LETTERS TO THE EDITOR

Marital status and suicide: some common methodological problems

EDITOR,—In a recent paper published in this Journal, Kposowa1 reported that divorced and separated men had a higher risk of suicide than married men. Men and women of other unmarried status reportedly did not suffer any excess risk of suicide in comparison with their married counterparts. We have some reservation about the findings, and wish to point out two methodological problems that may have affected many studies in this area.

In this study marital status was enumerated at the beginning of the study period. Marital transitions between the baseline survey and death or end of follow up were unknown to the researcher. The author mentioned this issue at the end of the discussion section, but seemed to suggest that the problem would not have seriously affected the findings. The failure to capture marital changes would lead to a misclassification of marital status during the follow up period and at death. The follow up period is from 1979 to 1989. Marital status would have changed during the 11 year period. For example, among the elderly, the married person would become widowed; the never married person among the age group 25–34 would have been married during the period, etc. As such, the hazard ratios based on the initial marital status would be biased towards the null value. One study in the USA and one in the UK2 have emphasised the importance of this misclassification error. Furthermore, there were only 545 suicide cases with a rate of approximately 10.5 per 100 000, which could be very sensitive to the possible misclassification. In this study both the models with and without adjustment for baseline factors contradict previous research results that used the information of the marital status at death.3 We should be very cautious about the insignificant results.

Studies have demonstrated increased psychiatric disturbances in the first year of widowhood.4 Afterwards the widened return to a usual level of psychological health. This strongly supports the plausibility that widowed people may have a higher risk of suicide shortly after the death of a spouse. Widowed persons available at the baseline survey were likely to be those who had survived the initial high risk period. The study design has a selection bias that favours the survivors. Much less is known about changes in psychiatric disturbances in divorces. The same selection bias may exist in divorced people. There has been a considerable number of studies on marital status and suicide since Durkheim’s classic study.5 Unfortunately progress has been made by some common methodological problems that are often overlooked. Our first suggestion is that much more effort should be spent on updating the marital status of participants during a follow up period. A closely related suggestion is to study the suicide risk of participants married at baseline but who become unmarried during the follow up. This will avoid the selection bias in the studies of people divorced or widowed at baseline. Secondly, testing for non-proportionality should be routinely carried out and reported if the analysis is based on the Cox model. There are various methods for the testing, such as allowing break points in a hazard function and the use of Schoenfeld residuals. An initial hazardous effect that diminishes over time is partial evidence for the above mentioned problems. No information on the proportional hazard assumption was given in the paper under discussion.

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Author’s reply: Marital status and suicide: some possible misunderstandings

Yip and Cheung express reservations in their letter about results in a previous report1 that failed to find significant effects of widowhood and being single on suicide. The purpose here is to consider these issues.

As discussed in my article, marital status was measured at baseline. The study was not designed to estimate the effect of marital transitions, but the impact of being in a given marital status at baseline. Marital status is a time varying covariate. While such a variable may change over time, Yip and Cheung exaggerate the impact of this change on mortality.

Table 1 Effect of marital status on suicide by selected years of follow upa

<table>
<thead>
<tr>
<th>Covariate</th>
<th>RR</th>
<th>95% CI</th>
<th>RR</th>
<th>95% CI</th>
<th>RR</th>
<th>95% CI</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>After one year of follow up</td>
<td></td>
<td>After three years of follow up</td>
<td></td>
<td>After four years of follow up</td>
<td></td>
</tr>
<tr>
<td>Married</td>
<td>1.00 (Reference)</td>
<td></td>
<td>1.00 (Reference)</td>
<td></td>
<td>1.00 (Reference)</td>
<td></td>
</tr>
<tr>
<td>Single</td>
<td>1.16</td>
<td>0.39, 3.47</td>
<td>1.18</td>
<td>0.73, 1.90</td>
<td>1.28</td>
<td>0.84, 1.96</td>
</tr>
<tr>
<td>Divorced</td>
<td>2.81**</td>
<td>1.15, 6.92</td>
<td>1.78**</td>
<td>1.12, 2.81</td>
<td>1.85**</td>
<td>1.23, 2.79</td>
</tr>
<tr>
<td>Widowed</td>
<td>1.22</td>
<td>0.32, 4.65</td>
<td>1.08</td>
<td>0.57, 2.05</td>
<td>1.02</td>
<td>0.57, 1.82</td>
</tr>
<tr>
<td>Unknown</td>
<td>—</td>
<td>—</td>
<td>0.61</td>
<td>0.07, 5.02</td>
<td>0.68</td>
<td>0.08, 5.47</td>
</tr>
<tr>
<td>LRS</td>
<td>511.10</td>
<td>2 333.81</td>
<td>3 066.12</td>
<td>471.85**</td>
<td></td>
<td></td>
</tr>
<tr>
<td>degrees of freedom</td>
<td>21</td>
<td>22</td>
<td>22</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Number of suicides</td>
<td>170</td>
<td>230</td>
<td>230</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Number of observations</td>
<td>12 680</td>
<td>17 293</td>
<td></td>
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<td></td>
<td></td>
</tr>
</tbody>
</table>

*Adjusted for age, race, sex, education, income, and region. For the last year of follow up, see Kposowa. LRS = likelihood ratio statistic; **significant at p<0.01.

