Reviewer's report

Title: Affinity for risky behaviors following prenatal and early childhood exposure to tetrachloroethylene (PCE)-contaminated drinking water: a retrospective cohort study

Version: 1 Date: 23 May 2011

Reviewer: Frank Bove

Reviewer's report:

Discretionary Revisions:

1. I am not convinced that the inclusion of those who were only exposed during early childhood is that helpful. First of all, there are considerably less in this grouping compared to those exposed both prenatally and early childhood and those unexposed. Because of this, there are very small numbers in this grouping that reported drug use or multiple risky behaviors, resulting in unstable odds ratios and very wide confidence intervals. The instability of the odds ratios is sometimes exacerbated when more than one potential confounder is added to the model - e.g., the odds ratio for ritalin jumps from 0.6 to 1.4. When there are small numbers reporting a behavior, adding more than one potential confounder to the model is likely to produce an inflated odds ratio because of model instability. Another drawback due to relatively small size of this grouping is the inability to evaluate tertiles of exposure. This leads to what I think are misinterpretations of the results for this grouping, in particular for the smoking and alcohol findings. The text states that "no meaningful increases in the risk" of smoking and alcoholic beverage consumption behaviors were observed, yet the odds ratios for "any vs never" exposure for those exposed only during childhood are very similar to the odds ratios for "any vs never" exposure among those exposed both prenatally and in early childhood. One could speculate that if it was possible to evaluate tertiles of exposure among the early childhood exposure group, that similar positive findings would have been observed to those seen in the prenatal/early childhood grouping for smoking and alcohol use. For most of the behaviors under evaluation, the findings for "ever/never" exposure are similar for the "early childhood only" grouping and the prenatal/childhood grouping. Where differences are observed, most often this can be attributed to very small numbers of reported behaviors and unstable odds ratios in the "only early childhood" grouping. Second, after mentioning some of the drug use findings for the early childhood grouping, (some of which are problematic due to wide confidence intervals and the inclusion of too many variables in the model), the text does not discuss this grouping anymore. No conclusions are presented concerning the importance of the findings for this grouping. Did the inclusion of this grouping shed any light on the issue of the effects of the timing of exposure on risky behaviors? The text provides no discussion of this issue. I think the paper would be strengthened by either (1) deleting this grouping or (2) including the grouping but with better interpretation of the findings, and a full discussion of
how the findings for this grouping enhance the understanding of the etiology of these risky behaviors.

2. The discussion should mention that the relatively small size of the "only early childhood exposure" grouping was a limitation.

3. The discussion mentions bias due to under-reporting but does not mention the possibility of bias due to over-reporting. This is curious since the text mentions that the prevalence of the reported behaviors in this study "is similar or higher than" previous surveys conducted in MA. Based on the evidence in the study, I would agree that over-reporting would also likely be non-differential.

Minor Essential Revisions

1. The Janulewicz et al 2008 reference is missing.

By the way, I think this is a very interesting article and well-written!

**Level of interest:** An article of outstanding merit and interest in its field

**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.