



Title	Marital status and suicide: Some common methodological problems (multiple letters) [1]
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Citation	Journal Of Epidemiology And Community Health, 2000, v. 54 n. 11, p. 878
Issued Date	2000
URL	http://hdl.handle.net/10722/45230
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Marital status and suicide: some common methodological problems

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J. Epidemiol. Community Health 2000;54:878-
doi:10.1136/jech.54.11.878

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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 did mention 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 USA² and one in the UK³ 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.^{4,5} We should be very cautious about the insignificant results.

Studies have demonstrated increased psychiatric disturbances in the first year of widowhood.^{6,7} Afterwards the widowed 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 divorcees. 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.^{4,5} Unfortunately progress has been prevented 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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- 1 Kposowa AJ. Marital status and suicide in the National Longitudinal Mortality Study. *J Epidemiol Community Health* 2000;54:254-61.
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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 their concerns.

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 up*

Covariate	After one year of follow up		After three years of follow up		After four years of follow up	
	RR	95% CI	RR	95% CI	RR	95% CI
Marital status						
Married	1.00	(Reference)	1.00	(Reference)	1.00	(Reference)
Single	1.16	0.39, 3.47	1.18	0.73, 1.90	1.28	0.84, 1.96
Divorced	2.82**	1.15, 6.92	1.78**	1.12, 2.81	1.85**	1.23, 2.79
Widowed	1.22	0.32, 4.65	1.08	0.57, 2.05	1.02	0.57, 1.82
Unknown	—	—	0.61	0.07, 5.02	0.68	0.08, 5.47
LRS	511.10		2 533.81		3 546.12	
Model χ^2	95.33**		304.44**		471.85**	
degrees of freedom	21		22		22	
Number of suicides	42		170		230	
Number of observations	4109		12 680		17 293	

*Adjusted for age, race, sex, education, income, and region. For the last year of follow up, see Kposowa.¹ LRS = likelihood ratio statistic, **significant at $p < 0.01$.

They failed to report the stunning finding by Cheung² that although some respondents changed marital status during follow up, updating marital status did not have any substantive impact on mortality. The American study³ cited by Yip and Cheung in their letter found evidence of misclassification of marital status, and suggested that this might bias results in hazards models of mortality. However, the study³ 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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- 1 Kposowa AJ. Marital status and suicide in the National Longitudinal Mortality Study. *J Epidemiol Community Health* 2000;54:254-61.
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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 not born in a hospital had an increased risk of neonatal death (odds ratio 1.93 with 95% confidence intervals 1.02, 3.63; $\chi^2=4.88$; $p<0.03$). A significantly higher proportion of mothers of children not born in a hospital 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 al*³ 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 death.⁴ It seems that the risk of neonatal mortality in deliveries not in a hospital is complex and further studies are needed before we can establish when home deliveries are safe in São Paulo.

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Table 1 Distribution of birth weight, gestational age, schooling and age of mother and whether father is named on birth certificate in cases and controls by place of birth. São Paulo, 1995

	Not in hospital		Hospital		OR	95% CI	χ^2	p Value				
	Cases	Controls	Cases	Controls								
	n	%	n	%								
Birth weight <2500 g	11	78.6	6	12.0	823	72.4	694	8.4	1.55	0.53, 4.71	0.74	0.39
Gestational age <37 weeks	8	57.1	4	8.0	645	53.6	441	5.3	1.37	0.37, 5.42	0.26	0.61
Mother's age <20 years	6	42.9	7	14.0	259	22.9	1097	13.8	3.63	1.07, 12.12	6.04	0.01
Mother's schooling <8 years	10	71.4	20	40.0	500	41.5	2932	35.3	2.93	1.27, 6.63	8.34	0.01
Name of father absent	5	35.7	15	30.0	457	38.0	2256	27.1	1.65	0.52, 4.86	0.94	0.33

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Family doctor advice and pneumococcal vaccine uptake

EDITOR.—Kyaw *et al*¹ highlight the importance of advice from general practitioners in the immunisation of patients with pneumococcal vaccine. However, greater knowledge regarding pneumococcal immunisation is required among GPs and doctors in general before patients in the target groups are likely to be vaccinated. I performed a postal questionnaire survey of the awareness of the indications for, and practice of pneumococcal immunisation among GPs in one inner London borough. Questions explored knowledge of the guidelines for pneumococcal immunisation with six real and six fictitious indications, and GPs' immunisation practice. One hundred and fifty six GPs were mailed the questionnaire. The response rate was 56 of 156. The responses are summarised in table 1.

