Reviewer's report

Title: Characteristics of Diabetic Foot Ulcers in Western Sydney, Australia

Version: 2
Date: 15 July 2014
Reviewer: Peter Lazzarini

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This paper investigates the characteristics and associates of diabetic foot ulcers in a large Western Sydney tertiary hospital’s foot wound clinic.

The authors have made some significant progress in improving this manuscript for publication and this reviewer believes they are nearly there in regards to having this paper ready for publication in JFAR. However, there are some areas of the paper that still need some clarification and correction, i.e.

• The link of low socioeconomic status to poorer diabetic foot outcomes is attempted throughout the paper, however, I'm unsure the soi results from outcome groups with no statistical differences and 3 of 5 cases in another group can justify this link
• Further clarification of the definition of neuropathy and PAD is required and/or needs to be potentially added to limitations
• Check and/or confirm some results that seem to be slightly different to each other, but, fortunately doesn't change the gist of the paper

Once the editors feel these points have been adequately addressed this paper should be of a standard ready for publication in JFAR. Again I congratulate the authors for the paper and look forward to it being published in JFAR once they address the above three broad points.

Major Compulsory Revisions

1. Abstract: Results Sentence 5. I have to query why disadvantaged SOI is mentioned in the context of 5 amputations when its not only a small number but only 3 were from a disadvantage area and 2 were from very advantaged areas. It really seems like the authors are trying to make a link to disadvantaged SOI that is probably not there with the data they have unfortunately

2. Methods: Paragraph 2. Sentence 5. Although the authors mention they have addressed this point in their response to the original Discretionary Revision 7, i.e. “we have included self reported history and previous clinicians’ diagnosis of comorbidities” I can’t actually see where this has been done. I have bumped this up to a major compulsory point as I assumed that neuropathy and PAD were clinically assessed with standard best practice non-invasive assessments to be eligible to be entered into the database from the original manuscript, however, it
looks like I may have assumed wrong as the reference to PAD and toe pressure cut offs has been removed. As per the definition of infection grade that the authors have now addressed so well, I would strongly recommend the authors also define how neuropathy was diagnosed or self-reported. A purely self-reported history of neuropathy for example without any reference could be argued to be very unreliable. I originally assumed that neuropathy was clinically assessed using a monofilament or similar. If neuropathy was self-reported by the patient only without any clinical diagnosis at the service or historically I think a sentence on the potential unreliability of this item should be added to the limitations section alongside ulcer duration in the discussion.

3. Methods: Paragraph 4: As per the point on neuropathy above I now note that the definition for PAD in the original manuscript with the use of Toe Pressures has been removed. Unless this is inaccurate I think this definition should be re-included so the reader knows where the diagnosis of PAD came from unless I’m missing something … which is certainly possible however please clarify.

4. Discussion: Paragraph 4. The authors attempt to link low socioeconomic status to poor diabetic foot outcomes seems to occur throughout the paper. That’s fine if the data supported this link, however, I don’t think you can say for example that “our study revealed a slightly lower than average soi status” when you actually go on to say it’s actually slightly higher than average in the next 2 sentences. Furthermore, from the data the authors analysed on history of amputation and foot ulcers no statistical difference was found on SOI status. Lastly, suggesting the 3 of 5 patients undergoing amputation from low soi suggests “most” are from low soi is a very broad link. I’d encourage the authors to really dampen down these type of statements to more closely mirror their findings or at least say that it’s a very broad link that is supported in other papers but not statistically here, only descriptively?

5. Discussion: Limitation Paragraph. If the ‘diagnosis’ of neuropathy is purely a patient self report, please include this in limitations as per duration of foot ulcer and above point.

6. Conclusion: Sentence 3 & 4. As per point 3 above I don’t think the authors can conclude so firmly that the “majority of patients …. were from relatively disadvantaged areas with the data and analysis they have to support this. I’d suggest significantly weakening this association or removing these sentences as I don’t think they can really be supported.

Minor Essential Revisions

1. Introduction: Paragraph 3: Sentence 1. I’m not sure this is the right reference for this sentence, as I’m not sure the ADS recommended this under the banner of the NHMRC guidelines. I stand to be corrected though.

2. Methods: Statistical Analysis. The authors have kindly added in the suggestion of why they used non-parametric tests as suggested originally, however, have now removed the actual tests performed, ie Mann Whitney U Tests and
Chi-squared. Please re-enter these, if accurate obviously, for completeness.

3. Results: Paragraph 2. Probably safest here to write something along the lines of what the authors did with the diabetes duration as only 121 participants had BMI recorded by the looks, so maybe “More than 70% of patients recording a BMI were overweight etc?”

4. Results: Paragraph 4: Last Sentence/Table 4. I really like the way the authors have now presented their ulcer types in Table 4, however, the 1B & 3B percentages given in the last sentence and table are slightly different to each other and need correcting.

5. Results: Paragraph 5 & Table 5. Table 5 seems to missing 49 participants when it comes to PEDIS infection, although it matches the infection numbers from Table 4 using UTWCS which is good.

6. Results: Paragraph 5: Sentence 2. The authors need to clarify the difference between 50 patients presenting with osteomyelitis and the 34 that were diagnosed and included in Table 4?

7. Results: Paragraph 6. The authors should define “traumatic events” in the Methods section.

8. Discussion: Paragraph 6: Sentence 2. These percentages for Grade C & D ulcers look like they should be reversed according to Table 4.

9. Discussion Paragraph 6. Sentence 3. The Results section suggests that 49.7% of ulcers were infected but this sentence says its 58.5%. The authors need to correct or clarify.

Discretionary revisions

1. Abstract: Results: Sentences 3 & 4. This is still a bit clunky and hard to easily interpret for the lay reader as really what the authors seem to be saying is simply % participants with: neuropathy, ischaemia, soft tissue infection and osteomyelitis

2. Results: Paragraph 3: Sentence 1. I’d suggest adding “(Table 1)” at the end of this sentence as the results have just been referring to Table 2 and I was looking for SOI and took me ages to realise it was back in Table 1.

3. Discussion: Paragraph 1: Sentence 4. It might be just me but I’m unsure what the 181 (79%) patients refers from Perrin’s study for this sentence, as it looks like its 79% were male but then it says 61.3% were male. The authors may need to clarify.

4. Discussion: Paragraph 2: Sentence 2. I still think the authors make too much of peripheral descriptive risk factors with the data they have instead of focussing more so on their ulcer characteristics initially where there is more alignment to the evidence. However, I’ll leave this to their discretion. For example 50%+ have a history of smoking (current or previous), I may be wrong, however I think that
this seemingly high proportion is pretty standard for the Australian population at large, along with the BMI proportions later on. Thus, it’s the authors may be drawing a long bow to diabetic foot complications if these proportions are similar to the general population without complications. Again I stand to be corrected.

5. Discussion: Paragraph 5. Sentence 6. I would have thought this sentence should strongly support the findings of this study but it doesn’t seem to make that point, ie this study found ~2% use of TCCs as did Wu et al.

**Level of interest:** An article of importance in its field

**Quality of written English:** Acceptable

**Statistical review:** No, the manuscript does not need to be seen by a statistician.

**Declaration of competing interests:**

As per previously declared in the first revision of this article.