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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Global risk from the atmospheric dispersion of radionuclides by nuclear power plant accidents in the coming decades" *by* T. Christoudias et al.

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Dear Editor,

We would like to thank the referees for their careful reading of our manuscript, "Global risk from the atmospheric dispersion of radionuclides by nuclear power plant accidents in the coming decades".

We are most grateful for the comments, constructive criticism and very useful suggestions received on how to improve the paper.

Please find attached our detailed answers to the questions (indicated by +) and an

updated version of the paper, which we hope satisfactorily address the points raised during the discussion.

*Referee 1:

The paper addresses risks from radioactivity exposures in the following 2 decades from hypothetical accidents in the operational, under construction and planned NPPs. In general, I am very positive about publishing this work in ACP due to the very attractive presentation of the results and the interpretation given by the authors. However, I would like to state some comments that, in my opinion, would make the manuscript stronger and more comprehensible to the reader.

-Abstract, line 13: Correct "dosages" to "doses".

+Corrected.

-Abstract, line 14: Correct "from the inhalation and the exposure to ground deposited radionuclides" to "from inhalation and ground-deposition exposures of radionuclides".

+Changed.

-Abstract, line 15: One of your main conclusions (also in the conclusions section of the manuscript) is that inhalation doses are the highest during boreal winter. However, I cannot see any evidence for this in the manuscript or in the supplements. Please add a graph or something to support it.

+ The increased concentration in the Northern Hemisphere during boreal winter is apparent when comparing the summer and winter seasons in Fig. 4. Rephrased the text to make reference clearer.

-Page 2, line 28: Correct "dosages" to "doses".

+Corrected.

-Page 2, line 29: There is one more pathway that should be taken into account (air-submersion). Please write a comment on its low contribution to the total dose or provide a reason why it is not included in the computations.

+Comment added on low contribution of air-submersion compared to inhalation dose (see for example "Fukushima Daiichi: ANS Committee Report", March 2012).

-Page 2, line 31: I think this paragraph is not useful and should be removed. We easily see the different sections of the manuscript.

+Removed paragraph.

-Page 2, line 104 (last sentence of the page): Please remove "and irradiation". You probably mean air-submersion, which is an external pathway that exists while fallout passes near human and non-human biota. Irradiation is the result of all these exposure pathways.

+Removed "irradiation".

-Page 3, line 5: Please correct "life-time" to "half-life".

+Changed.

-Page 3, line 56: I would like to see a graph of the daily emissions of these hypothetical releases in the supplementary information because I do not understand how the temporal release takes place. For example, if you claim that emissions take place throughout a year, this cannot be realistic, as in the two INES 7 accidents, atmospheric emissions occurred for 10 days (Chernobyl) and for 42 days (Fukushima). Moreover, it is not clear if you emit 1 PBq per year or 1 PBq in total. Please correct.

+ Text has been changed for clarity.

-Page 5, line 5: I do not understand how exactly you define the RISK. Please add a chapter in the supplementary information of the manuscript. Do you define it in relation to the burden at the lowest level, or the committed effective dose and for which period (20 years average???)? Is it a really dangerous exposure? As I see, the dose you get

is below 1 mSv, which is the maximum annual effective dose for the population from all artificial radionuclides according to the IAEA. Please explain.

+ This refers to the relative risk index that is defined as the cumulative expected dose from ground deposition in our model times the size of the population that is exposed for unit emission of radionuclides. Changed "risk" to "relative risk index" in the text to be more clear.

-Page 6, line 85: The definition of the risk as given here seems a bit speculative and meaningless. In radiation protection, when we discuss about population risk from radioactive releases, we usually mean how many people will get sick (morbidity risk) or die (mortality risk) from "all solid cancers" or from thyroid cancer (when we calculate exposures from 131I). The function for these calculations are given in detail by Ten Hoeve and Jacobson (2013) or Evangeliou et al. (2014) Env. International. I would recommend you to calculate suck risks. You can use future population density gridded data from NASA. The risk you mean here (but you don't calculate) is just a risk of a population to be exposed in a certain amount of radiation. If this risk is dangerous or not can be quantified using the cancer risk to the population exposed. You have to pay attention on that because exposure to radiation occurs continuously in human life. The question is how dangerous this risk is.

+ In our paper we calculate the population risk to receive radioactivity doses from emitted radionuclides. This allows for the application of particular morbidity and mortality risk models based on our findings. The study by Hoeve and Jacobson is cited in our paper but has received criticism in the literature on the specific factors used in the calculation (Accounting for long-term doses in “worldwide health effects of the Fukushima Daiichi nuclear accident”, Beyea et al., Energy Environ. Sci., 2013, 6, 1042). We believe that it is thus prudent to not apply a specific morbidity/mortality model ourselves, but present our results in a format that is appropriate and open to subsequent evaluation.

-Page 7, line 26: Please correct "lifetime" to "half-life"

+Changed.

