

## **J-Shape or Linear Relationship Between Alcohol Consumption and Depression: It Matters. A Response to Taylor & Rehm (2005)**

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We thank Benjamin Taylor and Jurgen Rehm (2005) for their useful comments on our paper (Alati *et al.* 2005). Contrary to their concluding remarks we would contend that our paper, as well as their comments, do make a useful contribution to this area of research. Clearly, the same people are responsible both for the J-shaped curve at 5 years and the linear curve at 14 years because it is the same cohort at both stages of the follow-up. We conclude that the association between alcohol consumption and depressive and anxiety symptoms may vary by life stage (Alati *et al.* 2005).

As with most epidemiological studies exploring categorical variables, there will be heterogeneity within categories. Further, there are often problems of misclassification so that to some degree if 'true' measures were available there would be some overlap between categories. However, mean values between different categories will differ, and in general the within-category variation will be less than the between-category variation. While the light-drinking category will include some individuals who, if their true values were known, would overlap in their drinking with some in the abstainers, there will also be some women who should have been classified correctly as moderate drinkers. However, we can see no reason why this measurement error should vary at the different ages and therefore explain the differences at different ages.

It is not appropriate to collapse the abstainer and light drinking categories in our analyses, any more than it would be to collapse any of the other categories. The categories were defined a priori and by collapsing them Taylor & Rehm mask the fact that at the 5-year follow-up symptoms were more prevalent in the abstainers than light drinkers, whereas at the 14-year follow-up symptoms were greater in the light drinkers than the abstainers. These two categories contain sufficient numbers to produce robust statistical inference and do not require collapsing.

As described in the Methods section of the paper, we followed an analytical approach that was relevant to our research question and similar to that used in a similar previous study (Power *et al.* 1998). We excluded participants who were lost to follow-up and therefore did not have appropriate data at all follow-up phases. We were completely open in describing how these women differed from those who were not lost to follow-up so that readers can take this potential limitation into account in their interpretation of the results. Despite clear differences between those included and those lost to follow-up, this would affect our conclusions only if there were an interaction between follow-up and drinking behaviour and the outcome: that is to say, if the association was completely different in those who do not respond. While we cannot rule this out we cannot think of any plausible reason why this might occur.

Regarding Taylor and Rehm's suggestion of imputing the whole data set rather than working on a restricted sample, there is currently much debate about dealing with missing data in prospective cohort studies (Hogan *et al.* 2004). It is not universally accepted that missing values should be imputed, particularly when there are complete missing data at a phase of the study due to participant non-response. We did not feel that it was appropriate to impute large amounts of data in this study.

Finally, we agree with Taylor & Rehm that alcohol drinking patterns and their mental health effects are likely to be culture-specific. As such we would not assume, nor do we imply in our paper, that our results are generalizable to communities where abstention is the norm among women of reproductive age.

## References

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