

Review of “Global Risk from the atmospheric dispersion of radionuclides by nuclear power plant accidents in the coming decades”

General Comments

This manuscript provides an assessment of the probabilistic, relative risk of nuclear power plant accidents from both operational and future nuclear power plants. The authors use the ECHAM model to 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 20-year 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 20-year 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.

Specific Comments

1. “mixing” is misspelled in the abstract
2. The authors assume constant continuous emissions over a 20-year 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 2010-2030 for different areas of the world.
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.

4. The authors call out Sec 4.2 in the last sentence of the Introduction, but do not discuss other sub-sections of Section 4. Please be consistent.
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 near-surface vertical resolution, and if so, please discuss briefly in the text?
6. Please include a reference where a mean radius of 0.25 μm for CS-137 was assumed.
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.
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..."
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?
10. In the simulation, I-131 should be removed by other mechanisms besides radioactive decay, such as dissolution, dry deposition, etc.
11. Reference Figure 3 in the first sentence of the 3rd paragraph of section 4.1
12. "Extend" should be "extent" in the 3rd paragraph of Section 4.1
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.

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.
15. Figure 2 and Figure 4 would benefit from a fourth panel that shows OP+UC+PL.
16. Do the 50-year ground deposition doses take into account weathering? Please explain 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.
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.
19. For the Figure 5 caption, change left to bottom-left and right to bottom-right.
20. There is no reference to the supplemental information in the manuscript. It would be helpful to briefly mention what information is contained in the supplemental information 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 upper-right corners of the figures in the manuscript. These descriptions are confusing – please clarify in the text.
22. In the Summary, the sentence, "...due to the long decay lifetime of Cs-137 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.