Reviewer's report

Title: The prevalence of pain and disability one year post fracture of the distal radius in a UK population: A cross sectional survey

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Reviewer: Heinz G. Endres

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As requested, I am pleased to provide the following brief opinion on the statistical analysis used in this manuscript.

The authors’ primary outcome appears to be prevalence of pain of any intensity at one year post fracture of the distal radius (FDR), which has not previously studied in the UK. Since the literature for the USA gives a prevalence of 30%, the authors use this number to calculate their sample size. The result of 80 study participants (based on the binomial function) required to obtain a 95% confidence interval of +/- 10% around a prevalence estimate of 30% (i.e. 20-40%) is correct.

However, it must be borne in mind that the confidence interval calculated in this way applies only to the population of study participants, and therefore is not valid for the general population. This prevalence estimate with its 95% confidence interval cannot be applied to the population of non-participants!

In “Study design und settings”, the authors report that from October 2005 to February 2006, 264 potential research subjects were identified in the Nottingham area. If all 264 of these subjects met the inclusion criteria, this should be stated more clearly by the authors at this point.

If all 264 potential research subjects met the inclusion criteria, then all of them should also have taken part in the survey in order for the authors to be able to give a prevalence for this Nottingham population (although the 95% confidence interval would then be considerably smaller).

The authors are clearly aware of the problems associated with responder bias, and address this issue at various points in the discussion. However, the fact that the pain prevalences in two studies happen to correspond does not necessarily justify the conclusion that this correspondence “reduces the potential of responder bias as an explanation for the results in the present study” (pp. 12-13)

Further down on p. 13, the authors emphasize the role of responder bias to explain differences in disability. These two interpretations – one emphasizing the importance of responder bias and the other downplaying it – are of course purely speculative.

Instead of the numerous bivariate analyses, I would prefer a logistic regression, which could have as its target variable “poor outcome one year after injury”. This would result in a much-needed adjustment of the odds ratios to the numerous
possible influencing variables (age >/=65, working status, medication usage, etc.)

Finally I would recommend that the tables show not only percentages but also the associated numbers of cases (n). This would make it possible to verify some of the results (e.g. odds ratios), and to recognize missings. For example, according to the tables only 62% of the subjects have some degree of pain, and not 63% as stated on p. 11 – is this because there are 1% missings?

**Level of interest:** An article whose findings are important to those with closely related research interests

**Quality of written English:** Acceptable

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

**Declaration of competing interests:**

I declare that I have no competing interests