Author's response to reviews

Title: Prevalence and Associated Characteristics of Recurrent Non-specific Low Back Pain in Zimbabwean Adolescents: A cross-sectional study

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Version: 4 Date: 14 October 2014

Author's response to reviews: see over
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**Author’s response to reviews**: see over
1. Reviewer's report

**Title:** Prevalence and associated characteristics of recurrent non-specific low back pain in Zimbabwean adolescents: A cross-sectional study

**Version:** 3 Date 14 August 2004

**Reviewer:** Vyvienne Roseline Piwai M’kumbuzi

**Major Compulsory Revisions**

1. The authors should consider the precise research question and specific objectives this manuscript addresses. The manuscript reads more about NSLBP where its recurrent nature is one variable of interest. If this manuscript is indeed about recurrent NSLBP (authors are not consistent nor convincing e.g. Conclusion line 275 refers to NSLBP), then the literature, results and discussion should focus much more acutely on this; the operational definitions for point prevalence and lifetime prevalence of recurrent NSLBP as opposed to NSLBP are required and greater assurance that the instrument used had sufficient sensitivity and specificity to detect recurrent NSLBP could be useful for the reader. Otherwise changes in the title and discussion focus are recommended.

   The manuscript reported on lifetime and point prevalence of non-specific low back pain (NSLBP) and, more importantly, on the prevalence of recurrent NSLBP. The literature, results and the conclusion emphasised on these three primary outcome variables, although the title portrays recurrent NSLBP as the variable of interest. Line 13-17 reads on lifetime prevalence of NSLBP, line 18 reads on point prevalence of NSLBP and line 19 onwards reads distinctively on the prevalence of recurrent NSLBP. The authors tried to be very clear with the operational definitions of point, lifetime prevalence for NSLBP and recurrent NSLBP in the methodology section. Because recurrent NSLBP has a more chronic evolution and leads to greater disability and increased medical attention based on literature, the authors wanted to determine the nature and character of the condition in adolescents.

2. More detail regarding the sampling methods could be useful indicators of the power of the study/representatives of this sample. Hence sample size determination at the school level and student level. Why were 3 schools selected?

   We have now included detail regarding sample size calculation and sampling methods of the students. The following statements now appear on the second paragraph under study design, line 41: “As no study has been conducted in Zimbabwe on adolescent recurrent NSLBP, Epi Info version 7.1.1.0 Statcalc package for population surveys was used to calculate the minimum sample size. The following parameters were considered: target population (71,458), estimated prevalence of recurrent NSLBP in school-children found regionally (13.5%) [25], precision effect 3%, design effect (1%) and 95% confidence interval. The sample size was calculated to be 495. However, the number was adjusted upwards in anticipation of attrition from school absenteeism and possible refusals. The final sample size was 620 students.
With regards to selection of schools, the following revised statements now appear on third paragraph, line 48: “A two-stage cluster sampling method was used to select schools and study participants. There are 55 relevant government-administered secondary schools in Harare, Zimbabwe. There are 17 secondary schools in the S1 and 38 secondary schools in the S2 category. In the first-stage of sampling, a list of all government secondary schools was constructed in clusters of two categories, S1 and S2. Each school in the two clusters was assigned a numerical number. Based on a strategy that considered proportionality between the S1 and S2 schools, one secondary school was randomly selected from the S1 category and two from S2 cluster box. This explains why three schools were selected in this study.

3. The authors underscore (and rightly so) the variability in prevalence rates of NSLBP owing to definitional issues; it may be useful to explore these in the introduction and perchance focus the manuscript more appropriately

The statement was revised and it now reads from line 11 as “……..the absolute lifetime prevalence estimates varied between studies [7-10]. This applied even for studies sharing similar definition of lifetime prevalence and similar methodological design”. The revised statement underscores variability in the prevalence rates despite similar definition of lifetime prevalence used or similar methodological designs. The definition of lifetime prevalence has been given in line 16 which reads as “Lifetime prevalence has been described to indicate the proportion of people that had experienced an episode at one point in life” [13].

4. The contents in Table 1 should be in cooperated meaningfully into the literature and may steer the authors to the research question the want to answer

Table 1 has been removed.

Minor Essential Revisions

5. Age for inclusion 13-19, any reasons why 19 was included when a child is defined as <18 years (UN) and the literature referred to mostly uses 16 as the upper limit

The World Health Organisation (WHO) definition of adolescents was adopted as people ranging between 10 and 19 years. A reference was given for the definition.

6. Apart from the lower socio-economic descriptor (which is not entirely accurate), how else could the government schools in Harare be characterised for comparison and for a global audience to appreciate the study setting. These schools include former group ‘A’ some of which are quite elite. Without disclosing the identity of the school, more detail about the study setting is warranted for a global readership

The reviewer suggestions are commendable especially considering the aspect of global readership. However, the authors feel that the categorisation of the secondary schools described probably represent reality in Zimbabwean schools. Nevertheless, the authors replaced “socio-economic status” with “location”. The revised statement now reads on line 35 “The government-administered secondary schools are classified into two categories (S1 and S2) based on location. The S1 schools are located in the low density
areas where people of high socio-economic status live [25]. The S2 schools are located in the high density suburbs where people of lowest socio-economic status reside [25].

