Author's response to reviews

Title: Are passive smoking and air pollution a greater health risk than major radiation incidents?

Authors:

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Author's response to reviews: see over
Dear Sir or Madam,

“Are passive smoking and air pollution a greater mortality risk than major radiation incidents?” MS: 8907299781108847

I enclose my responses to the referees’ comments on the above manuscript. I have included the referees comments and (in italics) my responses to those comments. In addition to amendments in response to specific comments from the referees, and in view of their general comments, I have also revised the manuscript to better clarify the methods, results and conclusions of the paper.

Yours sincerely,

Dr. Jim Smith
Response to referees’ comments.

Are passive smoking and air pollution a greater mortality risk than major radiation incidents?

MS: 8907299781108847
Dr. J.T. Smith

I thank the referees for their thorough reviews and useful comments. The referees’ comments are copied below in normal font, with my responses to each comment in italics. In addition to amendments in response to specific comments from the referees, and in view of their comments, I have also revised the manuscript to further clarify the methods, results and conclusions of the paper.

Response to reviewer #1

Reviewer: Mark Little
Reviewer’s report:

General

This paper is generally very well written, and persuasively argues that risks from radiation exposure may be comparable with or less than those from more common environmental and lifestyle factors. There is good discussion of the uncertainties surrounding these risk comparisons. There should be more consistency in inclusion of various groups in the Tables, and the statement about the Japanese atomic bomb survivors in the abstract should be corrected. Otherwise I have only minor comments, most of which are inessential (the author may care to include if he wishes).

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Major Compulsory Revisions (that the author must respond to before a decision on publication can be reached)

p.2 l.-7-6 The description of exposures by the survivors of Hiroshima and Nagasaki as “extremely high” is questionable. The range of (approximately whole body) doses received by the atomic bomb survivors is in the range 0-4 Gy and the average dose in the proximal (<2.5 km from hypocentres) part of this cohort is about 0.2-0.3 Sv (depending on the organ at risk) (Preston et al., Radiat. Res. 1994;137:S68-S97, Thompson et al., Radiat. Res. 1994;137:S17-S67, Preston et al., Radiat. Res. 2004;162:377-89). In comparison, radiotherapy patients generally receive much higher doses, typically 30 Gy or more to certain tissues.

The doses rates to which I was referring are those of the high dose group of bomb survivors (mean dose 2.25 Gy) which, in the context of the majority of postulated nuclear incidents, is extremely high. But I agree with the referee that it isn’t appropriate to use the term “extremely high” as it is used here in the abstract. I have deleted “Extremely high” from the sentence.

Tables 2-4 The rationale for presentation of various studies in these Tables is unclear. For example, two different groups of Chernobyl emergency workers are mentioned in Tables 2 and 3, the first apparently all Chernobyl emergency workers (the same average dose is quoted for this group by Cardis et al., J. Radiol. Prot. 2006;26:127-40), and the second some subset of this. A group of bomb survivors exposed within 1500 m of the hypocentres are mentioned in Table 4 (but nowhere else). The use of the “250 mSv dose group” for the Chernobyl workers in Table 3 appears arbitrary, especially as the comparison in this Table is with two other groups not at particularly elevated risk from their relevant exposures; I suggest that the same group as for Table
2 (with 100 mSv average dose) be used. Also, risks for persons exposed to passive (environmental) tobacco smoke are mentioned in Table 3, but not in Table 4, where only risks for active smokers are presented.

I have added a paragraph to the methods to clarify the rationale for the choice of exposure scenarios. Rather than use 100 mSv for both tables, I have revised the tables to show the 100 mSv mean dose and 250 mSv high dose in both tables. As discussed in the revised text this is to give an illustration of the range in exposures for this scenario. On the final point, I did not think it appropriate to include environmental tobacco smoke in Table 4 since the intention in Table 4 was to illustrate “high” mortality risks for comparison with the scenario for “high” radiation risk.

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Minor Essential Revisions (such as missing labels on figures, or the wrong use of a term, which the author can be trusted to correct)

p.5 l. -8-7 The ICRP estimates risks for a general population exposed at ages 0-90, and for a working population exposed at ages 20-64, with follow-up in both groups to age 90 (as far as I can infer). Mention of a life expectancy of 75 may be correct, but is possibly misleading. I would remove this sentence, or replace it by one making these points.

