Author's response to reviews

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

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**Version:** 3  **Date:** 13 February 2008

**Author's response to reviews:** see over
Attached is our revised manuscript MS: 1659482507162615 “Determinants of participation in a longitudinal two-stage study of the health consequences of the Chornobyl nuclear power plant accident.” We hope that you find our responses to the critique and the revisions to the manuscript satisfactory. The comments were very helpful, and we feel that the paper is stronger as a result.

Below is a point-by-point response to all of the reviewers. We have edited our manuscript accordingly to address all of these issues.

Thank you very much for the opportunity to resubmit this paper. We look forward to hearing from you in the near future.

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Reviewer #1

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

Version: 1 Date: 3 December 2007

Reviewer: Linda Grievisk

Reviewer's report:

This is an interesting paper that studies the determinants of participation in an Eastern Europe population. Different populations groups (western and non-western) have different reasons to participate. The results are helpful in the future when preparing longitudinal health studies after disasters.

Major compulsory revisions

1. From the data-analyses section and presentation of the results I conclude that the author's assume that the evacuees and (classmate) controls have the same response predictors, i.e. that there is no effect modification whether or not you are evacuated. I conclude this because they are pooling the two groups in the analyses and are adjusting for the group status. However, in this way you can not study effect modification. You loose some additional information that is worth looking at. Additional analyses are needed. Each table should be analyzed for the evacuees and for the controls separately. If the OR's are similar for both groups, you are allowed to pool the analyses. This should be described in the data-analyses section and in the results section. The last column in each table is redundant.

We thank the reviewer for raising this issue. The reason we had presented both unadjusted and adjusted (for group status) odds ratios was to show that the ORs were very similar, and thus that group status did not influence the relationship of the predictors to the attrition variables. In light of the concern of the reviewer, however, we have revised the table to show only the adjusted ORs. Moreover, to demonstrate that the predictors behaved similarly, we examined the interactions between group and each of the predictors and present the significant findings in the text. Actually, there was only one significant interaction effect (p=0.02), namely, that higher somatization scores as reported by the mothers (P-CS) were associated with clinic attendance at time 1 only for...
the classmate controls. As we comment in the Discussion (pg. 14), it is conceivable that this pattern was not observed in evacuee mothers because their children were exposed to potentially harmful toxic agents, and thus did not need this extra incentive to bring their children to the clinic. The alternative approach to examining effect modification suggested by the reviewer would be to show the findings for each group separately. This would mean that the tables would double and triple in size. Given that the effects of the predictors were similar in evacuees and controls, we feel that it is more parsimonious to show the data for the groups combined. Also, the stratified analyses have less power. Again, we are grateful that the reviewer raised this issue, and we feel that our paper is much strengthened by inclusion of the interaction analysis.

2. The analyses of variance are used for the continuous variables. This is a good analysis for descriptive analyses. However, I could argue if this is the best way to study the predictors of follow-up. For the multi-variable regression model, this analysis can not be used. Unclear from the paper is how these continuous variables are treated in the multivariable regression model. I assume that the variables were dichotomized. Why not use the dichotomized variables in the crude logistic regression analyses instead of the analyses of variance?

In the revised version, we use logistic regression consistently for all analyses of both non-continuous and continuous variables. To facilitate interpretation, we standardized the continuous variables in the logistic regression so that their corresponding odds ratios may be interpreted as the odds of participation given a one standard deviation increase in the continuous predictor. The tables and text were changed accordingly. We note here that although dichotomizing continuous variables allows for easier interpretation (especially in logistic regression models), there are no universally agreed upon cut-off points for the predictors included in our paper. Moreover, dichotomizing variables is not always optimal because it can lead to a loss in power and efficiency (e.g., Royston P, Altman DG, Sauerbrei W: Dichotomizing continuous predictors in multiple regression: A bad idea. Statistics in Medicine 2005;25: 127-141; Altman DG, Toyston P: The cost of dichotomising continuous variables. British Medical Journal 2006;332:1216; Babyak MA: What you see may not be what you get: A brief, nontechnical introduction to overfitting in regression-type models. Psychosomatic Medicine 2004;66:441-421).