7. Line 102- Explain how ‘original data was corrected if necessary’?

Raw data from the questionnaires was entered into Microsoft Excel. Before imported to STATISTICA version 11 for analyses, all computerised data was cross-checked against the original data to check for errors (data cleaning). However, that statement was revised.

8. Line 111- pain intensity was dichotomized, hence <5 minor? This includes a score of 0.

This statement now reads on line 154: “Pain intensity was dichotomised as mild pain (1-4 score on VAS) and severe pain (5-10 score on VAS)".

9. Provide references for line 112 and line 181

References have been given. They now appear on line 155 and 234 respectively

10. Line 121- a modal age may be a better measure of central tendency as the data is not continuous

According to Fagerland (2012) article entitled “t-tests, non-parametric tests, and large studies — a paradox of statistical practice”? Parametric tests should be used for studies with large samples even for heavily skewed data. In addition, the central limit theorem allows for the use of parametric tests when the sample size is relatively large. However, normality tests for continuous data were run for confirmation sake using the Kolmogorov-Smirnov test in association with the Lilliefors test. As such, the suggestion by the reviewer is valid but because of the large sample size, the mean age should be a better descriptor than mode.

11. Line 126-complete section heading Prevalence rates of?

We have completed the section heading and it reads on line 169 as “Prevalence rates of non-specific low back pain”

12. Provide n=? for Table 4 and Figure 3

As indicated by the reviewer, the n value for Table 4, which is now Table 3, has been provided. The n value for Figure 3 has been provided.

13. Figure 4 may be expressed precisely as text.

Change made as indicated by the reviewer. Hence Figure 4 has been removed.

Discretionary Revisions

1. In general the presentation of the results could me more creative to sustain the reader’s interest

Point noted but the general presentation of results remained similar.

2. Presentation of mean and standard deviation, consider e.g. 16±1.72 years
Change made as indicated by the reviewer.

3. Line 85- parents/guardians give parental permission not consent

The authors decided to maintain consent instead of permission

4. Line 86- the medical health questionnaire mentioned for the first time, no information on what it collects, its source/ validity?

We have now included this in the methods section from line 91 under the subsection heading medical health questionnaire

5. Line 93- 3 schools are mentioned for the first time

This was clarified first on line 51 under the study design 3rd paragraph. The statement reads “Based on a strategy that considered proportionality between the S₁ and S₂ schools, one secondary school was randomly selected from the S₁ category and two from S₂ cluster box”. This highlights that three schools were selected in total.

6. Line 141- ANOVA mentioned for the first time

The authors changed that. A full description of ANOVA as one-way analysis of variance was given in line 185.

7. Line 193- a methodological concept (Delphi) being presented for the first time

The sentence has been removed.

8. Table 2 is not mentioned in the text, it could be significant

Changes made as indicated by reviewer. Table 2 is now mentioned in the text on line 163

9. Avoid terms such as Nigerian students, Zimbabwe adolescents/students and South African students- has a labelling effect and sometimes derogatory tone

Changes were made as indicated by the reviewer.

10. In many places 2 types of referencing style are used in combination e.g. line 63, line 251. Please refer to the journal’s guidelines for authors on text referencing

This is acceptable as per instructions for authors given for BMC.

11. Avoid the terms females and males rather than use female students-this is throughout the text

Changes made as indicated by the reviewer.

12. Line 255-273 reads like a thesis. The authors may want to write more positively in the sense of how dealt with these limitations or what the future research needs to take into consideration

We added the following statement on line 327 to highlight how we dealt with limitations “However, since pain has been described as a subjective phenomenon, subjective recall
has been regarded as the most valid way to assess pain [13]. In addition, to assist the respondents in understanding the anatomical region of the lower back, a mannequin was used with an arrow pointing to a posterior view of the lumber region".

13. Delete list if Figure Legends page 32

List deleted as suggested by the reviewer.
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2. Reviewer’s report

Title: Prevalence and associated characteristics of recurrent non-specific low back pain in Zimbabwean adolescents: A cross-sectional study

Version: 3 Date 15 September 2004

Reviewer: Nicholas Henschke

Major Revisions

1. Background (line 10): the sentence that “prevalence of NSLBP in the young population has increased dramatically” is incorrect. While the amount of research highlighting the prevalence in this age group has increased, the prevalence has not shown to be increasing. Please revise this sentence, or provide references to studies showing an increase in prevalence.

   The reviewer is correct. We have revised the sentence. We have now included this sentence in the background section on line 6 “Over the last two decades, research highlighting the prevalence of NSLBP in the young population has increased as evidenced by substantial literature published on the matter”. This statement has been suggested by the reviewer.

2. Table 1 is not exhaustive list of prevalence studies for NSLBP and does not add to the rationale for the study. It should be removed and replaced with a reference to a systematic review of prevalence such as Hoy et al.

   Table 1 was removed as suggested by the reviewer.

