Reviewer's report

Title: Determinants of participation in a longitudinal two-stage study of the health consequences of the Chornobyl nuclear power plant accident

Version: 1 Date: 19 December 2007

Reviewer: Denis BARD

Reviewer's report:

This study aimed to explore the determinants of follow-up participation in a sample of mothers and children that have been involved (possibly exposed to ionizing radiations) in the Chornobyl disaster.

Objectives

Major compulsory revisions

Overall, the paper is rather vague. It should be clearly focused on questions such as:

â¢ What are the specificities of attrition predictors in disaster cohorts as compared to other cohort studies, not aimed at exploring disaster consequences?

â¢ For disaster cohorts, what are specificities of attrition predictors for mental health as compared to somatic outcomes?

References, which actually mix very different kind of studies, may be more carefully chosen in this regard and being slightly more extensive.

Methods

Minor revisions

Characteristics of the sample should appear (gender for children, ages for mothers), see results and discussion

Authors state that the evacuees were thus in utero to age 15 months when the accident occurred and constituted a high risk group for thyroid cancer: a reference should be included.

The basis of initial random selection, in particular for classmates, should be mentioned. This is done (phone directory for the follow-up survey) I don't really what could be the ethical constraints on gathering information on non participants: demographic information (age, gender for instance doesn't pose ethical constraints, I think). There is no indication (see above) on whether evacuees classmate controls and population controls were matched for gender. Regarding children well-being, the performance of the index chosen (is it CSI or P-CSI? It is not clear whether it refers to children's somatization inventory (ref 37) or the Stony Brooks one) The index pertinence is in any case not discussed.
Major compulsory revisions

What is the purpose of using a population based control group? What is expected? Again, what about the non respondents in this group? How is standard of living assessed? No justification appears for choosing the parents academic graduation as a measure of parental education.

What is a Chornobyl-related illness, and what makes the difference with having health being affected by the accident? Why choosing having â¥ 2 colds in the past year and also headache or migraine as a variable?

The adjustment for group status (I donâ##t understand on what practical basis this could be done) is essentially unwarranted, since adjusted or unadjusted analyses yield essentially similar results. Other methods used seem to be appropriate but checked by a statistician.

Results and discussion

Minor revisions

There is no indication on refusal rates page 11 for population based controls and no mention on that in figure 1. Page 12, the sentence â##The examined children also had higher self-reported CSI scores than children not brought to the clinicâ## is vague (figures?). In the tables, despite of columns heading indicating OR, some results are expressed differently. Table 4 caption doesnâ##t allow to know what is the population described. Form the text it can be understood that it relates to young adults, not mothers? Results of multivariate analyses should appear in the Tables.

Major compulsory revisions

The fact that evacuee status was not a predictor of participation is indeed intriguing. Possible changes in classmatesâ## status after 8 years should be addressed. Discussion of the discrepancy with Havenaar results is lacking. Results are somewhat over interpreted. For instance, distrust of authorities was a predictor of a lower attendance to physical examination. The authors consider page 13 that this might be linked to the observation quoted from the ref 46 that distrust in authorities was a predictor of perceived danger and psychological distress after the TMI incident. However, no result appears in the paper showing an association between distrust in authorities and say, CSI and/or P-CSI that may provide a ground for comparison.

As the authors state â##all disasters have unique characteristicsâ##, therefore their results can hardly be generalized. A discussion on what could be the requirements to allowing such a generalization is lacking. Despite the unique characteristics of any disaster, it remains that, especially regarding perceived health, the agent involved, here ionizing radiations that are not accessible to sensory experience, may have different implications for constructing post disaster cohorts if the agent is a flood, a visible plume, manifest through smell and so on. As final conclusion, a pint should be made on disaster preparedness, a topic currently widely discussed, see papers on aftermath of WTC disaster.
What next?: Unable to decide on acceptance or rejection until the authors have responded to the major compulsory revisions

Level of interest: An article of limited interest

Quality of written English: Acceptable

Statistical review: Yes, but I do not feel adequately qualified to assess the statistics.