3. The analyses do not show the multi-variable analyses in the table. This way the reader does not know the independent variables that predict the follow-up. Only the significant predictor is mentioned in the text (unknown is whether statistical significance is meant or a clinical significance). Since the last column in the table is redundant (see point 1), I would like to see the multivariate logistic regression analyses in the last column. Personally I am not a fan of step-wise backward-elimination techniques. Since this is a matter of taste, I leave it up to the authors how they do the multivariate analyses.

In the revised version, the last column of each table now shows the multivariable results. In our previous draft, we reported the estimates of all of the variables retained in the final multivariable model in the text. Because they are now in the table, we shortened the text somewhat.
Minor essential revisions

1. From the abstract it is unclear to the reader whether the response figures mentioned were from both stages and from evacuees/controls.

   We have modified the Abstract (pg. 2) to clarify this. In addition to providing stage 1 and stage 2 participation rates separately for children and mothers, we provide the group participation rates for child evacuees and child classmates as well as mother evacuees and mother classmates.

2. Background, third paragraph, second sentence: "studies of toxic exposure are often implemented". Often? To my knowledge I do not know other studies after disasters that choose for such design. Maybe the authors mean that this is done often after other studies. Clarify this to the reader by adding references. In the whole paragraph the studies among the general population and among survivors of disasters are mixed. Do the authors suggest that these studies can be compared?

   We have changed the wording of the paragraph to suggest that studies of toxic exposure may want to be implemented as two-stage studies to study both subjective and objective measures of health. Also, we are not suggesting that studies of toxic exposure on the whole be compared to general health population studies. Rather we are comparing the results from the handful of such 2-stage studies that provided data on attrition and have clarified this in the text on pg. 4.

3. Sample. Clarify to the reader why this a high risk group for thyroid cancer.

   We have added a sentence in the last paragraph of the Background section (pg. 4) to clarify that the children in our study constitute a high risk group for thyroid cancer and cite both the Chernobyl Forum report: 2003-2005 and Baverstock and Williams (2006).

   According to the Chernobyl Forum report (2006):

   "Fallout of radioactive iodines led to considerable thyroid exposure of local residents through inhalation and ingestion of foodstuffs, especially milk, containing high levels of radioiodine. The thyroid gland is one of the organs most susceptible to cancer induction by radiation. Children were found to be the most vulnerable population, and a substantial increase in thyroid cancer among those exposed as children was recorded subsequent to the accident"

Discretionary revisions

1. Background, first sentence. Although we could argue until what time long-term follow-up is needed, the reader likes some clarification on why long-term follow-up is needed.

   We have added a second sentence in the introductory paragraph (pg. 3), emphasizing that long-term follow-up studies are especially needed in disaster studies that focus on children as there remains considerable debate regarding the effect of traumatic experiences on childhood development and adjustment. We note here that from a public health perspective, it is unfortunate that there are so few closely monitored populations, especially children, after toxic events that involve a threat to health
because such data would greatly inform the timing and substance of intervention efforts, if indeed they are needed, for such populations and their families.

2. Second paragraph, first sentence is unclear to the reader.

This sentence has been changed to emphasize that it is important to understand possible selection bias for subsequent statistical analyses and the interpretation of results (pg. 3).

3. Sample and procedure. Which ethical constraints are there on collecting data of non-participants?

We removed the phrase regarding ethical constraints as it was not necessary and caused confusion. What we were referring to is the fact that some of the non-participants volunteered information when they were contacted, such as whether they were attending university or the fact that their health has been fine and therefore they did not want to participate.

Our paper focuses on participation at follow-up and in the medical exams following each interview. We have no information on those who declined initial participation, meaning evacuees and classmates in 1997 and population-based controls in 2005-6.

4. The description of the design is not very clear on page 6. Who were the participants at both times? Were both the mother and child at both times interviewed and had both a medical examination? What was the length of the interviews? Were they real interviews with an interviewer or did the participants fill in questionnaires themselves (either on the computer or on paper)? The description of the study design is important since the objective of this paper is to improve the design to increase the response in future studies.

