Reviewer’s report

Title: The relationship between a less gender-stereotypical parenthood and alcohol-related care and death: a registry study of Swedish mothers and fathers

Version: 2 Date: 11 June 2008

Reviewer: Amanda Nicholson

Reviewer’s report:

Thank you for submitting your revised paper and for your constructive response to my comments. I think the paper is clearer now and many of my points have been resolved. The language is, with a few exceptions listed below, now easier to follow. I do however now have some serious concerns about the biases which may have been introduced by your exclusions. I regret if my comment that sequential adjustment should be presented on the same participants led you to exclude a large number of participants.

Major compulsory revisions

1. Exclusions and changes to results

The results in the revised paper for women are considerably different from the first version of the paper. Women who work part-time no longer have a significant protective effect and the risk associated with full-time work has increased from 0.85 [0.72-1.01] to 1.47 [1.03-2.11]. The results in men are more similar between versions.

Two groups have been excluded in this version of the paper:

1. Men and women who had subsequent child in 1979 -1980. From p12 this appears to be 5,000 + couples. The number excluded should be stated more explicitly. This was at my suggestion and I think this is important. It would be helpful to know the main results with just this group excluded.

2. Men and women with missing data on confounders on themselves or their partner. This has led to 7,000 + men and 16,000+ women being excluded, due mainly to missing type of work. I suspect this may be accounting for the change in results. The authors state that men and women with missing type of work had higher risk of alcohol-related harm & lower income.

What is the distribution by working hours of women/ parental leave in men without type of work? Are they excluded disproportionately from women (& men) not working? I wonder whether mother who are not working but at risk of alcohol-related harm are being excluded thus exaggerating the observed risk in working fill-time and reducing the protective effect of part-time working. It is not clear in the paper how type of work is completed for participants who are not working. Is there a marker for not working which could be used in place of the missing data?
The authors should consider whether this variable (type of work) is really needed as a control variable. I continue to have concerns that adjustment for 58 different types of work is likely to lead to problems with statistical models, even in this large sample. In fact, adjustment for type of work has very little effect in either men or women (between models II & III). If the authors wish to keep the adjustment in they should present results without this group excluded and discuss any differences in results.

The authors have also added a section on p8 explaining that there is also likely to be a selection bias introduced in women due to missing data on the main exposure (hours of work) which may be similar to those missing type of work. Women excluded due to not having working hours recorded have lower incomes and higher rates of alcohol-related harm. Some sensitivity analyses/estimates to assess how these women may be distributed and what effect their inclusion would have had on results would be helpful. A discussion of these biases and any likely distortion of result should be discussed in full in the Discussion rather than in the Methods/Results section. The existing section at the end of Methods mentions comprehensiveness of the data - but the real issue is bias not generalisability.

If my understanding is correct, the type of work & hours of work come from the census. Are these missing data due to women who did not complete the census at all, linkage problems between datasets or did women miss out these questions on the census questionnaire? Your added section about the accuracy of census data (p22) does not really deal with issue of linkage between datasets and its reliability. Missing data on type of work is much higher than 2.5%!

2. Part-time working in women
Statements in Abstract, Results (p15), Discussion (p17-18)& Conclusion about the benefits of part-time working are no longer sustained by the data, which show a non-significant benefit of longer part-time work.

3. Stratification
These analyses are interesting in the context of role theory and might identify groups more likely to be affected by stress/ expansion but this is not fully explored in the paper. This could be explained more clearly in the Methods/Discussion. Effects in women stratified by cohabitation and whether father took parental leave are most relevant in this context. The apparently contradictory results between men and women with being born outside Sweden should be addressed in the Discussion or, if space is limited, not included in the paper.

Minor essential revisions
4. References appeared with A1(bottom & top p3) – A3
5. Line1 p 3 – all over the world

Discretionary
6. The background is now long, due to the addition of sections on role & caring theory. I wonder whether this could be shortened or moved in part to the Discussion.

7. The section on reverse causality in the Discussion could be reduced in length.

8. Abstract: background: line 3 convergence in alcohol-related harm rather than consumption ??

9. line 7 p16 – Suggested rewording: As regarding females, the results seem to be consistent across age groups & cohabitation status

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

**Quality of written English:** Acceptable

**Statistical review:** Yes, and I have assessed the statistics in my report.

**Declaration of competing interests:**

I declare that I have no competing interests.