**Reviewer's report**

**Title:** Health-related quality of life in diabetic patients and controls without diabetes in refugee camps in Gaza strip: a case control study

**Version:** 1  **Date:** 18 May 2006

**Reviewer:** Ken Redekop

**Reviewer's report:**

**Major Compulsory Revisions**

**Limited generalisability?**

The diabetes patients approached for inclusion into this study represented a random sample of patients treated at a UNRWA clinic and the response rate was 88%. The authors note in the discussion that there is no problem with internal validity but a possible limitation in the generalisability (presumably to all diabetes patients in refugee camps in the Gaza Strip). However, the article’s title and the stated objectives refer to diabetic patients in refugees camps in the Gaza Strip. What are the possible differences between diabetes patients treated at a UNRWA clinic and other diabetes patients in refugee camps? Is it possible that diabetes patients treated at these clinics are “sicker” than other patients (i.e., greater need for medical care, greater frequency of patients requiring diabetes medications [oral and/or insulin], increased frequency of complications)? If so, to what extent could this explain the much poorer HRQOL amongst diabetic patients?

**No comorbidity in the study groups**

One exclusion criterion for both study groups was having any co-morbidity clearly unrelated to diabetes. The results of this study therefore pertain to the impact of diabetes on HRQOL amongst people with no comorbidity. This exclusion probably explains (at least in part) why age showed no effect on HRQOL in the study, also in the control group.

**Differences in characteristics between study groups**

The statistical tests used to compare the characteristics (shown in Table 1) are not completely described on page 5, unless chi-square analyses were always performed. Interestingly, these results show several differences (education, marital status, income, employment, and number financial dependants), and while these findings are mentioned in the results they are not discussed. Although the method used to compose the control group could have resulted in the group of persons with a similar demographic & socioeconomic profile, the differences found here suggest that this kind of bias is not present. In fact, these differences might be reflective of the factors associated with the occurrence of diabetes (e.g., lower education).

**Multivariate vs. multivariable**

The authors should explain why they chose to analyse all domains in one single multivariate analysis instead of four multivariable analyses (one per domain).

**Approach to interaction terms**

When I look at Table 3, I don’t see exactly what I would want to see. That is, in my opinion, the second study objective, besides the aim to examine the HRQOL of diabetic patients (bottom of page 3) (and to determine the average impact of diabetes on HRQOL), is to ascertain whether the impact of diabetes on HRQOL is the same for everybody. In Table 3, I see estimates of the difference in HRQOL between a reference group (such as females in the control group) and other persons (such as males in the diabetes group). What I would like to see is whether the difference in HRQOL between women with diabetes and women without diabetes is similar to the difference in HRQOL between men with diabetes and men without diabetes. Take for example physical health. The difference in HRQOL for women is stated: -45.2. The difference for men is (-36.1)-(-0.5) = -35.6. What I can’t see here is whether the difference between -45.2 and -35.6 is statistically significant. Moreover, if I look at the results for the four domains, I see that the CIs for the HRQOL amongst men and women with diabetes overlap. Based on that observation, I then wonder whether there is in fact a (statistically) significant difference in impact of diabetes on HRQOL between men and women. Is the conclusion simply based on comparisons of the mean reductions? Am I overlooking something here?

(These comments regarding significance also hold true for the analyses of the age groups.)
Incidentally, in our study of HRQOL amongst diabetes patients using the EQ-5D (reference 4 in the article), we found that women reported a lower HRQOL than men. The results of this study correspond with our observations as well as observations by others (Rubin & Peyrot, Diab Metab Res Rev 1999, Review of QOL and diabetes).

Minor Essential Revisions
In epidemiology, the case-control study refers to a study aimed at estimating the strength of association between possible determinants and the risk of disease. In this study, it is the association between diabetes and quality of life that is being studied. Unless the authors have good reasons to choose this term in the title, I would strongly suggest that the reference to case-control be changed to something else.

Minor comment: The grammar is generally quite good; some improvement could be made here and there. Please recheck or ask a native English speaker to look at it.

What next?: Unable to decide on acceptance or rejection until the authors have responded to the major compulsory revisions

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

Quality of written English: Needs some language corrections before being published

Statistical review: No

Declaration of competing interests:
I declare that I have no competing interests