We have clarified the description of the design in the Methods section (pg.5-7). Both children and mothers participated at both time points although only the children were given a medical examination. As stated in the last paragraph of the Background section and repeated again in the Methods section, both children and mothers were interviewed in their homes (stage 1) and then the children were subsequently medically evaluated at a clinic in Kyiv (stage 2). The interviews were administered by interviewers who were employed by independent survey research firms in Kyiv at both assessment points (SOCIS-Gallop at time 1 and KIIS at time 2). At time 1, the interviewers filled out a paper-pencil interview booklet, which included a few self-administered symptom questionnaires that the mother/child completed; the time 1 interviews lasted approximately 45-60 minutes for the children and 2 hours for the mothers. At time 2, we created computer-assisted interviews, and the interviewers each had a laptop that they brought with them to the interview. Again, the interviewer asked most of the questions, but we also had a few self-administered symptom scales that the respondents filled out on the laptop. It proved to be a very successful experience. The time 2 interviews lasted approximately 2.25 hours for the children and 2 hours for the mothers.

5. Results, first paragraph. Since this journal does not have a space limit, a table in which the non-response groups are compared can be included in the paper. The reader can look at the results themselves.
Because the non-response groups do not differ significantly on any of the key predictors that were studied in the manuscript, we decided to provide this table as an Additional table, rather than include it in the printed manuscript.

6. Results, page 10. The paragraph with all the numbers of response is hard to read. My suggestion is either include the numbers in Figure 1 or in a table. In addition, why describe both the conditional participation and unconditional participation rates? This differences is not explained later on in the discussion.

In the revised version, we no longer repeat the numbers in Figure 1 in the text. Also, we now only report the percent of baseline respondents who participated in the follow-up (Results, pg. 10). In short, we agree with these comments completely.

Reviewer #2

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?

The reviewer’s question is very important, and that is why we included the key findings from non-disaster cohorts by the Eaton group, the NEMESIS group, and others. Our study focused on a specific population after a specific event. To the extent that the pattern of findings from our study mirrors findings from general population follow-ups and other disaster cohorts, we are able to draw some general inferences. As the reviewer implies, a key difference in selecting predictors of attrition in general population versus disaster cohorts is the inclusion of disaster-related risk factors. This is the reason we emphasized the influence of Chornobyl risk perceptions upon participation. As discussed in the manuscript, Chornobyl risk perceptions did indeed have a significant influence on both mothers’ follow-up participation and attendance at the clinic. We also included the types of demographic and mental health predictors that were highlighted in other attrition papers.

â€¢ For disaster cohorts, what are specificities of attrition predictors for mental health as compared to somatic outcomes?
We are not entirely clear that we understand the question. If the question is whether mental health or somatic complaints predict attrition in disaster studies, as discussed in the paper, most studies looked only at mental health and found it to be a significant predictor although the direction varied according to whether the population was western or non-western. We seem to be the first study to also examine perceived physical health with respect to follow-up participation. If the question refers to more general issues, we know of no studies that compared events whose primary outcome involves physical health consequences to events having mental health consequences only.

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

First and foremost, we attempted to identify all previous long-term disaster studies (defined as having follow-up periods greater than 2 years) to learn what they reported, if anything, about the rates and predictors of attrition. For comparison purposes, we also reviewed long-term health studies of general population samples that were specifically focused on the issue of the predictors of attrition. We did so because with the exception of van den Berg et al., there has been very limited attention given in the disaster literature to the predictors of attrition. We have reorganized the paragraph on pgs. 3-4 to clarify that our primary interest was in the disaster literature. Indeed, we were greatly impressed with the van den Berg report and see our paper as complementary to theirs. We hope that other disaster researchers will do this kind of careful examination in the future.

Methods
Minor revisions

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

Gender and age of sample have been added into the Methods-Sample section (pgs. 5-6).

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.

We thank both Dr. Bard and Reviewer #1 for asking us to be explicit about why the cohort is considered a high risk group. We now reference Baverstock and Williams (2006) as well as the U.N. Chernobyl Forum. We note here that Bard et al. (1997; Epidemiologic Reviews) commented that during the first decade after the accident, children under 15 were at risk for thyroid cancer, and the younger the age at exposure, the greater the risk.