I agree that this may be confusing as the 75 year life expectancy (typical for industrialised countries) and a more typical 30 year working life (in the occupation in which elevated exposure occurs) was used to calculate the total lifetime dose and does not refer to the life expectancy assumed for the ICRP estimates. I have removed the sentence.

Figures 1-3 I am unsure how informative these figures are. If Figure 1 is retained, it should reflect the latest (DS02) mortality report (Preston et al., Radiat. Res. 2004;162:377-89) rather than this now quite old paper (ref. 40).

Figures 1-3 were retained as I believe that they are useful illustrations for those not especially familiar with the empirical evidence for the different risk factors. I have updated Figure 1 to reflect the more recent data, as suggested.

Table 1 Most of the useful information from this Table is presented in the text (p.6), and this could therefore be removed.

I have removed Table 1 and incorporated the information in the text.

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Discretionary Revisions (which the author can choose to ignore)

p.5 l.3-4 The ICRP (ref. 5) risk estimates assume a Dose and Dose-Rate Effectiveness Factor (DDREF) factor, used to reduce risks derived from acute exposed groups to chronic exposure. This is probably correct for most of these groups. However, for at least some of the Chernobyl emergency workers listed in Tables 2-3 dose rates are likely to be very high, so a DDREF might not be applicable. Perhaps risks could be given with and without a DDREF?

The question of whether it is appropriate to apply a DDREF to the Chernobyl emergency worker group is an interesting one. Though dose rates would have been higher than for most of the other exposures presented here, it is not clear whether they would have been high enough to make the DDREF inapplicable. The dose rates to the Japanese bomb survivors were of order 6 Sv per second - orders of magnitude
higher than dose rates even to the emergency worker group. A recent study of Europeans exposed to low doses from Chernobyl (Cardis et al. 2006; cited in the paper) applied a DDREF, but the WHO study (WHO; 2006; also cited), of populations with average doses from 7-100 mSv, did not.

I have not presented risks both with and without a DDREF as I think there is already a lot of information in the tables and even more would make them confusing. I have, however noted this as one of the sources of uncertainty in the table notes, and in an additional paragraph discussing this issue.

p.5 l. -9-8 As the author points out further up p.5, the ICRP estimate risk both for all age (0-90) and for working age (20-64) populations. It appears from Tables 2-3 that the author may in fact be applying the slightly lower (worker) risk estimate (4% per Sv) to worker groups; this could perhaps be made clear here.

I have added a sentence to clarify this and altered Table 2 (now Table 1) to separate exposures to the general population from those to the working population, as recommended by referee 2.

p.8 l.9 A review of tobacco smoking and cancer by Vineis et al. (J. Natl Cancer Inst. 2004;96:99-106), which inter alia also surveys studies of environmental tobacco smoking (and finds persuasive evidence of excess risk), could also be cited here.

This is a useful reference, but it focuses on cancer risk rather than overall health risk. Since, for passive smoking, predicted mortality from cardiovascular diseases greatly outweighs cancer mortality, and since the reference list is already rather long, I have decided not to add this reference: there are already references to the various health risks of active and passive smoking.

p.9 l.5 A more recent review of the Chernobyl epidemiology is given by Cardis et al. (J. Radiol. Prot. 2006;26:127-40) and could also be cited here.

I have now cited this recent reference

Table 5 NCRP Report 126 (National Council on Radiation Protection and Measurements 1997, Uncertainties in Fatal Cancer Risk Estimates Used in Radiation Protection) could perhaps also be cited here.

I have now cited this reference

Referee’s decision:

What next?: Accept after minor essential revisions
Level of interest: An article of outstanding merit and interest in its field
Quality of written English: Acceptable
Statistical review: No
Declaration of competing interests: I declare that I have no competing interests.
Response to reviewer #2

Reviewer: Yasuhiko Yoshimoto
Reviewer's report:

General
This is a review article. Also author's conclusions are misleading. For policy decisions in public health services, QALY has been developed to quantify a health impact that takes into account changes into longevity and quality of life. ICRP also has developed a concept of radiation detriment, including cancer incidence and hereditary effects, with a dosimetric quantity effective dose for radiological protection in low dose and low dose rate range from daily practices. In the case of major radiation incidents it is important whether countermeasures to reduce radiation exposure of the individual were timely and adequate or not. A primary target of radiological protection depends on the type of each radiation incident situation. For example, effective dose is not appropriate in making decisions on the necessary treatment for ARS at extremely high doses. An increase of thyroid cancer incidence has still continued in the former USSR contaminated areas by Chernobyl accident. Continuous efforts are essential to share the past experiences scientifically with the public and build a sound risk communication.

