Reviewer’s report

Title: Trends in cause-specific mortality by marital status among elderly Norwegian men and women

Version: 1 Date: 4 April 2011

Reviewer: Lucia Galluzzo

Reviewer’s report:

In my opinion this work potentially represents a useful contribution to the literature on the association between marital status and mortality in the elderly, and it might deserve publication because it is one of the few works on this topic that takes into account specific trends in mortality, analyzing also causes of deaths.

In general, the impression one gets from reading the work in its present form is that the premises are quite ambitious. There is a great amount of results shown in Tables and Figures that are very interesting but poorly described and discussed, especially as regards differences in the associations observed over time among non-married subgroups and for various causes of death. Finally, the discussion and conclusions fails to identify one or two focal points to be analyzed in depth, trying to suggest some original hypotheses to explain them, even in the light of the current scientific knowledge.

Although I have some major and several minor reservations about the manuscript as it currently stands, I believe that if these issues are tackled the paper will add to the current scientific knowledge in the field.

- Major Compulsory Revisions

1. The questions posed by the author in the Background section are too redundant in comparison with the following development of the paper. Many issues presented in this section, through an accurate review of the scientific literature, seem irrelevant to the present investigation as they are not directly related to its main findings, remain without an answer and are not discussed or considered in the other sections of the work. The length of this section is disproportionate to the rest of the work (more than 40% of the total). Some of its parts – most of the issues presented in the two Subsections “Possible mechanisms” – could be definitely cut, or just mentioned if specifically related to data at disposal in this study. While those issues related to the main focus of the work and to its findings should deserve a more detailed and extensive analysis in the Discussion and Conclusions sections. For example, it should be interesting to give more relevance to the different trends in cancer (almost stable) and all circulatory disease mortality (definitely increasing) over time. May these differences contribute to hypothesize an explanation for the unsolved problem of
the protective mechanism of marriage, even in consideration of the risk factors for these two groups of diseases?

In conclusion, in order to achieve a better balance between the different sections of the work and to improve the readability and scientific accuracy of the work, I should suggest:

1.a reducing the length of the Background section (as described above);
1.b circumscribing the analysis of causes of death to the main broad categories identified in Table 1 (i.e. all cancers, all circulatory diseases, respiratory diseases, other diseases, external causes), focusing the attention on them, on all cause mortality, and on the relative trends observed in relation to marital status categories in men and women. Consequently, I believe that Figure 3, and charts 3-8 of Figure 4, and the few relative parts of the Results and Discussion where such data are mentioned, are not necessary and must be cut accordingly, in favour of a more detailed discussion of main findings, which are those – except for the lower risk of dying of lung cancer for never married men – correctly summarized in the Subsection Results of the Abstract.
1.c adding the cause-specific results for broad causes of death categories, mentioned in the 3rd Paragraph of the last Subsection of the Results but not shown. This will help to have a clear picture, as in Table 2, of the linear trends in the association between marital status categories, all-cause and cause-specific mortality over the whole time period, with relative 95% confidence intervals and level of statistical significance.
1.d changing the title accordingly in “Trends in total and cause-specific mortality by marital status among elderly Norwegian men and women”.

2. Although methods seem appropriate to the objective of the study, some specific parts are quite confusing and need further explanations:
2.a The education class adopted appear rather misleading (Methods, 3rd paragraph). To my knowledge, compulsory education in Norway was increased to 10 years only in 1997, its previous duration was 7 years from 1889 to 1969, and 9 years from 1969 to 1997. The choice of adopting a category “compulsory education, 10 years” appears unjustified, given that all participants should have attended primary school before 1969. Moreover, the Author writes that “those with missing information on education were grouped together with compulsory education”. How many are they? Why were they included in the first education class? Both issues are particularly relevant if there have been educational inequalities in mortality of elderly subjects in Norway over the study period.
2.b The variable “centrality” should be better defined. On which data is it based? How were they grouped to obtain the 4 classes presented in Table 1?
2.c The description of the Second Procedure used to analyze the trends in mortality by marital status needs to be clarified (Methods, 4th paragraph). Please explain the meaning of the sentence in brackets “(minus 1990 to ease interpretation of the main effects of marital status)” and the choice of year 1990.

3. The decision of highlighting the possible explanations for the weaker
association between marital status and mortality among the elderly, giving them
a central position (Background section of both Abstract and article) is rather
arguable in a work that took into consideration only elderly subjects, without
comparison with younger age groups. So I should suggest cutting the last two
sentences of the Background Subsection of the Abstract, because they are
irrelevant to the study design and findings, and reducing all the part on
explanations of age differences in the Background Section (avoiding to begin the
sentences with “On the one hand, ...”, “On the other hand, ...”).

4. Many recent studies – among which one conducted by our research group:
Scafato E, Galluzzo L, Gandin C, et al. Marital and cohabitation status as
predictors of mortality: a ten-year follow-up of an Italian elderly cohort. Soc Sci
Med 2008;67:1456-1464 – have found that cohabitation status is an independent
predictor of mortality among the elderly and that the magnitude of the association
between increased survival and living with someone is even stronger than being
married. The Author just hints at this important issue in the Subsection
“Limitations” of the Discussion (fourth point), saying that from data at disposal it
wasn’t possible “to identify cohabitants back to 1971...”. The question is not only
who the cohabitants are but whether subjects were living alone or with someone
else. Please explain the decision of not taking into account the association
between mortality and cohabitation status that could be defined also using a
simple dichotomous variable yes/no, usually available from censuses and
registers. To say that “there are few cohabitants in the old age groups in focus” is
rather inaccurate. How many are they? Does the Author refer to all cohabitants
or only to those living with a partner?

- Minor Essential Revisions

5. Background, 1st paragraph: The statement that Manor et al. in the two articles
cited as n. 6 and 7 reported “significant effects only for older women” is arguably
inaccurate. In the first of the two papers (p. 1185) the authors report no gender
differences for marital status and that mortality differentials among women aged
70-89 were generally smaller than those found for the younger age group.

6. Results, title of the 2nd subsection: The last part of the title “Sub-heading for
this section” should be removed.

7. Results, 2nd subsection, 1st paragraph: In the description of results of Figure
2 (all cancers) it should be add that, among men, being never married show an
overall protective effect on cancer mortality (although not statistically significant
in the end of the study period). It should be interesting to discuss this finding also
in the following sections of the work.

8. There is a mistake in the title of the last chart of Figure 5: replace “men” with
“women”.

- Discretionary Revisions

9. As pointed out by the Author in the 1st paragraph of the Subsection “Possible
mechanisms - . Differences in mortality by marital status” of the Background, the association between procreation and mortality has been demonstrated in several studies, highlighting both the beneficial effect of social supports and the positive physiological effect provided by having children. So it should be interesting to include procreation among covariates. Wasn’t it possible to retrieve a simple variable “procreation, yes/no” from the source of information at disposal?

10. Methods, 4th paragraph: Please consider rewording of the sentence “Otherwise, a change in the effect of marital status might have reflected that the effect of some important control variables ... account” which is not very clear.

11. Methods 4th paragraph: At the end of the last sentence, before the reference “[37]” add “among middle aged people in Norway”. Moreover, was a similar trend present also for elderly subjects in Norway?

12. In the Discussion it would be helpful to include some statistics of the distribution of possible confounders (i.e. procreation, cohabitation, socio-economic factors, life habits, etc.) in the elderly population in Norway. Variations over the study period might have influenced the observed associations.

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

Quality of written English: Acceptable

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

Declaration of competing interests:

I declare that I have no competing interests.