The basis of initial random selection, in particular for classmates, should be mentioned. This is done (phone directory for the follow-up survey)

Homeroom classmates were selected based on gender. The classroom roster was alphabetized, and the next same-sex non-evacuee child served as the control. Three potential classmate controls were chosen in case of refusal. This has been added to the manuscript (pg. 5), and we are glad the reviewer raised this omission.

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).
As noted in response to Reviewer #1, we removed this phrase because it was confusing and not readily understandable.

There is no indication (see above) on whether evacuees’ classmate controls and population controls were matched for gender.

As stated in the Methods section (pg. 5), the classmates were matched according to gender and homeroom of the evacuee child. Because of the matching, we examined whether the evacuees’ participation was influenced by the matched classmates’ participation (with the McNemar test for matched pairs).

The population controls were not matched according to gender. However, gender was not significantly different among the three groups in the follow-up study (52.5% evacuee females, 51.3% classmate females, 51.4% population control females) (this is now reported on pg. 6).

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.

We now clarify in the Background section (pg. 5) that we examined we examined demographic, health, mental health, and disaster-related attitudinal variables because these variables were previously shown to be associated with participation. We further note that mental health appears to be a robust predictor of attrition although the direction differs for western and non-western populations. We also reorganized the measures paragraph on child well-being (pg. 8) to more clearly separate mothers’ perceptions of the child’s physical health from her rating of behavior problems (mental health) in 1997. We note here that behavior problems and somatization (P-CSI in 1997) were not correlated with one another (r=0.03).

Major compulsory revisions

What is the purpose of using a population based control group? What is expected?

The population-based control group was added to serve as a comparison group that is representative of Kyiv. Our original controls were classmates and obviously were not representative of the population of Kyiv. Indeed, this was a significant limitation of our original study. This is now clarified on page 6.

Again, what about the non respondents in this group?

Unfortunately, we were unable to ask questions of the population controls who declined participation.

How is standard of living assessed?

Perceived standard of living was assessed by a Likert scale developed by our colleagues in Kyiv and widely used in socio-economic surveys in Ukraine (“How would you rate the material standard of living of your family on a scale of 0 to 10, 0 being the lowest and 10 being the highest?”). This has been clarified in the manuscript (pg. 8).

No justification appears for choosing the parents academic graduation as a measure of parental education.
In addition to standard of living, being a university graduate is another important indicator of SES. We chose “completed university” rather than simply “attended university” because having a university degree is the most important educational milestone. This has been included in the manuscript on pg. 8.

What is a Chornobyl-related illness, and what makes the difference with having health being affected by the accident?

In the revised manuscript, we clarified the distinction between these variables both in the text and in the labels used in the tables. Specifically, diagnosed with a Chornobyl-related illness indicates whether the respondent was ever told by a physician that he or she had an illness that was a direct consequence of Chornobyl. Examples of illnesses that physicians commonly linked to Chornobyl include vascular dystony (the official Chornobyl diagnosis), enlarged thyroid, enlarged lymph nodes, and problems with the immune system. However, doctors in Ukraine often told patients with other health problems, ranging from cardiovascular disease to the common cold, that these too were caused by Chornobyl. Believing that your health was severely affected by the accident is a risk perception. In some cases, people believed their health was affected most likely because they were told this by a doctor; in other cases, they believed their health was affected without any formal diagnosis or discussion with their physician. We have also clarified the description of these two variables on pages 8 and 9.

Why choosing having ≥ 2 colds in the past year and also headache or migraine as a variable?

At issue here is what physical health variables predict participation in the follow-up. Frequent colds and headaches are two common ailments in any young adult population, and they were commonly reported by the evacuees and controls in Kyiv as well. We thus examined whether these health complaints increased the likelihood of participating in the follow-up and of taking part in the medical examination. Regarding the decision to dichotomize number of colds, we decided based on the frequency distribution of this variable that the best uniform cut-point was 2 or more colds within the past year.

The adjustment for group status (I don’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.

See our response to reviewer #1. We have removed the unadjusted odds ratio columns from all the tables. Adjusting for group status was necessary because it was a design variable. The fact that the ORs were indeed similar shows that group status did not influence the relationship of these predictors to the attrition variables.