* Referee 2:

General Comments This manuscript provides an assessment of the probabilistic, relative risk of nuclear power plant simulate inhalation and ground deposition doses worldwide from radionuclides assuming a constant 1 PBq emission from all power plants. While the methodology is sound and the tools used are appropriate to address the scientific question, I am concerned with some of the authors' assumptions, how the experiment is formulated, and how results are presented. The authors use a 1 PBq emission source for all power plants and a constant emission rate over a 20âĂŘyear period to account for different meteorological conditions. Yet, the experimental set up does not allow for dosage rates to be attributed to meteorological conditions or emission sources because neither are held constant in the experiment. In other words, one cannot determine if high dosage rates in a given area are due to meteorological factors or local emission sources. Because the experiment is meant to show the relative risk of radiation worldwide, I would suggest that all figures should not be presented in terms of Bq or Sv, but instead using some relative risk index, such as in Figure 5. A continuous 20âĂŘyear 1 Bq emission rate from all power plants is unrealistic, and so the actual values of radiation or dosage do not have relevance. Instead, it may be more useful to create/use a unitless metric to describe the results. In addition, one of the primary assumptions of the study is that the risk of an accident, and the emissions from an accident, are equal for older operational plants and newer planned plants. The authors cite that PRAs are not available, as discussed in Lelieveld et al. 2013. While the risk of an accident may be very speculative, I would imagine that the probability that the accident will be contained is higher for newer plants planned or in construction. At a minimum, the discussion of risks needs to be expanded in the manuscript. Also, this concern supports the suggestion above to present all results in terms of a relative risk index, not a 1 Bq emission rate.

+ Agreed. We have reformatted the presentations of our results to address all the points raised by the referee. We believe that this improves the clarity of the paper.

Specific Comments 1. “mixing” is misspelled in the abstract

+Spelled as "mixing".

2. The authors assume constant continuous emissions over a 20âĂŘyear time period to account for different meteorological conditions. However, it would be beneficial to be able to quantify the range of concentrations based on these meteorological conditions. This is somewhat addressed in the discussion of different seasons, but I would suggest that the authors present results which highlight low or high years between 2010âĂŘ2030 for different areas of the world.

+ We have added plots of the global coefficient of variation (standard deviation over mean value) to illustrate regional temporal variability.

3. Does the continuous 20 year emission rate generate similar results to averaging 1040 1 week simulations, which is more realistic? If so, please explain in the text.

+ Our study uses a 20-yr constant emission rate for the simulation for climatic representability. Our results do not realistically portray the outcome of any individual NPP accident under specific meteorological conditions but aim to estimate the risk from all potential atmospheric states. One-week simulations would not capture the longer-scale transport of radionuclides.

4. The authors call out Sec 4.2 in the last sentence of the Introduction, but do not discuss other subâĂŘsections of Section 4. Please be consistent.

+ This was also commented on by the other reviewer and the last paragraph of the introduction was removed.

5. Please describe the depth of the bottom few pressure levels in the model when the 31 pressure levels are mentioned in the text. The surface layer is defined as 30 m off

the ground. Have the authors performed sensitivity simulations to the nearâĂŘsurface vertical resolution, and if so, please discuss briefly in the text?

+ The EMAC model uses a hybrid system for specifying the levels of the data. The system combinesthe constant pressure level system with the sigma level system based on surface pressure such that closer to the earths surface, the levels more closely resemble a pure sigma level, while the higher up you go, the more the levels are like constant pressure levels. More information is available in Ritchie, H., Temperton, C., Simmons, A., Hortal, M., Davies, T., Dent, D. and Hamrud, M. (1995) Implementation of the semi-Lagrangian method in a high resolution version the ECMWF forecast model. Mon. Wea. Rev.,123, 489-514. Emission height is discussed in comment 7 below.

6. Please include a reference where a mean radius of 0.25 um for CSâĂŘ137 was assumed.

+ Added reference.

7. The authors have not accounted for plume rise due to fires, and have put all tracer release points at a pressure level of 1000 hPa, which I'm assuming is the lowest pressure level in the model (See Comment 5). Please quantitatively support this assumption (e.g. show sensitivity simulations that suggest results are not significantly affected by the injection height, or evidence that emissions from previous accidents were not affected by plume rise?) This assumption has the potential to significantly affect the percentage of radiation that is deposited/dispersed locally versus globally.

+ Due to the required computational resources it is not feasible to simulate varying emission height profiles. Over all past accidents registered in the IAEA INES scale, only one major accident resulted in an open fire over an exposed core (Chernobyl) and the graphite core material which burned is deprecated technology. Accidents that are lower in the INES scale (and much more numerous in occurance) more likely relate to radioactive leaks at surface level. To account for different likelihoods, we use a point source at 1000 hPa (equivalent to a height of approximately 100m above the surface).

For model injection height sensitivity analysis, we refer the reviewer to the study by N. Evangeliou at al., Atmos. Chem. Phys., 13, 7183-7198, 2013.

