26-30 (Beginning 'Although typically...'): Is this relevant here?
- Response: We were unsure at original submission whether this sentence was required or not and in response to your comment, this sentence has now been removed
- Comment: Page 6, last bullet point: How were these non-English articles assessed? By the authors, or were other assistants used? This could be mentioned somewhere because the ability to review papers in several languages is mentioned as a study strength later
- Response: We consider it would break the flow to add this clarification in the bullet point. However, we have clarified this point in the strengths section of the discussion, where the relevant sentence now reads 'Fourthly, drawing on the linguistic skills of the authors, articles in seven languages could be considered for inclusion'
- Comment: Page 7, first bullet point: Were authors contacted if full text was not available online?
- Response: We have now added the following sentence of clarification regarding access to full text articles: 'Full text articles were retrieved in electronic form where this was available, otherwise in paper copy. An array of leading research libraries were accessible to the authors including the British Library. If an article could not be retrieved from any of these sources, it was excluded'. We managed to retrieve 96% of full text articles we sought, but the non-retrieval of 2 potentially relevant articles was mentioned as a limitation in the original submission and this sentence of limitation remains in our revision
- Comment: Page 8, lines 16-21 (beginning 'Some studies...'): Could this sentence be reworded? The meaning is not clear to me
- Response: We have considered carefully this comment and decided that this sentence is not essential to our argument and is potentially confusing – it has therefore been removed
- Comment: Page 9, line 28: Could a more useful metric be given than F value? Do these refer to ANOVA F values or some other F? Perhaps some context could be given
- Response: Here we are reporting the effect magnitudes using the measures reported in the original study reports – these F values do refer to analysis of variance (ANOVA) and we have added the words 'analysis of variance' for clarification
- Comment: Page 10, lines 8-12: This finding of Gabell et al is mentioned a few times in the paper (also at the bottom of page 13). I think some kind of explanation needs to be given for how the habit of wearing high heels can influence falls likelihood, even when not wearing the shoes. It is known that regular high heel use is associated with various structural adaptations, and perhaps these contribute to the higher falls risk (see point below re page 14, line 8)
- Response: We agree and have added the following sentence on page 14 'Indeed, these structural adaptations may be a possible explanation for the finding of Gabell et al that HHS wearing habit may predispose women to falling even when not wearing HHS at the time of the fall.'
- Comment: Page 12, lines 8-10: The fact that there is little evidence regarding second-party injury is not really a major finding in my opinion
- Response: Second party injury was one of the major strands of our research question and the absence of substantial research on this area is surprising to us. In the context of current public

debates, for example around 'health and safety', it is important for us to emphasise here that, while we do provide evidence that high heels appear to be detrimental to women's own health, the fears that they may injure other people are supported by very little evidence, at least currently

- Comment: Page 12, lines 43-45: The wording here is a little awkward. Instead of 'can demonstrate that the effect is due to heel height rather than heel width', could something like this be clearer: can separate/distinguish the effects of heel height and heel width

- Response: We have made the suggested change

- Comment: Page 14, line 8: The wording here is too strong. Csapo et al did not demonstrate damage to the tendon, but rather a shortening/stiffening. Although this has negative consequences for movement, it does not imply damage as such. Nonetheless, the increase in stiffening could support the point being made here, because even when removing the high heels, there is likely to be much higher passive stiffness in the muscle-tendon unit, and this may have implications for balance control and thus falls risk

- Response: We have revised the sentence in light of this comment and comments from Reviewer 1. It now reads 'This finding should be interpreted in combination with evidence of stiffening and shortening of the Achilles tendon and radiographic evidence of alterations to the spino-pelvic structure associated with HHS use.'

- Comment: Page 14, lines 19-21: I agree that this is striking. Could it even be a major finding of this review?

- Response: We agree with the reviewer. We have now added the sentence 'The third major finding is the striking injury toll among underage girls including a substantial number under the age of 10.'