I believe that many of the referee's critical comments stem from an understandable misperception of the purpose of the paper. In the original manuscript it was perhaps not sufficiently clear that the paper is not intended as a full review of the consequences of the Chernobyl accident. There are many such reviews, a selection of which have been cited in the manuscript. Rather the paper is focused on an evaluation of the long term mortality risk from exposures to ionising radiation in comparison with other more common health impacts. Mortality risks from Chernobyl and the A-bomb survivors are used as examples of the exposures and risks from ionising radiation. I have altered the Introduction to clarify this.

I agree with many of the referee's comments, but don't believe that they are appropriate criticisms of this manuscript. I am aware that QALY is often used by decision makers, but for the objective of this manuscript, such an approach was not appropriate. Firstly, as stated in the paper, morbidity endpoints are not always accurately known for the range of health stressors considered here. Secondly, for this reason, and because QUALY is necessarily to some extent subjective, the mortality endpoint is clearer and more objective. Whilst I have noted the limitations of this approach in the manuscript, I believe that it was the best one to use for this study.

The referee is correct in his statements about the importance of countermeasures, and about the Chernobyl accident. These issues are covered at length in my recent book on Chernobyl, the recent Chernobyl Forum report and in other references cited in the manuscript. The paper is not a review of the Chernobyl accident: it is a comparison of radiation risks with other risk sources. Thus, whilst I have mentioned ARS and thyroid cancer in the paper, it would not be appropriate to discuss them at length. As discussed above, I have amended the manuscript to clarify this.

Major Compulsory Revisions (that the author must respond to before a decision on publication can be reached)
Title: Title is not appropriate. Author could not derive to the question a balanced and impartial comparison of ionizing radiation, air pollution, obesity, and smoking, particularly with respect to major radiation incidents on which author focuses.
In view of the referee’s comment I have altered the title to “Are passive smoking and air pollution a greater mortality risk than major radiation incidents?” As the paper focuses on mortality risks, I agree that this is more appropriate than the previous title which referred to “health risk”.

The following “Background” given by the referee consists of comments of a general nature (most of which I agree with) and do not suggest specific revisions to the manuscript. I have briefly responded to these general comments.

Background:

It should be pointed out that:
1) It is an international consensus by Chernobyl Forum 20 years after the accident that the interventions implemented by the governments were mainly timely and adequate to reduce radiation exposure from the accident. For example no case of ARS was recognized among the residents in the 30 km zone around the reactor with relatively swift evacuation of the people in the areas.
2) There is no little doubt that an excess of thyroid cancer has occurred due to childhood/adolescence exposure in the Chernobyl highly contaminated areas, far beyond the 30 km zone. But the experiences indicate the excess would be reduced by an appropriate official control of uncontaminated food supplies/restrictions of 131-I contaminated food stuffs (in particular milk) and providing information to personal control over the risk after the accident and pre-information of protection action for radiiodine inhalation such as sheltering/evacuation/iodine prophylaxis and area improvement of iodine deficiency in daily life.

These statements are generally true, though many would not agree that interventions by the Soviet authorities could be described as “timely”. The failure immediately to evacuate Pripyat town, to distribute potassium iodide tablets, or (as the referee notes) to avoid drinking milk does not, in my opinion, represent a “timely” response, though I agree that in other respects, the emergency response was appropriate. As discussed above, however, this paper is not a review of the Chernobyl accident. The intervention measures and health consequences of Chernobyl have been reviewed in many publications, a selection of which are cited in the paper. I have clarified this in the revised manuscript by rearranging the introduction and adding a sentence reading:

“This paper necessarily focuses on stochastic mortality risks from ionising radiation and does not aim to give a full assessment of the health consequences of radiation incidents such as Chernobyl. Health consequences of Chernobyl (including, for example, ARS and thyroid cancer) have been reviewed elsewhere (e.g. [2, 13-15]);”

3) The Chernobyl accident reminds us that uncertainties for assessments of radiation doses from internal exposure are larger than those from external exposure. This may raise a concern of an increase for leukemia as well as solid cancers other than thyroid cancer in the contaminated areas. But no persuasive evidence was found to support the concern.

Yes, I agree that there are significant uncertainties, as discussed in the paper. I also note that Ivanov and co-workers’ (2004; Medical consequences of the Chernobyl catastrophe in Russia; Nauka, St Petersburg) study of Russian emergency workers did find evidence of a leukaemia increase linked to radiation.