They failed to report the stunning finding by Cheung that although some respondents changed marital status during follow up, updating marital status did not have any substantive impact on mortality. The American study2 cited by Yip and Cheung in their letter found evidence of misclassification of marital status and suggested this may increase results in hazards models of mortality. However, the study2 used a subsample of the Longitudinal Study of Aging, which by design inevitably increases misclassification. The subsample comprised persons aged 70 years and older. Clearly, if persons in that group change marital status, it is more likely to widowhood. My study consisted of all age groups.

The authors speculate that widowed people may have a higher risk of suicide shortly after the death of a spouse. The fact is that we do not really know, and a similar argument can be made about the divorced as well. Their suicide risk may be especially high after separation.

The first suggestion made by Yip and Cheung is not feasible, as official data were used. The proportionality assumption of the model in my study was tested using methods discussed by Hosmer and Lemeshow.4 As no problems were detected, results were not reported.

Yip and Cheung affirm that an initial hazardous effect that diminishes over time indicates a methodological problem. I agree, but it also depends on how one interprets such an outcome. As shown in table 1, when break points in follow up were allowed, widowhood did not significantly increase suicide risk at baseline.

I am gratified that Yip and Cheung do not question my basic finding on divorce and suicide, but have problems with null results. I stand by my study and conclude that in the data used, widowhood and being single have no significant effects on suicide.

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Neonatal mortality by place of delivery in São Paulo, Brazil

EDITOR,—In São Paulo, Brazil, 0.6% of all births do not occur in a hospital. Although neonatal mortality in home deliveries in the UK is higher than that in hospital deliveries, it is accepted that home births consist of two different groups: those who planned to deliver at home with higher average birth weight and low neonatal mortality, and those who either booked a hospital delivery or did not book, with lower birth weight and extremely high neonatal mortality.1 In São Paulo, home deliveries are not encouraged and there is no policy of booking in the state sector.

As part of a study of neonatal mortality in the city of São Paulo, a case-control study is being undertaken to investigate maternal and hospital risk factors. For the first six months of 1995, birth certificates were identified and linked to all neonatal deaths and a 10% sample of children who survived up to 28 days. Children with birth weight under 500 g were excluded.

The study included 9583 births with known place of birth, of which 64 did not occur in a hospital. In our data, children who were born at home were teenagers (<20 years) and had not completed primary education (eight years). There were no statistically significant differences according to the proportion of low birth weight, prematurity, and whether a father was named in the birth certificate, but the study may not have had enough power (table 1).

The risk associated with delivery not in a hospital was restricted to mothers of lower educational status and much more marked in younger mothers. In Chile, home deliveries presented a higher neonatal mortality and were more frequent in teenagers.2 Campbell et al3 suggests a pattern similar to ours in England and Wales and quotes the example of illegitimate births at home to women aged under 20, who had 5% chance neonatal mortality.

In São Paulo, home deliveries do not occur in a hospital, and the study may not have had enough power (table 1).

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ence of similar uncertainty in the early 1990s, regarding the indications for influenza vaccine. With regard to pneumococcal vaccine, the situation is probably worsened by the fact that the evidence in support of effectiveness in high risk elderly persons is restricted to the prevention of invasive disease (bacteraemia). We also agree that GPs face many issues around appropriate workload and remuneration. Nevertheless, we cannot accept that pneumococcal vaccination is too difficult to incorporate into routine practice. The indications for influenza and pneumococcal vaccines overlap considerably. The UK Department of Health recommends that patients recalled annually for influenza vaccination, are offered pneumococcal vaccine simultaneously (on a “once only” basis and at a different injection site), thereby delivering two preventive measures for the same consultation and administrative costs. We agree that the availability of new conjugate vaccines, which offer the hope of reducing nasopharyngeal carriage, may bring about the need to review policy. However, until these become available, efforts to improve the uptake of polysaccharide pneumococcal vaccine in high risk persons should continue.

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BOOK REVIEW


The contradictory role played by mental health services in society has been progressively described by commentators and different interest groups since the beginning of the 1960s when mental health practice moved from an institutional to an increasingly larger community-based system of care. However, the complexity of the issues related to the acknowledgement and application of civil rights to mental patients on the one hand, and on the other hand, the disparate power and demands of involved parties—that is, relatives, users, professionals and agencies—have often hindered an objective joint outlook. In this sense, the Perkins and Repper book provides a valuable contribution because it entails a comprehensive up to date well documented overview of most of the underlying problems brought to surface by the application of the Community Mental Health Practice. Aimed at the defence of the full citizenship rights of the people with serious mental problems and anchored in this respect, it represents a meeting point for scientific literature, professional expertise and user experience where classically crucial and unresolved issues as the cure concept, the limits between madness and human condition, the interference of professional roles in mental health teams, the duty of patient protection, the treatment priorities, compliance and compulsory detention, or the different view of mental health difficulties in users, relatives and professionals, are thoroughly analysed and further discussed by means of the dilemma of choice or control. Reading this book will surely be of help to any professional in the community mental health field interested in deepening the understanding of the serious demands of mental patients to increase the quality of their service.

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