- Comment: Page 15, last line: There seems to be a word missing at the end of the sentence

- Response: There was not a missing word, however we apologise if our sentence here was awkwardly phrased. We have now changed it to 'Fourthly, we suggest that HHS use is taken into account in future studies of women's and musculoskeletal health to address the potential confounding effects of HHS use on the relationships being investigated.'

- Comment: Supp table 2: From this summary it seems that the definition of heel height or what constitutes a high heel shoe differs quite markedly. Perhaps more could be made of this issue in the discussion? Also for this table, is it possible to add the age of the included participants for the Moore, Nagata and Tencer studies?

- Response: We agree that the definition of heel height is variable and this was mentioned as a limitation in the original submission. However, our original mention of this issue was confined to the fact it precluded meta-analysis rather than how it could affect the precision of the results. In order to address issues of the variability of heel heights and indeed heel shapes, we have now added to the limitations section 'In addition, the variability of cut-offs for heel height and the frequent failure of studies to take into account factors such as heel shape may reduce the precision of the findings and in some cases may explain the absence of significant results. With regard to heel shape, it is possible that the effect of stiletto, platform and wedge heels for example may differ and that curved heels and 'heelless heels', which are now fashionable, may have a different effect than more traditional heel styles.' Regarding age ranges, with regard to Moore and Nagata (and indeed Williams), these studies use the incident as the unit of analysis rather than the individual woman/participant. Therefore, defining a mean or median age is not appropriate for these studies in our considered opinion. However, we present the age distribution as part of the results in the ED admissions studies of Williams and Moore. Now, we add clarification to Supplementary Table 2 regarding the general age criteria for the Moore, Williams and Nagata studies. With regard to the Tencer study, we have added further clarification of the participant ages to clarify the meaning of 'older adults' in Supplementary Table 2

- Comment: Finally, for the Yung-Hui reference, what is meant by 'normal' women? Healthy?

- Response: Yes this is correct and we have changed the word from 'normal' to 'healthy' in Supplementary Table 2

VERSION 2 – REVIEW

REVIEWER	Cylie Williams Peninsula Health/Monash University, Australia
REVIEW RETURNED	04-Dec-2015

GENERAL COMMENTS	<p>Thank you to the authors on the quick turn around of review and for the changes considered as part of making this article more relevant to the readership. I have a number of small suggestions that are more editorial in nature that I am happy for the editor to consider during the review process as to whether they are relevant.</p> <p>Page 1 – The addition of narrative in the title is unnecessary. You have conducted a systematic review and included the prisma flowchart, you have been unable to analyse the data with metanalysis however it is still a systematic review.</p> <p>Page 4 – I still continue to have issues with jargon acronyms and the HHS but will leave that to the editor.</p> <p>Page 5 – Ln 3 – Consider changing to “the potential for high heel shoe wearing to result in injury to both the wearer and on bystanders should be considered”. The shoe is only a part variable in injury.</p> <p>Page 5 – Ln, 50 – removal of narrative</p> <p>Page 8 – Suggest the addition in results: “Metanalysis was unable to be undertaken due to the heterogeneity of results. Results were thematically described.</p> <p>Page 10, Ln 10 – change didn’t to did not</p> <p>Page 10 – Ln 27 – Suggest change to “found that 74% of stair injuries within the workplace were women”</p> <p>Page 11 Ln18 – Value should be \$71,579.62 AUD</p> <p>Page 12 Ln 23 – I appreciate the authors changing to toll and agree it is better terminology however have issues with the quantifier – substantial. This quantifier implies that anyone wearing high heels will be injured and we know that just isn’t the case. I would say moderate or even just remove the quantifier. This is similar to the emotive language used in Lns 25 and 26 – of striking injury toll and substantial number of injuries in children. The fact there is injuries is alarming however these low numbers are neither striking nor substantial .</p>
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REVIEWER	Neil Cronin University of Jyvaskyla, Finland
REVIEW RETURNED	17-Nov-2015

GENERAL COMMENTS	The authors have made a thorough job of revising the paper and I am happy to recommend acceptance.
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VERSION 2 – AUTHOR RESPONSE

Reviewer: 1 (Cylie Williams)

Comment: Thank you to the authors on the quick turn around of review and for the changes considered as part of making this article more relevant to the readership. I have a number of small suggestions that are more editorial in nature that I am happy for the editor to consider during the review process as to whether they are relevant.