4) A concern of the public differs in degree for women of child-bearing age and for the population at large. Yet no evidence has been found on human for heritable genetic
effects of radiation. Radiation is much less likely to have adverse genetic effects than had worried in the past when conducting many tests of nuclear weapons in the atmosphere after military combat uses in Japan in 1945.

On the other hand efforts have been undertaken to identify the special population of in utero radiation exposure from Chernobyl accident. This concern may be raised because there is an association between exposures to diagnostic x-rays in utero, only 10 mGy and subsequent childhood cancers. But no convincing evidence was found because of the limited population and crude dose assessment. Among the somatic effects of radiation other than cancer, developmental effects on unborn child due to in utero exposure are of greatest concern. But they depend on the stage of gestation, the dose, and the dose rate. Termination of pregnancy is an individual decision affected by many factors. Doses below 100 mGy low LET of in utero exposure should not be considered a reason for terminating a pregnancy, especially prolonged exposures.

Again, I agree with these statements, but this MS is not intended as a full review of the health consequences of the Chernobyl accident or of the A-bomb effects. I hope this has been clarified in the revised manuscript.

5) Health effects including benign neoplasms other than cancer have been linked to radiation exposure. A special attention has been paid to recent accumulated statistical evidence for the increased frequency of nonneoplastic diseases such as cardiovascular disease. But the role of radiation is uncertain in such nonneoplastic diseases and there is lack of evidence to estimate the risk in doses below 100 mGy, especially prolonged exposures.

I agree. In the paper, I deliberately separated the risks from low dose/dose rate exposures (for which the endpoint is believed to be cancer) from the high dose/dose rate exposures to the high dose groups of bomb survivors, for which other diseases such as cardiovascular disease may be a factor in life-shortening. The life-shortening was obtained directly from the Life Span Study mortality data by Cologne & Preston, so includes all potential impacts of radiation on mortality.

6) After a large scale nuclear facility accident such as the Chernobyl accident its operating organization or its national regulator may sometimes faces the allegations of acute symptoms which could be caused by radiation although the possible maximum dose is thought to be less than the necessary dose to produce; they are considered to be a product of psychological stress (radiophobia used in the past as a dismissive term) or efforts to gain compensation from the organizations associated with the accident.

The psychological impact of Chernobyl accident on the local residents of the former USSR, especially women of child-bearing age, is understandable due to evacuation and relocation with the worry of possible health effects from the radiation exposures. Also intervention practices caused high radiation exposure to emergency workers including facility personnel during the accident and lesser exposure to more large recovery workers after the release of radionuclides had ceased. Over 20 emergency workers died of ARS.

Psychological stress may differ for voluntary and involuntary risks. Increase in non-specific health effects other than cancer has been reported among the residents and recovery workers. It is difficult to interpret these findings in terms of radiation dose. Unfortunately the consequences of the USSR break-up also have had a profound effect on the health of the population.

The radiation risk which most of the public can encounter is small even at a major radiation incidence and its uncertainty becomes greater as the risk becomes smaller. To allely concern over this uncertainty could be reached through the timely and adequate implementation of remedial measures to reduce radiation exposure at a
radiation accident and trust and public confidence at provided information of radiation risks without misleading so that the public can become more aware of and comfortable with the responsible radiation use in our daily life.

See responses above: these points concerning psychological stress are beyond the scope of the paper (as noted in the Introduction) and are covered in references cited in the MS. The final point about “public confidence in provided information” is, I believe, part of the rationale for the submitted manuscript.

7) It is a controversial to evaluate cancer burden including the risk of leukemia and solid cancers other than thyroid cancers with a lack of clear evidence to demonstrate an excess, in Europe as a whole from radioactive fallout from the Chernobyl accident. The publication (Cardis et al, IARC, 2006), not based on the past twenty-years experiences after the accident, is particularly troublesome, perhaps even irresponsible, because the implied health consequences were based on hypothetical calculations of very large number of the population and the small cumulative dose based on average country- and region-specific doses.

I disagree with the referee’s statement that the Cardis et al. publication is “particularly troublesome, perhaps even irresponsible…” Both the recent epidemiological and radiobiological evidence points towards a cancer risk from even low dose ionising radiation exposures – this, as I’m sure the referee is aware, is the position taken by radiation protection agencies worldwide. As I have pointed out in the paper, risk factors such as low-dose ionising radiation, passive smoking and air pollution all carry significant uncertainties. However, that does not mean that we should ignore the implications of the scientific evidence as it now stands which, for all three risk factors, points to a definite health risk to those exposed.

