Author's response to reviews

Title: The foot-health of people with diabetes in a regional Australian population: a prospective clinical audit

Authors:

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Author's response to reviews: see over
Thank you again for the opportunity to improve our paper. We appreciate the thoughtful comments and have addressed them as appropriately as we can.

We would be grateful if the revised paper is considered for publication in the *Journal of Foot and Ankle Research*. Byron Perrin, Marcus Gardner and Susan Kennett.

Please note: x2 typographical errors were fixed and the email address for B. Perrin was updated.

Reviewer 1: Peter Lazzarini

Abstract: Results: Sentence 2. “Over 49% had a UT risk classification at a level of at least peripheral neuropathy”. I think this is still unfortunately misleading, resulting in the reader assuming that over 49% had peripheral neuropathy”. May I suggest this is tightened as the authors have done in Discussion: Paragraph 2: Sentence 2 by just saying “Over 49% had a UT risk classification at a level at least peripheral neuropathy or more serious diabetes-related foot morbidity” to absolutely clarify to the reader reading the abstract.

Response: The change has been made as suggested.

Reviewer 2: Vanessa Nube

1. Reviewer one, point 3. I also found it difficult to conceptualise the actual patient population. I agree that Table 1 provides a high level of detail but an overarching statement would improve the reader’s understanding given that the eligibility would strongly influence the characteristics of the patient population.

Response: A sentence has been added in the Background section to give an overview of the podiatric services’ funding sources and eligibility criteria. Paragraph 3, sentence 3.

   “Eligibility criteria for the podiatric services were consistent with the aims of the specific funding source for each service, which ranged from helping maintain independence in frail-aged and disabled populations (the Home and Community Care program) to preventing re-admission for serious diabetes-related foot complications (the Hospital Admissions Risk Program).”

2. Reviewer one, point 4. I agree with reviewer one. The authors have used the classification system published in 1996 ¿A treatment-based classification system for assessment and care¿ that includes a separate category for people with ischaemia. I don’t agree with the authors’ interpretation of the 1996 paper as justification to group ischaemia (alone) alongside foot ulceration, infection and Charcot arthropathy as an “active diabetes-related foot complication” but accept that this is their prerogative. The limitation in assessment of blood flow is not the point of contention and everyone agrees ischaemia is a risk factor for amputation. Please consider including a statement in the method that the inclusion of ischaemia (alone) as an “active diabetes-related complication” is the authors’ design and in the discussion that this grouping may not be consistent with other systems such as described in Lavery (2008).
Response: It is possible we are misinterpreting the point of the reviewer. It seems to us that the reviewer is making point that there are classification systems available to be used that may better incorporate levels of arterial disease with other risk factors for complications such as peripheral neuropathy. We agree with this point, and this is demonstrated by the attached article kindly provided by the reviewer (Lavery 2008). However, the system we used was the UT Texas system, which from our interpretation has placed “ischaemic foot” as a separate category in line with risk factor for amputation. As such, in order to be consistent with the use of the UT system we approached the categorisation according to our interpretation of the system- as such we would argue that this is not our design but that of our intention to be consistent to the system described. What was our design was how we analysed the results, and we have raised the limitations of this in the discussion.

We wonder if it were more appropriate if we discuss the limitations more explicitly about both issues (of pooling categories and the ischaemia) in the discussion: Paragraph 9, sentence 2:

“Whilst this strengthened the statistical analysis, this may detract from comparing the results of the study to other studies that report the use of the UT risk classification system without pooling the risk categories. Caution should also be taken in comparing the results of this study with other studies that utilised other risk classification tools available that integrate risk factors (such is ischaemia) to designate risk categories differently to the UT Texas system [29].”

3. Bergin’s data is population level and the current study should expect to find that people with diabetes attending the podiatry service to have foot problems. As such, I suggest that the information from the Bergin study be included as background information about the region’s population and that the authors not infer that the current cohort is representative of the population per se. See also, reviewer one point 14.

Response: Bergin’s paper was raised in the discussion (paragraph 4- reference 8) as we feel this work provides important contextual information about the potential levels of burden of disease for diabetes-related foot problems but we also stress that we did not actually measure levels of social deprivation and that further research was required. In this section, we do not believe strong inferences are made about our population based on Bergin’s data specifically.

With respect to reviewer 1 point 14 we are not entirely clear how this is linked...

“14. Discussion: Paragraph 6: Sentence 4. Again this reviewers statistical ability is limited, but quickly please clarify where its demonstrated that “the effect of age was reduced from a medium to a low effect”

We would value the editor’s advice on this issue.

9. I agree that more information about the populations used for comparison would be helpful in addition to the modification already made. On page 15 it refers to ‘similar’ populations however the McGill study to which they compare is comprised of people with DM attending a diabetes centre (not a podiatry service) and the mean age is ~59 vs 71 years for the current study.
Response: We make the point in the discussion that the population in our sample is different to that of the studies used as a comparison in paragraph 8 (page 16). The contextual background of the studies is described in the Background. In order to reduce repetition we think it may be appropriate to reinforce the link to the introduction by adding in the citation for the studies we are referring to and by adding a guiding link at the end. See sentence 1 below:

“Caution is recommended in comparing the results of this study to other similar clinical population studies from Australia [13, 14] and the United Kingdom [15] (see above). Whilst the participants in this sample are regarded as coming from a clinical population a large proportion of the sample attended the podiatric service within PDM that sees patients for primary prevention of diabetes-related foot complications in a community setting.”