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.

The refusal rate for the population-based controls in 2005-6 is given on page 6 (Methods section). Their participation rate in the medical examination is provided on page 12 (Results section). Figure 1 only shows the numbers assessed at each time and within time, at each stage.
We reorganized the Methods and added headings to Results so that all of the response rate information is now clearly accessible to the readers. We apologize that these rates were somewhat buried in the previous version.

Page 12, the sentence “The examined children also had higher self-reported CSI scores than children not brought to the clinic” is vague (figures?).

We rewrote to sentence to clarify that at time 1, the children’s self-reported CSI scores were higher for those who received the medical exam compared to those who did not get the examination (pg. 12).

In the tables, despite of columns heading indicating OR, some results are expressed differently.

We have modified the table to present odds ratios for continuous variables so that there is no confusion regarding test statistics. All continuous variables were standardized in logistic regression models to facilitate interpretation of odds ratios.

Table 4 caption doesn’t allow to know what is the population described. From the text it can be understood that it relates to young adults, not mothers?

We have also modified the titles of Tables 3 and 4 to clarify that the tables present the associations between children’s clinic attendance and key variables.

Results of multivariate analyses should appear in the Tables.

This issue was also raised by reviewer #1, and we have added the multivariable results to the Tables.

Major compulsory revisions

The fact that evacuee status was not a predictor of participation is indeed intriguing. Possible changes in classmates’ status after 8 years should be addressed.

The fact that evacuee status was not a predictor is indeed intriguing – we agree completely. We fully expected that it would be. One possible explanation is that even though the classmate controls were not immediately exposed to the disaster, they still considered themselves to be affected by the disaster and thus were just as motivated to participate in the follow-up study. A discussion of this has been added on page 14.

Discussion of the discrepancy with Havenaar results is lacking.

We have added a comment on this discrepancy in the Discussion (pg. 14). The most likely explanation is that the Havenaar sample was composed of adults, whereas we studied children at ages 11 and 19. It is also conceivable that even at follow-up, the mothers, regardless of group status, encouraged their children to take advantage of the free medical examinations.

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.

The statement discussing the association between distrust of authorities and perceived danger and psychological distress after the TMI accident has been deleted from the manuscript. We were simply suggesting that this variable may serve two purposes: it may be a powerful correlate of psychological distress and perceived danger as well as a powerful predictor of clinic attendance.

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.

We completely agree with the reviewer’s point of view. Our view is that to the extent that findings are confirmed across different disasters, or attrition predictors are detected for different types of disasters, then we will be able to draw general inferences. The problem in the field is that researchers tend to use different risk groups and often unrepresentative samples, different measures and different time intervals after the disaster when designing their studies. The intangible nature of radiation and other exposures is unique. In previous reviews that we have published, we argued that the psychological impact of these toxic exposures is more chronic and protracted than that of natural disasters. However, there have been very few well-designed, long-term follow-up studies of disaster survivors, and the jury is still out. Regarding disaster preparedness, this particular paper is about attrition, and we feel that the topic of disaster preparedness is beyond the scope of the current paper. However, it is most certainly an issue to consider in future outcome papers.

We also want to emphasize that the issue of selective attrition in disaster cohorts is extremely important to examine because as the field grows, and more longitudinal studies are implemented, we will need to fully understand the biases inherent in each disaster study population. We hope that this paper will lead to other detailed examinations of well-defined disaster cohorts assessed longitudinally, as well as stimulate researchers to consider a long-term perspective rather than rely only on short-term findings.

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Reviewer #3

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

Version: 1 Date: 3 January 2008

Reviewer: Lars Weisaeth

Reviewer's report:

Major Compulsory Revisions

I would like the authors to reconsider the available evidence related to the first of the three main concluding points they make. It is to the explanation of why higher levels of psychological distress were associated with participation in their follow-up study in contrast to findings in Western studies. Although I agree that the interpretation provided by the authors, the distinction between a Western and a non-Western population, may be correct, their discussion of an alternative explanation should be broadened. Could it rather be that it is the type of disaster that makes the difference? Could it be that help seeking behaviour differs after disasters with sudden and violent invasive impacts, that are likely to produce post-traumatic stress responses such as explosions, transport disasters etc. At least two of the Western disaster studies the authors refer to in terms of the non-response issue, are of this nature. Toxic disasters on the other hand do not typically produce disorders within the post-traumatic stress spectrum.

