Reviewer's report

Title: Changes over time in the effect of marital status on cancer survival

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Reviewer: Michelle McDowell

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Major Compulsory Revisions

(1) In the Results section, second last paragraph, sentence 5: the percentage point increase for the never-married is written to be 1.9 percent whereas the result from Table 3 suggests it should be 1.8 percent with rounding (1.00178).

(2) In the Results section, second last paragraph, sentence 5: The result for change in the divorced should be reported as not significant rather than as an even weaker indication of change. The non-significant result suggests no change.

(3) The final paragraph of the Results section needs to specify which results are significant for each gender. Increasing excess mortality among elderly widows approached significance for women only and the significant reduction in excess mortality for younger widowers was found for men only.

Minor Essential Revisions

(4) The manuscript would benefit from additional proofreading in terms of correcting some basic grammatical and typographical issues throughout the document and writing in a more active voice.

(5) The authors could provide a clearer description of the datasets used, exclusion criteria, and the criteria for classifying/categorising outcomes (e.g., are deaths from secondary cancers recorded in the registry under the primary cancer diagnosis or are only those deaths attributable to the initial cancer diagnosis included in the analysis?).

(6) Where alternate analyses are described suggesting that the inclusion/exclusion or recategorisation of variables did not influence estimates than this should be stated clearly or any actual effects should be mentioned (e.g., last sentence in the first paragraph of the Methods section states that the inclusion of benign tumours had essentially no impact on estimates; the term essentially suggests that there may have been some impact?).

(7) Graphing odds ratios for mortality over time period may facilitate interpretation of trends over time (e.g., for Table 2).

(8) In connection to the above point (7), graphing of the relationship between cancer-mortality over time for the never-married women would suggest that
mortality declined over the period 1970-1989 and then increased over 1990-2007 rather than remaining roughly the same as described by the authors?

(9) The Background should provide a more solid argument for why the relationship between marital status and cancer-related mortality specifically may have increased. Some of the justifications for the results provided in the Discussion would be better placed in the Background section to justify why cancer-specific mortality would have increased for the unmarried. As previous research has examined trends in all-cause or cause-specific mortality, the focus of the current paper should be on reasons contributing to cancer-specific mortality.

(10) The Background and Discussion should concentrate on explanations that are more specific to cancer-related mortality and on more proximal as opposed to distal causes. For example, issues associated with cancer as a disease (vs. other diseases/illnesses) and treatment regimes (cancer treatments vs. other illness trajectories). Speculation for alternative indirect pathways should be described following evidence for clear causal pathways.

(11) The last line of the fourth paragraph in the Discussion suggests that poorer physical health among the unmarried may be a result of more risky health behaviours, and that social control and economic advantages are two factors behind this behaviour. Evidence for how these factors would influence the behaviour (e.g., citations) and description of the direct causal pathway (see point above) is needed.

(12) Could the authors propose possible moderation or mediation pathways for future research to investigate based on some of the speculations they make as to the relationship between marital status and cancer mortality in the Discussion?

(13) The paragraph on page 11 suggests how selection may contribute to differences in cancer survival as a result of marital status. The section needs further evidence to support the arguments made or a clearer description of how these factors may directly impact on the differences in cancer mortality by marital status. These explanations are too distal and are not linked well together (e.g., how exactly does a person’s knowledge and wage potential affect marital status and cancer survival through health at time of diagnosis or treatment (also, time of diagnosis was controlled for)?)

(14) First sentence of the first paragraph on page 13 is not clear (to the extent that there are marital status differentials in treatment in a wide sense, it is not impossible that these have increased).

(15) The last paragraph on page 13 is too speculative and should be removed.

(16) First paragraph on page 14 contains the sentence “Selection into widowhood is very different”. This sentence should be rephrased as widowhood is not something one can select into.

(17) The first paragraph on page 15 discussing the inclusion of a 14th category
for all other localisations of cancer and its impact on the results is not clear. Do the authors mean that they included a 14th category (which increased the sample size by one third) but this did not change the general pattern of results?

Discretionary Revisions

(18) The use of the term social control to describe the influence of partners on the health behaviours of partners sounds too coercive and would be better described as social support or pressure from partners to undertake better health practices.

(19) When describing the need to control for the localisation of the tumour in the analyses (see second last paragraph of the Method) the authors mention that in the present study relatively many of the malignancies among the divorced are lung cancers. This seems like an interesting finding related to the relationship between cancer mortality and marital status. Could the authors elaborate on this finding and were any other interactions between marital status and cancer characteristics significant? Could these interactions be explored further in analyses over time?

**Level of interest:** An article of importance in its field

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

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

**Declaration of competing interests:**

I declare no competing interests.