Respondents may represent those GPs interested in immunisation. However, few GPs seem to have a clear understanding of the Department of Health guidelines and some seem to confuse indications with those for influenza and meningococcal vaccines. On the other hand some GPs are aware of the

indications but do not immunise accordingly, possibly because of the workload implications, lack of remuneration for the service or because of doubt regarding the vaccine's efficacy.^{2,3} For some patients there may be uncertainty as to whether hospitals or primary care should be taking the lead. The guidelines may be difficult to integrate into routine practice; current indications vary from those for influenza immunisation and there is a need to avoid re-immunising in most patients. Communication between hospital departments and patients' GPs is important in this regard.

Local immunisation campaigns can be effective but may not be seen as a priority by health authorities. In the future the indications for pneumococcal immunisation will need to be reviewed as conjugate pneumococcal vaccines are further evaluated and licensed.

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Authors' reply

We accept the point made by James that many GPs may be uncertain about the indications for pneumococcal vaccine. Indeed, earlier work we performed demonstrated the exist-

Table 1 Perception of target groups for and practice of pneumococcal immunisation

Pneumococcal immunisation is recommended by the DOH for:	Yes (%)	No (%)	No answer (%)
Adults living in residential or nursing homes	17 (30.3)	31 (55.3)	8 (14.4)
All children under 5 years	1 (1.7)	46 (82.1)	9 (16.2)
All adults over 65 years of age	7 (12.5)	40 (71.4)	9 (16.1)
All adult diabetic patients*	35 (62.5)	15 (26.8)	6 (10.7)
All adult patients with chronic heart or lung disease*	43 (76.8)	8 (14.3)	5 (8.9)
Patients over two years old with chronic renal disease*	43 (76.8)	6 (10.7)	7 (12.5)
Adult patients with hyposplenism from whatever cause*	53 (94.6)	1 (1.7)	2 (3.7)
Adults with HIV infection*	37 (66.1)	11 (19.6)	8 (14.3)
All adults with previous pneumococcal meningitis	6 (10.7)	37 (66.1)	13 (23.2)
Contacts of cases of pneumococcal meningitis	11 (19.6)	33 (58.9)	12 (21.5)
Travellers to areas with a high incidence of pneumonia	5 (8.9)	40 (71.4)	11 (19.7)
Adult patients receiving immunosuppressive drugs*	35 (62.5)	12 (21.4)	9 (16.1)
For which of the following groups is it your current practice to carry out pneumococcal immunisation?			
Splenectomy*	52 (92.8)	0 (0)	4 (7.2)
Hyposplenism from sickle cell disease*	48 (85.7)	2 (3.6)	6 (10.7)
Hyposplenism from other causes, for example, coeliac disease*	36 (64.2)	11 (19.6)	9 (16.2)
Chronic renal disease or nephrotic syndrome*	32 (57.1)	15 (26.7)	9 (16.2)
Chronic heart disease*	21 (37.5)	25 (44.6)	10 (17.9)
Chronic lung disease*	21 (37.5)	24 (42.8)	11 (19.7)
Chronic liver disease including cirrhosis*	19 (33.9)	27 (48.2)	10 (17.9)
Adult diabetic patients*	20 (35.7)	26 (46.4)	10 (17.9)

*Patient group recommended for immunisation by the UK Department of Health.

ence of similar uncertainty in the early 1990s, regarding the indications for influenza vaccine.¹ 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).² 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.³ 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),³ 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.⁴ 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

Dilemmas in community mental health practice: choice or control. By R Perkins, J Repper (Pp 151; price not stated). Oxford: Radcliffe Medical Press, 1998. ISBN 1-85775-181-7.

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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