We agree that there could be other plausible explanations for the finding that psychological distress predicted participation in our sample, rather than the reverse as is often found in western samples. The other likely explanation is that toxic exposures that create a general fear about cancer would lead people who are worried, and hence score high on anxiety measures, to take advantage of an opportunity to be evaluated. We have added this consideration into the Discussion paragraph on pg. 15. We do not know of data to support the hypothesis that anxiety is inversely related to participation in disasters that do not have potential long-term health effects, but it is certainly a reasonable supposition. As more studies are published on the determinants of attrition in disaster cohorts after different types of events, we hope that we will have fuller data on this very issue. Again, this is one reason we feel that this paper is important for the field since it is rare for investigators to provide a full exposition of the determinants of attrition.

(Leaving my normal modesty aside for a moment, I give in to the temptation to refer to a study I myself did on the non-response to a combined systematic outreach research project following an industrial explosion: The 19% who did not at once respond positively to the invitation to be examined, but were later recruited into the study, turned out 7 months post-disaster to contain 40% of the total number of cases of PTSD. The main explanation for the non-response was the avoidant behaviour which is a regular part of the post-traumatic stress syndrome. Weisæth L (1989) Importance of high response rates in traumatic stress research. Acta Psychiatr. Scand. Suppl.. 355; 80:131-137).

Actually, it was Dr. Weissaeth’s paper that triggered both the enormous efforts during the fieldwork to get as high a response rate as possible and our decision to write this
paper. So we are duly embarrassed that after all that, we managed to omit his paper from our manuscript !!!!! It is now incorporated both in the Background section on pg.3 and in the Discussion on pg.15.

It is perhaps to be expected that there will be a high participation rate to medical follow-up examinations that focus on so-called chernobyl related illness. If the focus was on somatic symptoms as well, and not only on the psychological aspects, the avoidance of stigma that is usually associated with post disaster psychiatric follow-up studies, could have been avoided, which would be helpful in achieving lower non-response rates.

We agree with the comment and in fact emphasized the physical health aspect of the study in recruiting respondents for the interview and the examination. We also note, as the reviewer knows very well, that people with mental disorders are highly stigmatized in Ukraine. Thus we went to great lengths to emphasize the somatic part of the study. Just to be clear -- the medical examinations were not focused on Chornobyl-related physical or psychological conditions but rather they were general physical examinations. Also, as we are sure the reviewer understands, ideally one would want the physicians to be blinded to exposure status; in reality, everyone seems to know who is who in Kyiv, and much as we tried, it was impossible to implement a blind study.

Minor Essential Revisions

Is the spelling Chornobyl throughout the paper a Ukrainian convention for the international spelling? The same applies to Kiev and Kyiv? Particularly the latter spelling may confuse the reader.

We use the Ukrainian transliteration for Kyiv and Chornobyl, given that our study was done in Ukraine. However, if the journal prefers to use the Russian forms (Chernobyl and Kiev), it is fine. We now include a sentence about that in the Background section on pg. 5.

Discretionary Revisions

The authors express surprise that the young adults in the focus groups expressed boredom with the topic of Chernobyl while they were likely to accept a clinical evaluation and follow-up. In my experience I rather find that typically young adults, at least the males, in a group context have a need to express such negative attitudes. Their individual help seeking behaviour, however, may illustrate a more independent motivation.

Young adults certainly have “an attitude” that gets expressed in a group context. The fact that their risk perceptions were significantly associated with going to the clinic took us by surprise perhaps because we were so taken by the contrast with their mothers, who couldn’t get off the topic during the focus group meeting. In any event, we revised our comment about this issue on pgs. 16.

The gender aspects of help seeking behaviour could also be discussed a bit more in the paper.

We have added a bit more discussion on this on pg. 14 and refer to two other studies that have also found that young women have higher rates of medical help seeking than young men. We note though that gender did not remain significant in the multivariable analysis.