Author's response to reviews

Title: Marital status and ischaemic heart disease incidence and mortality in women: a prospective study

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Author's response to reviews: see over
Dear Dr Denyer,

Thank you and the reviewers for your helpful comments and suggestions to improve our paper. You thought that the choice of wording of married/unmarried could be confusing for the reader, so we have taken Michael Murphy’s suggestion and changed the nomenclature to partnered/unpartnered. See tracked changes throughout the document. The vast majority of the women in our study are likely to be married rather than living with a partner, so we have now emphasised that in the methods section on page 4, given the evidence from the General Household Survey 2002. However we agree that calling the other category ‘unmarried’ could be confusing, given that it suggests that the women were never married.

“Marital status at baseline was assessed by asking “Are you currently married or living with a partner?” Those who replied “yes” are referred to as partnered and those who did not are referred to as unpartnered. The unpartnered category thus includes women who were never married, as well as women who were divorced, separated, or widowed. It is likely that the vast majority of the partnered category were married and that a large proportion of the unpartnered category were divorced, separated or widowed, since the General Household Survey for 2002 reported that 71% of women aged 55-64 years old were married, 3% were cohabiting, 4% were single, 13% were divorced or separated and 9% were widowed [22].”

At your suggestion, we have further emphasised in the discussion on page 12, the diverse nature of the unpartnered group.

“Marital status itself was relatively stable during follow-up in this study, but we do not know if the women who were unpartnered at baseline were never married, divorced, separated or widowed, although the 2002 General Household Survey indicated that most would be divorced, separated or widowed [22]. This unpartnered category is therefore diverse and it could be that being divorced or widowed rather than never married places women at higher risk of IHD, but findings from previous cohort studies show little consistency in the associations between IHD mortality and the various non-married states for women [8, 16, 44].”

In most Million Women Study papers we provide a link to our website where all the paper questionnaires are freely available to download. It was an oversight that this was not included in the methods section, which we have now rectified (page 4).
Michael Murphy suggested that another potential mechanism for advantage among partnered women is that they tend to have higher standards of living than unpartnered women. We did refer to greater financial security for married women in the introduction but it is not possible to pursue this further because we did not have information on household income. We have added a comment to this effect in the discussion (page 11).

“It was not possible to adjust further for individual-level measures of deprivation, since information on household income was not collected.”

Michael Murphy also questioned what “living alone” (page 5) referred to. So we have changed the text to make it clear that we are talking here about living arrangements:

“We also compared marital status at baseline with reports of how many people lived in their household 9 years later. Only 12% of women who were partnered at baseline reported 9 years later that they were living alone compared to 79% of the unpartnered women.”

We agree with Stephen Stansfeld that it is a pity that we couldn’t distinguish the various unpartnered states in this analysis. It was not a question of numbers, just that the participants were not asked a question on whether they were never married, divorced, separated or widowed. Therefore it is not possible to perform any sensitivity analysis to look at this further.

The particular aspects of diet that affect the risk of IHD are still being debated and the evidence is not consistent, so we decided not to adjust for dietary factors. The effects of some of these factors will be captured by other adjustments for correlates of diet, including body mass index and smoking status, which are well-established risk factors for IHD.

Although we were unable to directly address the potential mechanisms suggested by the reviewers (standards of living and diet), compliance to medications after IHD could be an additional explanation for the survival benefit for partnered women. We found no difference in medication use after IHD by marital status, and now present this finding in the results and discussion.

We also take Stephen Stansfeld’s point that the measures we have for social interactions are indirect. We have therefore added a sentence on page 11 suggesting other measures which might have had more of an effect.

“However, we cannot exclude possible roles of unmeasured aspects of social support such as the frequency of social contact or the quality of social support.”

We have made a few additional minor edits to the paper where the wording was a bit unclear.

We look forward to hearing from you.

Kind regards,

Sarah Floud