Reviewer's report

Title: Divorce and professional care seeking for mental health problems in Europe: a cross-sectional population based study

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Reviewer: Lisa Strohschein

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The purpose of the paper is to evaluate whether there are differences in health care utilization by marital status, and test whether higher utilization among those who are separated/divorced is attributable to age, economic and social resources, and mental and physical health. Using data from the Eurobarometer 64.4, the authors find that those who are divorced or separated use more mental health resources, and that differences persist when controlling for age, economic and social resources, and mental and physical health.

There are several strengths of the paper. First, the arguments are well laid out and the conceptual model is clear. Moreover, the data are adequate for addressing the research questions posed by the authors. The authors also are careful to note the limitations of their work and to discuss their findings as they related to other studies.

However, there are some areas for improvement. I'll divide my comments between those related to substantive issues versus methodological issues. First, the authors don’t do enough to distinguish what their study adds to the existing literature. The Joung et al (1995) article that is cited does not specifically compare differences in the utilization of mental health care services, which is a strength of the current analysis. However, the Prigerson et al (1999) article they cite is longitudinal and is able to show that women who divorced prior to follow up were more likely to increase their use of mental health services relative to women who remained married, even after controlling for physical and mental health. The present study uses both men and women, and the data are based on a European rather than an American population. So, although their data are cross-sectional, the ability to test the association for both men and women for utilization of mental health care services does appear to address a gap in the literature, but the authors have not fully exploited this.

A second substantive problem with the paper is the ways in which the authors attempt to make links that are causal by implying that it is the loss of a partner through divorce that is responsible for differences. Unlike the Prigerson et al (1999) article where causal assumptions are stronger, the authors in the present paper are simply conducting a cross-sectional comparison, where the divorced could have been divorced for as little as one year or as long as many decades. Thus, the authors are overstating their findings. For example one page 14, the authors state: “We expected that after divorce or separation women would
display a higher decrease in mental health care use.” The authors simply haven’t followed respondents over time, nor do they even know how long divorced people in their sample have occupied this role. So, the language about what happens when people get divorced is simply not appropriate because there is no way to whether divorce itself or something else is responsible for differences in mental health care use. I would consider this a major compulsory revision.

I also have questions about the methods the authors use. The authors use multilevel models which is an appropriate technique when the data are clustered. However, the others do not indicate how the data are actually clustered when they talk about the clustered sample design (p. 9): for example, are there multiple respondents within the same household? Importantly, the authors never present the random coefficient for the intercept (only the term for the intercept in the fixed part of the model). What cannot be determined then is whether the authors needed the added complexity of a multilevel poisson regression model. If the coefficient for the random term for the intercept is not significant (in running a model where one has an empty model ie only the fixed and random term for the intercept), then there is no need for a multilevel model (it tells you that there is no significant variation between clusters). So, the authors need to provide evidence that there is indeed a need for a multilevel poisson model, and make clear to the reader what between-cluster differences refer to.

There are also a number of other clarifications that the authors could make in the methods section. In the construction of variables for marital status, who is put into the category of other - those for whom a marital status cannot be determined? In the construction of the education variable, what does ‘those who enjoy no full-time education’ (p. 8) mean? The variable presence of children should indicate that the omitted reference group is having no children in the household under the age of 18. The authors should also indicate the proportion of the sample with missing values on mental and physical health, and difficulty in finding information about mental health problem so the reader can evaluate how much influence imputing the mean value on missing cases has on estimates (p. 9).

Other changes recommended:

page 4: Household, composition should be Household composition
page 4: rather than indicate out of work, should indicate not in the labour market
page 6: acquaintance should be acquaintances
page 11: accept should be except

Level of interest: An article of insufficient interest to warrant publication in a scientific/medical journal

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 that I have no competing interests