8) It is obvious that health problems are different among selected issues. Radiation exposures in daily life are controllable except natural radiations. Major radiation incidents such the Chernobyl accident rarely occur. Health problems of smoking habits and obesity have a strategy as a primary prevention to attempt the entire population to move the population distribution in the direction of lower risk. The decisions to not-smoke or diet are based on the individual risk management. Health problems of air pollution are similar with respect to the responsibility of government and industry risk managements. However, more clear mechanisms of health effects from air pollution remain to be identified compared with those of radiation. It is suspected that a message by the present manuscript can allow a broader audience to gain a reasonable impression of what should be learned from the Chernobyl accident.

I agree with these statements. As noted in the paper, there are many risk issues over and above the long-term mortality risks which this study (necessarily) focuses on.

I agree with these statements. As noted in the paper, there are many risk issues over and above the long-term mortality risks which this study (necessarily) focuses on.

Minor Essential Revisions (such as missing labels on figures, or the wrong use of a term, which the author can be trusted to correct)

Method:
1) A brief description should be needed for risk measurements such as absolute risk, relative risk (RR), excess RR, and Years of life lost (YOLL) as well as confounding factors.

I agree that this could be useful, but do not think it necessary or appropriate for this paper – these terms are standard ones and inclusion of such a description would unnecessarily lengthen the manuscript.
2) Calculating radiations risks: Effective dose is intended for use as a radiation protection quantity. It should be noted that for the risk estimation of a known exposed population, relevant absorbed dose should be used.

*I have clarified where effective and absorbed dose is used.*

3) Calculating radiation risks: It should state that the recent BEIR VII has defined low dose as doses in the range of near zero up to about 100 mSv of low-LET radiation.

*I have added a sentence to state this.*

4) Calculating radiation risks, second paragraph: Delete "At this level, excess cancers may not be observable against a background cancer rate of 20-25% in populations in industrialized countries", In a hypothetical situation this argument is nonsense because power to detect an excess depends on sample size.

*I agree. I have altered the sentence so that it now simply notes the “background” cancer rate in industrialised countries and makes no comment on the power to detect an increase.*

5) It should be noted that risk in a population also depends on the distribution of important modifiers of risk such as age and sex.

*I have added a sentence to note this.*

6) YOLL is not a sensitive risk measurement and not useful for non-fatal cancer. Instead years of life lost per excess death (incidence) for a specific disease is reasonable although this measurement is not sensitive to dose.

*Yes, this is true, but this does not invalidate its use for this study which, as stated, focuses on mortality. YOLL per excess death is useful for some purposes, but, as the referee states, is not sensitive to dose so is not useful for this study.*

Results and Discussion

1) References [40-49] were not clearly cited in the manuscript except Tables.

*I don’t see it as a problem to cite some references in Tables which do not appear in the MS.*

2) References should be cited in Figures.

*I have cited necessary references in the Figure captions.*

3) Exposures and risk scenarios/ radiation risk: It should be stated that the doses relevant to ASR cases and an increase of thyroid cancer in Table 2.

*The point about thyroid cancer was made in the notes to Table 2 (now Table 1) – I have expanded this to mention ARS.*

4) Title in Table 2 should be “Radiation risk for cancer mortality”.

*I have altered the title of Table 2 (now Table 1). It now reads: “Risk of fatal cancer from radiation”.*

5) In Table 2 general population and working population should be separated to help readers to understand.
I have separated these in the revised Table 2 (now Table 1).

6) Exposure and risk scenarios, third paragraph: It should be noted that each endpoint in Table 3 is the mortality from different cause group; all cause mortality for air pollution, mortality from ischaemic heart disease for passive smoking, and cancer mortality for Chernobyl emergency workers.

I think this is clear from the Table (now Table 2).

7) Shielded whole body kerma should be noted for radiation dose, Gy in Table 4.

This is now noted in Table 4 (now Table 3).

8) Column title "Risk estimate" in Table 5 should be "Risk factors".

Now changed.

9) Uncertainty for obesity should be also summarized in Table 5.

I have added this to Table 5 (now Table 4).

10) Unit of exposure and baseline group should be noted in Table 5.

I have added this to Table 5 (now Table 4).

Discretionary Revisions (which the author can choose to ignore)

Referee’s decision:

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: No