8. Please move the discussion that ingestion pathway is not included in the study from the Results section to the Methods section. Also, please support the statement with a reference or argument: "It can be assumed that food intervention measures will prevent significant doses to the population due to ingestion..."

+ Added reference.

9. Please be clearer throughout the paper with the terms nuclear power "station", "plant" and "reactor". Is each reactor being treated as an individual emission source, or are the plants (containing multiple reactors) being treated as individual emission sources? If the reactors are treated as individual sources, please defend why the authors assumed that the risk of an accident wouldn't increase for reactors at the same plant, or in the case of assuming a plant is an individual emission source, why plants with multiple reactors would have the same emission rates as plants with one reactor?

+ As is mentioned in the manuscript text, we make no assumption on the type, capacity, reactor core count to asses the probability of an accident happening at each particular location or the total emission magnitude. By employing unit emission source, and quoting our results per 1pBq, we assess the comparative potential risk and provide a scale for the absolute size of any one accident. It should be also noted that we aim to capture the risk patterns based on climatic behaviour and not any one individual meteorological event. We have changed all references to "nuclear power plant" in order to be more accurate and consistent.

10. In the simulation, IâĂŘ131 should be removed by other mechanisms besides radioactive decay, such as dissolution, dry deposition, etc.

+ Removal through dry deposition and particle sedimentation only contributes to a 5-10% effect (not considering particle resuspension). I-131 is treated as being purely in the gas phase in our model, and is removed from the atmosphere via radioactive decay. This allows for the reduction of computational complexity and for the direct comparison of gaseous and aerosol components of radioisotopes and is a valid approximation as the atmospheric gaseous to particulate fraction is estimated to be close to a factor of four by a number of relevant measurements.

11. Reference Figure 3 in the first sentence of the 3rd paragraph of section 4.1

+ Fig. 3 referenced.

12. “Extend” should be “extent” in the 3 rd paragraph of Section 4.1

+ Corrected.

13. Paragraph 4 of Section 4.1, and the Figure 4 caption, state that the surface deposition in Figure 4 is scaled to unit PBq per station. Isn’t this how the experiment was set up – unit PBq per station? Have the results been scaled again in some way? Please clarify in the text.

+ This is indeed how the experiment was set up. The results have not been scaled in any other way. Clarified in the text.

14. Figure 2, Figure 3, and Figure 4 contain mSv and kBq, however the discussion of mSv doesn’t appear until Section 4.2. Please reorganize the paper so that the discussion of dosage occurs before the figures are introduced.

+ Restructured the text so that the discussion of doses occurs before the figures are introduced.

15. Figure 2 and Figure 4 would benefit from a fourth panel that shows OP+UC+PL.

+ Added fourth panel to show totals to Figs 2 and 4.

16. Do the 50âĂŘyear ground deposition doses take into account weathering? Please explain in the text.

+ The method for the calculation of 50-year integrated effective doses from ground deposition, including consideration of resuspension, weathering and ground roughness can be found in INES 2009, Appendix I). The integrated dose conversion factors, per unit ground deposition of each radionuclide, include the external dose and committed dose from inhalation (resuspension) resulting from remaining on contaminated ground for lifetime (50 years) (IAEA-TECDOC-1162). This is now explained in the text.

17. The acronyms OP, UC, and PL do not appear until Section 4.2 in the text, and they are present in previous figures as well as the supplementary information. Please define earlier in the manuscript.

+ Defined in introduction.

18. The figures showed that the global distribution of dosage from ground deposition is quite different than the global distribution of dosage from inhalation. I would suggest combining both pathways for the computation of the relative risk index in Figure 5.

+ Added both pathways to the figure.

19. For the Figure 5 caption, change left to bottomâĂŘleft and right to bottomâĂŘright.

+Changed.

briefly mention what information is contained in the supplemental information in the manuscript.

+ Supplemental information contents are now mentioned in the manuscript.

21. Similar to Comment 13, please clarify why some figure captions (in the manuscript and supplemental info) are presented as “unit PBq per station” and “unit PBq atmospheric load.” In addition, “1 PBq/station atm load” and “1PBq / station” are shown in the upperâĂŘright corners of the figures in the manuscript. These descriptions are confusing – please clarify in the text.

+ Changed captions to clarify.

22. In the Summary, the sentence, "...due to the long decay lifetime of CsâĂŘ137 compared to the short timescale of the atmospheric removal processes considered, its radioactive decay can be neglected..." is not correct as written since this applies to the inhalation pathway but not other pathways.

+The sentence refers to the removal processes of Cs-137 from the atmosphere and not to the human dose pathways.

Please also note the supplement to this comment:
http://www.atmos-chem-phys-discuss.net/13/C12618/2014/acpd-13-C12618-2014-supplement.pdf

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Interactive comment on Atmos. Chem. Phys. Discuss., 13, 30287, 2013.