3. Questionnaire: despite reporting that one limitation of previous prevalence studies is the use of variable methods to assess prevalence, the authors do not provide any information as to questions asked in the current study. There should be a description of the questions asked, or the questionnaire provided as an appendix.

   A description of the questions asked was given. The authors have now included a new sub-section entitled “low back pain questionnaire” starting on line 65.

4. Results: the age of onset of NSLBP is reported however it is unclear how this was obtained. What question was asked here to determine the age of onset?

   We have clarified this in the methods section under sub-section entitled “low back pain questionnaire” line 77. The absolute question used to obtain age of onset of the reported lifetime prevalence of non-specific was included.

5. Results: The results for health seeking behaviour are unclear. It is reported that there is a significant difference in pain intensity among those who sought treatment and those who did not. However, figure 4 appears not to show any significant difference. Please clarify these results and explain how the health seeking behaviour was assessed.

   The authors have clarified the determination of health seeking behaviour, as was expressed in the questionnaire. Line 88 now reads “The health-seeking behaviour for adolescents with recurrent NSLBP was also ascertained by the following question “Have
you ever sought treatment for recurrent non-specific low back pain or information from any health-care professional regarding the pain that you frequently feel? This behaviour described seeking formal or informal health care services for recurrent NSLBP. According to the student’s t-test, there was a significant difference in the mean intensity of recurrent NSLBP reported by adolescents who sought treatment compared to those who did not \( t (151) = -5.03, p < 0.001 \) (line 204). However, figure 4 was removed as it was confusing to the reviewers.

6. Discussion: While the prevalence results are comparable to one or two previous studies in other countries, it is not clear what this section adds to the manuscript. The discussion should focus on the strengths and limitations of the methodology rather than the estimates obtained. Where comparisons are made to previous literature, these should be comparable in terms of methodology used. The authors should revise the discussion and rather than use other literature to support the findings of their study, they should be more critical as to the nature of their findings of their study in such a small sample size. Their conclusions regarding recurrent NSLBP are based on a sample of 153 adolescents and cannot be considered to be a large study with regard to identifying population prevalence estimates.

Changes suggested by the reviewer were incorporated into the revised discussion. The discussion, however, still focused on comparing the present study results on lifetime or recurrent prevalence with other studies to put into perspective the magnitude of the problem in Zimbabwe with respect to other countries. Nevertheless, the revised discussion emphasised also on the strengths and limitations of the methodology used as suggested by the reviewer.

Line 223 of the discussion section has been added as an attempt to be more critical of the nature of our findings and others in literature. It reads as “However, this study could be criticised on the grounds that the authors left the judgment of NSLBP to the respondents. In addition, the sample was relatively younger compared to the present study sample. Nevertheless, the possibility that lifetime prevalence could have been under-reported in the present study cannot be over-ruled. This is because the screening question on lifetime prevalence had a kappa coefficient of 0.72 indicating substantial but not perfect agreement. This finding highlights the possible existence of recall bias secondary to a phenomenon described as memory decay. Hence, it is possible adolescents forget past lifetime episodes of NSLBP resulting in under-estimated prevalence figures”.

Line 263 of the discussion has been added and it reads “The seemingly high prevalence of recurrent NSLBP in Zimbabwean adolescents could be attributed to several factors. Recurrent NSLBP may indeed be a problem of concern among Zimbabwean adolescents. Since the study had no monetary benefits, it is highly unlikely that the school-children exaggerated or reported a non-existent problem. However, due to the relatively small sample size used (n=153) and the cross-sectional nature of the study, it is possible that the 12-month prevalence of recurrent NSLBP could have been overestimated through a concept of forward telescoping [34]. In addition, the reliability of the screening question for recurrent NSLBP showed moderate agreement (k= 0.51) between the test and re-test responses”. Highlighted in this paragraph is the small sample used which could influence the reporting and generalisations of the results.
Lastly, the last subsection under discussion line 311 on critical assessment of the study highlighted some of the limitation of the study.

7. The conclusion (also in the Abstract) that there is need to implement spinal health educational programmes in schools to improve awareness of the condition is not supported by any of the data provided in this study and should be removed. There is no evidence to suggest that spinal educational programs will reduce the prevalence of NSLBP. Similarly, the conclusion that NSLBP is unlikely to lead to severe disability in the majority of adolescents is also not supported by any of the data in the study. This should be removed and the conclusions revised.

The conclusion has been changed as suggested by the reviewer. The conclusion on the need to implement spinal educational programmes has been removed. In addition, the conclusion on NSLBP leading to severe disability has been removed as it is not supported by data in the study.

8. Table 2: it is unclear from the methods how these Kappa values were obtained. There is no information regarding the sample and the methodology used to assess the reliability of the questionnaire. Please include this information or remove this table.

We have clarified this in line 106. The statement reads as “Subsequently, the questionnaire was administered twice to a sample of 40 students twice. During the initial test, students were not told that they would be re-tested again after one week. For the primary outcome measures of NSLBP, moderate to substantial kappa coefficients (0.48-0.72) were observed (Table 1). These results were consistent with findings from other studies [29,32].