Reviewer's report

Title: Marital history, health and mortality among older men and women in England and Wales

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Reviewer: B Modin

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'Marital history, health and mortality among older men and women in England and Wales'
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Research article

Overall, this is a well-written paper addressing an important research topic, namely the role of marital history for old age mortality and self-rated long-term illness. However, because of the ambitious way in which marital history is categorized (12 categories) and analyzed (3 outcome variables for men and women separately + an additional table where parity is also considered) the paper becomes somewhat long-winding and repetitive to read. Below, I have a few suggestions on how to make the manuscript a little bit more “reader-friendly” and focused. I also raise a number of other viewpoints and comments on the manuscript.

Method section

1) Why not use a time-varying covariate for the full period of 1971-2001 in the mortality analyses instead? By expanding the data so that each marital status entered by an individual constitutes one observation in the dataset, 4 different states (observations) stemming from one person could be constructed: (1) never married, (2) being married (first time), (3) being divorced or widowed and (4) being remarried. After that, further sub-categories could be discerned (e.g. years during 1971-2001 of (1) being single, (2) being first-time married, (3) being recently divorced, (4) being divorced since xx years, (5) being recently widowed, (6) being widowed since xx years, (7) remarried after divorce, (8) remarried after widowhood).

Beside being a more precise method of targeting the research question, this would also allow changes in marital statuses between 1991 and 2001 to be taken into consideration in the mortality analyses. As it is now, the end-point for marital status documentation is in 1991, and it is thus likely that some of the studied subjects changed marital status before their death. During the mortality follow-up (1991-2001), the studied subjects are between 60-89 years, and the most likely event during this period is probably to go from married to widowed (and probably
more so among women than among men). In order to create the time at risk for each category of marital status, however, the “exact year” of each marital event would have to be imputated in-between the three decades in question (for an example of how to do this, see Modin, 2003).

2) Regardless of whether one uses a time-varying covariate or not, multiple remarriages will end up in the “remarriage-categories” in the analyses, with a logical overrepresentation in the “long-term remarriage” category. This could thus be a possible explanation behind the elevated mortality risk found in this group (which otherwise seems like a strange finding). The same can be said for multiple divorces. This should be pointed out in the manuscript, and preferably also taken into account when interpreting the results.

3) Considering the strong attenuation of the studied associations when social class was adjusted for, it might be clarifying also to test for a possible interaction between marital status and social class on health outcome. Social class has been shown to interact with marital status in its influence on mortality in Sweden (Hemström, 1996) and Finland (Koskenvuo et al, 1978), demonstrating a much larger variation in mortality by social class among unmarried than married men.

4) A more careful account of how long-term illness (in 1991 and 2001) is assessed is needed? How was the question (and the response alternatives) formulated?

5) Social class was entered as a continuous variable in the analyses. Were there even intervals on the six different categories’ effect on mortality? Being unemployed at both time-points could be an indicator of poor health (and I suspect, therefore, that this group causes a leverage in SES-effects on health and mortality in the analyses).

6) Would it be possible to control for some kind of “health at baseline” (1971)? This would strengthen the argumentation that it is, indeed, marital status in itself (rather than health-related selection into the unmarried statuses) that is related to health and mortality.

8) The assumption that women who were never married in 1971 (ages 40-59) were childless is a considerable drawback with the parity-measure. This means that childlessness was imposed by the researchers on practically all of the never married women (since it is very rare that women have children after 40 years of age). See further points 12-13 below.

Results

9) This part of the manuscript becomes tedious to read after a while – there are too many categories of marital status to keep track of, and when it comes to it (as the authors themselves note), not much additional info is gained by dividing this variable into so many sub-categories. One of the finding revealed by using these sub-categories was that those who had been remarried for a long time had worse health than the more recently remarried, As pointed out under point 2, this finding is difficult to understand (and there is no discussion in the paper about what this
might reflect). One possible explanation might be that a selected group of individuals having gone through "multiple divorces" and multiple remarriages" are overrepresented in this category.

10) I miss a section where the findings from the different analyses (by gender and outcome) are systematically compared with each other. This is of course not possible to do for each and every estimate, but some contrasts are more interesting to note than others, for example:

(1) Male-female mortality difference among never married and widowed, (2) difference in mortality and long-term illness among women in their first marriage since 1971, (3) gender difference in what happens for "all divorced" and "never married" between model 1 and 2 – it is also interesting to note how these differences vary according the three health outcomes.

11) Table 5: It is not clear to me whether it is the same sample of females that are analyzed here as in Tables 2-4. At least for “long-term illness 2001”, the reported number of deaths in the sample differs from those presented for females in Table 4. This means that the altered estimates in Table 5 vis-à-vis those in Tables 2-4 cannot be interpreted as caused by the adjustment for parity since any such change could just as well have been caused by the loss of study subjects who lacked info about parity. It is thus, firstly, important that it is clear from each table how many study subjects that were included in each of the analyses, and secondly, that the models for each gender-specific outcome are based on the same number of study subjects.

12) All interpretations of what happens to the never-married women when parity is adjusted for should be removed from the text, since the authors imputed childlessness on practically all these women (see point 8 above).

Discussion

13) On p. 15, the authors make a case of what happens to the never married women (for whom they had imputed childlessness for almost all) when parity is adjusted for, e.g.: “Results for the never-married showed a divergence between associations with mortality and with long-term illness and also indicated the importance of considering parity.” This statement rests on an extremely poor foundation (see point 8 and 12).

14) The authors need to interpret their finding in the Discussion section instead of just summarizing the Result section. What do the findings of the study suggest, and which possible mechanisms could be at play? The findings should be related to previous research in a way that clarifies for the reader whether they confirm or contradict previous findings.

15) The authors could speculate a little bit more in the Discussion section (the role of alcohol, homo-sexuality, marginalized life-style among never-married in this particular cohort, a little bit more about the cross-over among the never married in Table 4 (e.g. is it a selected group of healthy survivors?) The study subjects were born in 1912-31, so the norms surrounding matrimony for this
cohort probably differed dramatically from those of more recent cohorts. It would be good to have some reflections upon this in the Discussion section as well. For example, women who remained unmarried throughout life are likely to differ from the corresponding group of men, since educational and occupational careers was not easy to combine with family life in those days (e.g. remaining unmarried might have been a deliberate career-choice that women, but not men, had to take). Moreover, since it is generally the- usually younger and healthier - wife who cares for her husband during old age, the finding that widowed men seem to have a higher mortality risk than the corresponding women (Table 2) could be due to the fact that this generation of men tends to become more “helpless” and lost when their spouse dies. These kinds of “speculations” would make the discussion more interesting and worthwhile for the reader, I think.

Nitty-gritty

16) Present number of participants in Abstract
17) P. 2 (abstract): It says that subjects are 60-79 in 1971 (should be in 1991)
18) p. 12-14: when referring to “raised mortality” in certain marital groups, this need to be related to the reference group in the text.
19) Table 2: The estimate for “all widowed” men seems to be wrong? This estimate is higher than each of the three sub-categories of widowhood higher up in the table….

Level of interest: An article of importance in its field

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