Response: We thank the reviewer for her positive comments and have sought to address the remaining minor issues raised in her review

Comment: Page 1 – The addition of narrative in the title is unnecessary. You have conducted a systematic review and included the prisma flowchart, you have been unable to analyse the data with metanalysis however it is still a systematic review.

Response: We have made the requested change and removed the word 'narrative' from the title

Comment: Page 4 – I still continue to have issues with jargon acronyms and the HHS but will leave that to the editor.

Response: We have now decided to eliminate the acronym HHS from the manuscript and all associated supplementary files. We use 'high-heeled shoes' at the first mention in the text with 'high heels' in brackets and use the shorter, but familiar, term 'high heels' at subsequent mentions

Comment: Page 5 – Ln 3 – Consider changing to "the potential for high heel shoe wearing to result in injury to both the wearer and on bystanders should be considered". The shoe is only a part variable in injury.

Response: We have made the suggested change

Comment: Page 5 – Ln, 50 – removal of narrative

Response: We have made the suggested change

Comment: Page 8 – Suggest the addition in results: "Metanalysis was unable to be undertaken due to the heterogeneity of results. Results were thematically described.

Response: We have made the suggested addition, but in keeping with the rest of the article, used the UK spelling 'meta-analysis'

Comment: Page 10, Ln 10 – change didn't to did not

Response: We have made the suggested change

Comment: Page 10 – Ln 27 – Suggest change to "found that 74% of stair injuries within the workplace were women"

Response: No change made. The Nagata source states that 74% of all stair injuries at work in the study sample that occurred to women under the age of 24 involved shoes defined as high or semi high heeled, rather than that 74% of total occupational stair injuries were suffered by women rather than men. Therefore, we believe our sentence more accurately summarises the finding. The reviewer's suggested phrasing is more elegant, but we believe it would introduce unhelpful ambiguity of meaning.

Comment: Page 11 Ln18 – Value should be \$71,579.62 AUD

Response: We have added 'AUD' as requested to clarify that the currency in question is the Australian dollar rather than the US dollar.

Comment: Page 12 Ln 23 – I appreciate the authors changing to toll and agree it is better terminology however have issues with the quantifier – substantial. This quantifier implies that anyone wearing high heels will be injured and we know that just isn't the case. I would say moderate or even just remove the quantifier. This is similar to the emotive language used in Lns 25 and 26 – of striking injury toll and substantial number of injuries in children. The fact there is injuries is alarming however these low numbers are neither striking nor substantial .

Response: We have carefully reconsidered this paragraph and made the suggested revisions to the phrasing to eliminate the problematic quantifiers. We have modified the second major finding to read: 'The second major finding is that there is strong evidence that HHS are associated with first-party injury but that scientific evidence regarding second-party injury is limited'. We have decided to present the actual figures in the third major finding to enable readers to make an informed assessment. It has

been modified to 'The third major finding is the injury toll among underage girls including 4737 injuries to those under the age of 10 in the study by Moore et al.38'

Reviewer: 2 (Neil Cronin)

Comment: The authors have made a thorough job of revising the paper and I am happy to recommend acceptance.

VERSION 3 - REVIEW

REVIEWER	Cylie Williams Peninsula Health/Monash University, Australia
REVIEW RETURNED	10-Dec-2015

GENERAL COMMENTS	Thank you for the work undertaken to develop and refine this article. I am now happy to recommend the article in its current form be published.
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REVIEWER	Neil Cronin University of Jyvaskyla, Finland
REVIEW RETURNED	10-Dec-2015

GENERAL COMMENTS	The reviewer completed the checklist but made no further comments.
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