Author’s response to reviews

Title: Is health related quality of life associated with the risk of low-energy wrist fracture? A case control study.

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Author’s response to reviews: see over
Dear Editor,

Thank you for the comments and the positive attitude to our manuscript. We hereby submit the revised manuscript “Is health-related quality of life associated with the risk of low-energy wrist fracture? A case–control study”. We have to the best of our abilities made changes in the manuscript according to the issues raised in the comments from the reviewer.

Yours sincerely

Gudrun Rohde, RN, PhD-student
REPLY TO REVIEWER

We thank the reviewer for helpful and thorough comments, and the positive attitude to our manuscript. We have revised the text according to the reviewers’ suggestions as explained below.

Please revise the "Background section “ of the Abstract to include some context information in addition to aims.

Reply: Context information has been added in the Abstract.

Reviewer 1.

Introduction
The hypothesis for a relation between poor health related quality of life measures and increased risk of (specifically) wrist fracture is not well described.

Reply: The relationship between health related quality of life and increased risk of wrist fracture has been further described in the introduction p.5 and p.6, in para 2 and 3.

Greater details should be provided as to defining health related quality of life, how it is measured, and its hypothesized relation to fracture of the wrist.

Reply: Health related quality of life is defined (p.4, para 3). Furthermore, we have described how HRQOL may be measured.

Some background on known risk factors for wrist fracture should be provided in the introduction. This should be presented making a case for why a summary measure of self-reported general health, pain, emotional and physical function, mental health, etc. would increase an individual’s risk for fracture of the wrist.

Reply: Some background on known risk factors for low-energy fractures in general has been added. We have tried to show why dimensions included in Short Form 36 could be associated with the risk of wrist fracture (p.4, para 2 and para 3).
The significance of the study needs to be described.

Reply: The introduction has been re-written and the significance of the study is hopefully made more explicit.

Methods
Description of participants should be described using a section (or paragraph) on cases and a second section/paragraph on controls

Reply: The description of cases and controls are now in separate paragraphs (p.5 and p.6).

Results for recruitment should perhaps be given separately in the section, “Results”, since the text describing ascertainment of cases and controls, recruitment methods, inclusion/exclusion criteria, participation, etc. is very confusing with the recruitment results interspersed throughout.

Reply: This section has been re-organized. The characteristics of the patients included in the study, the excluded patients and the patients unwilling to participate in the study are no given in the first paragraph of the result section (p.9). Furthermore, the section has been shortened.

The choice of covariates needs to be justified. Why were these variables included?

Reply: With regard to the choice of covariates, most of the factors/covariates have shown to be associated with the risk of low-energy fractures, and were thereby potential risk factors retaining in the final model (p.8, para.3).

The methods used to ascertain demographic and clinical variables need to be described.

Reply: These methods have been described, p.8, para4.

If poor health related quality of life measures are thought to increase risk of hip (?) fracture, this could be through an increase in risk of falls or decreased BMD (or other mechanism). It is not clear how BMD will be treated in the analysis. Will lumbar spine BMD be treated as a
potential confounder? If it were the casual pathway, it would not be appropriate to adjust for it.

Reply: In the introduction p 4, para 2 and 3, we have emphasized that HRQOL might cover self-reported health aspects or dimensions of disease/illness which are not captured by other objective measures. To explore if the dimension included in HRQOL are independently associated with the risk of wrist fracture, other strong risk factors (like BMD) need to be adjusted for (p. 8, pare 3).

Greater detail should be provided regarding the constructs measured by the SF-36. This section is written poorly, for example, “role limitation Physical (four items)” – what does this mean? While the SF-36 is a well-validated instrument, its application to measuring risk factors for wrist fracture is not clear. The validity of presenting each of the domains individually (with respect to risk for wrist fracture) also needs to be justified and explained.

Reply: The section has been rewritten (p 7, para. 3). Furthermore, the validity of presenting each domains individually have been explained in the introduction (p.4 and5, para.3), and in the method section (p.8, para. 3).

The statistical analyses section is difficult for the reader to follow. The grammar and organization need to be significantly improved.

Reply: The statistical analyses section has been re-organized, and hopefully this section is more organized.

Odds ratios should be presented throughout (including Table 2) rather than “effect size” (which should not be abbreviated). The clinical significance of the differences between cases and control in each domain needs to be provided. For example, what does a 10+point (in “role limitation physical”) or 2-point difference (in bodily pain) mean for any of the domain?

Reply: Odds ratios have been presented instead of effect sizes, including table 2. Furthermore the clinical significance of the differences between cases and controls has been provided, p10, the first line. [1]
Similarly, what is the unit for the SF-36 domains in Table 3? Assuming it is 1 unit increase for each of the 1-100 scales, what is the meaning of a 0.99 decreased odds of fracture per 1 unit increase in physical limitation). The unit provided may not be the most interpretable for readers. Whatever unit is given however, some explanation for the meaning of the magnitude of effect needs to be described.

*Reply. The SF-36 domains have been divided by a factor of 10 to estimate the OR. Furthermore, has a footnote been used to explain the significance of the OR units. An example has also been added (p.8, para 2).*

**Discussion**

Overall, the discussion covers a lot of territory but is very difficult to follow. Careful editing and better organization will improve the discussion.

*Reply: The discussion section has been edited and re-organized.*

The discussion of age as a risk factor does not seem appropriate since it was a matching factor in the design.

*Reply: We agree and the paragraph has been rewritten.*
Reviewer 2

The authors had a clear question to explore whether people with low trauma wrist fractures had poor quality of life pre-fracture. They drew on a cohort of cases from their Osteoporosis fracture prevention service and argued that the majority of wrist fractures in that community would have presented to their hospital. This seems reasonable but in the end they only got 56% of the possible fracture patients to complete the SF-36 – they do discuss this as a limitation. They chose to draw controls the same population-base in the community. They acknowledged that they could have compared to national norms and that these were different to their control population norms.

Reply: The results for recruitment of patients have been edited, and we hope that this has made it clearer why some of the patients were excluded from the study (p10, para 3 and p.11). In addition the discussion section is re-organized, and the reason for why 56% of the patients were included in the study (p.14, para 1) is highlighted.

The major limitations still remains that it was a retrospective analyses of QOL and the risk of recall bias is very high with fracture subjects being more likely to be influenced by the events than the controls. However there is no final conclusion about QOL and it would be more interesting to follow the patients to see what happens to QOL after the fracture as well.

Reply: A final conclusion about QOL has been added in the beginning of the discussion section p.13, para. 2, and in the end of the abstract. The focus in this study is HRQOL as a potential risk factor of wrist fracture and thereby HRQOL prior to fracture. Furthermore, comparison between HRQOL prior to fracture and HQOL after the fracture is focused in another paper.

Minor essential: I think that the risk of this bias should be stressed further.

Reply: The risk of recall bias has been stressed further p. 13, para. 2

Discretionary: and that identifying the effects of fracture on QOL may be useful for assessments of the impact of the condition and analysis of effectiveness of prevention strategies like their OP service.
Reference List