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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Global risk of radioactive fallout after nuclear reactor accidents" *by* J. Lelieveld et al.

Anonymous Referee #2

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I wish I could deliver a review as enthusiastic as those that I have seen so far. Unfortunately, although I think that the idea of the paper is good, the work is a bit rushed over and there are series of issues that, in my opinion, are of paramount importance, that are treated in an excessively cursory way.

I want to be clear, the topic is very relevant. In spite of the latest events, it has been in fact relevant for years and will be even more relevant in the coming decades with the renovated attention on nuclear power in many regions of the world. The approach is interesting too but I am afraid treated with not the sufficient caution and attention that a quantitative analyses requires. The fundamental message is also clear and in a way it could also be brought forward almost independently of the atmospheric dispersion analysis. If the atmospheric dispersion results have to be the central element of the

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Interactive Discussion

Discussion Paper

paper, then the authors have to consider elements that could, obviously not change the take-home message, but influence sensibly their quantitative analysis.

I will avoid any analysis form the reactor engineering point of view which would be a far too easy way to turn down this paper. There are sufficient elements that require serious attention by the authors that tangentially relate to engineering issues and that are going to impact seriously on their atmospheric dispersion analysis.

The very large number of modeling studies and experiments that were performed since the Chernobyl accident, as well as fundamental point-dispersion theory, identify as critical parameters for the dispersion and consequently (dry-) deposition:

1. the amount of mass released per unit time (that is the scaling factor) 2. the duration of the release 3. the height of the release

The authors release a Chernobyl equivalent amount of activity for Cs and I for one year therefore:

- Considering all reactors Chernobyl equivalent and having the same technology of Chernobyl. Using the Chernobyl source terms (in Bq) for all the existing NPPs in the world is a too gross simplification. - Furthermore in Chernobyl the same mass was released in 10 days! The continuous 1-year release is not suitable to identify consequences for accidents which are associated with short releases (from hours to some days), even if the interest is on risk. - The seasonal dependence of the dispersion patterns is completely masked - Assuming that the release occurs in the boundary layer (although I have not found trace of the effective or real emission height used).

Any quantitative conclusion driven without a sensitivity analysis on at least these four parameters makes your effort futile and severely reduces in my view the significance of your work. Indeed you present a map that can be analyzed and conclusions could be driven but there is no indication whatsoever on the robustness of the results. As far as the conseuqnces of your conclusions are already clear: my fellow reviewer is already

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planning to move out of his living area. I would say, hold on for a moment still. Any dispersion model would disperse, no matter what, when and how one releases it. My experience tells me that especially with point releases for which a plume goes through a large series of stages before becoming a dispersion field, those initial conditions can be crucial and change your final map a good deal. Your conclusions, your general conclusion could be still valid, but the quantitative details could be substantially different simply by perturbing the above mentioned four parameters. Should it be not the case evidence should be provided.

There are studies conducted in the past on the European region like the RISKMAP project (http://www.umweltbundesamt.at/fileadmin/site/umweltthemen/kernenergie/Riskmap/English/Main.htm) and the most recent flexRISK (http://flexrisk.boku.ac.at/), as examples, that are totally ignored even in the approach used there in and why a similar approach was not adopted in this study.

The evaluation of the model on the Chernobyl case needs to be quantitative, the credentials of the scientists involved in this study are undisputed but I am afraid it is not enough to show a comparison where only the model results presented are those of the authors and not even the Smith and Berensford map is shown in copy and it is not accessible unless one buys the book. On top of that it is not true that the data are not available. Both deposition and atmospheric concentrations are publicly available (at: http://rem.jrc.ec.europa.eu/RemWeb/Browse.aspx?path=Chernobyl Data) since many years. Literally tens of model results have been compared against those data prior to be used in forecasting, support to decision making or planning.

I assume it was very well known before Lawrence (2007, in reality it is 2006!) that vertical transport in the tropics is more intense than in the mid-latitudes. Such kind of arguments does not seem to be sufficiently corroborating the quality of the results, one thing is the internal consistency of the behavior of a model, the other is a verification of the model fitness to the purpose.

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Interactive Comment

The 1990 probabilities published by NRC are not strictly independent which would not allow a strict multiplication, the over all probability would therefore be largely reduced. The NRC analysis is much more detailed than those few points reported and needs, if accounted for, to be considered with more care and attention.

The paper is too conclusive to list in the end all the other relevant factors that needed to be accounted for in the first place. All those factors are very relevant and are going to modify substantially your quantitative conclusions and risk maps.

As I have said at the beginning the paper contains important elements of relevance but it looks to me more a research proposal rather than a conclusive study. Should the authors wish to maintain it in this form I'd rather opt for a different allocation than a research paper. Your final message is clear and I support the idea and the motivations, I am afraid I cannot support the way in which you want to convince the reader.

Minor remarks

p.31208 line 14: suggest "...human exposure due to deposition..." p.31210 line 2: burn up of the fuel is also a very important factor, unless it is implicitly included under fuel line 12: equivalence of 1 between Gy and Sv is only true for gamma and beta radiation line 15: suggest "graphite moderated" in stead of mediated p. 31211 line 6: radiologically very important as I is concentrated in the thyroid, where it can lead to high doses line 8: sources for table1 (internet pages and dates) are missing p. 31212 line16: a large fire is a relative description, however Chernobyl burnt for 10 days, hence seems to me that it is open for criticism if used in this study p.31214 line 26: scientific notation not consistent: suggest > 37 kBq m-2 137Cs p. 31216 line 21: Cs-134 has half-live of about 2 years, hence not decades line 28: idem as line 26 on p. 31214 p. 31219 line 2: safety culture is essential (could be added to this line)

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 31207, 2011.

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