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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Comment on "Global risk of radioactive fallout after major nuclear reactor accidents" by J. Lelieveld et al. (2012)" *by* J. Lelieveld et al.

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Review of the manuscript entitled 'Comment on "Global Risk of radioactive fallout after major nuclear reactor accidents" by J. Lelieveld et al. (2012)', by J. Lelieveld, M. G. Lawrence, and D. Kunkel

The authors have submitted a short manuscript in the form an FAQ, addressing some questions in relation to their article published previously in ACP (Atmos. Chem. Phys., 12, 4245-4258). This text does not contain new scientific findings, nor is it a corrigendum to the earlier publication. It seems that the authors have received questions frequently and want to answer them in the present form. In my opinion, this does not constitute material that justifies publication in a scientific journal, especially as the questions and criticisms have not been submitted formally and are not being published in ACP.

This FAQ would be more appropriately placed on the authors' web site. In any case, it has now already been published as a discussion paper, which makes it widely accessible and easily findable. I don't think it needs to be promoted to the status of an ACP paper.

As for the scientific content of the FAQ, I can offer the following comments.

Ad Q1. There is no consensus about the emissions from the Chernobyl disaster. Publications give wide margins of uncertainty, and inverse modelling studies have added their own estimates. In any case, I think the question whether a consensus exists or not has little relevance for the selection of a source term for a generic nuclear risk study. In this case, one should rather aim at the most representative source term. The Chernobyl release fractions are rather on the higher side of typical severe accident scenarios, but within their range.

Ad Q2. I do not think that the uncertainty of the emissions from the Fukushima disaster is much larger than that of Chernobyl. See also comment to Q1.

Ad Q3. The authors avoid answering the more important question, namely the statistical significance of a probability inferred from two (or four, if you want) events.

Ad Q4. Applying a threshold value of 40 kBq Cs-137 / m2 is a reasonable choice. However, it is obviously not the only relevant contamination level. The authors refer to an IAEA paper, but don't discuss the significance of that level. It is a level that would trigger monitoring activities and some measures, mainly with respect to agricultural production, but it would probably not entail long-term consequences such as land-use restriction. It is a value that has been exceeded in several countries after the Chernobyl disaster. A more comprehensive risk assessment would take into account other levels

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as well.

Ad Q5. I would agree with the authors that their results are probably not very sensitive to the release height. However, their rationale for 0-60 m release height is not very convincing. Why do they want to exclude explosions? Hydrogen explosions are a recognised risk factor in nuclear accidents, and Fukushima has given ample evidence for that. Even if there were no explosions, it is obvious that nuclear accidents are associated with not small amounts of energy which may cause thermal plume rise. Also release through the exhaust stack is possible.

Ad Q7. The answer to the question about other “risk assessements associated with major nuclear accidents” is not clear because it does not differentiate between assessment of possible accident sequences and assessment of environmental consequences. Furthermore, the status of probabilistic safety assessments (PSA) is not considered which is a standard procedure in nuclear engineering.

Ad Q8. See comment on Q7. This is basically a nuclear engineering question, a field that falls outside the authors’ expertise. They don’t appreciate the large number of PSAs that have been carried out since 1990. Additionally, the possible differences in probability for core melt and for large release aren’t discussed adequately.

Ad Q9. The question whether the three core melt events at Fukushima-I can be counted as three independent events for statistical evaluation is not adequately addressed. Obviously, the probability for a large release caused by an external event is the product of the probability for this event and the probability of a large release as a consequence of the initiator. While for the latter part we may count the three units with core melt & large release separately, obviously this does not hold for the tsunami. However, given the crude statistical methodology applied by the authors on one hand, and the fact that in nuclear risk studies we are discussing orders of magnitude and not focus much on factors of two on the other hand, what is the relevance of that question?

Ad Q10. The answer given by the authors to objections related to the spreading of the

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release over a whole year, and using the resulting contamination at the end of the year for comparison with a threshold value is not convincing:

1. Basically, the authors just repeat the explanation from their previous paper. They don't offer new arguments.
2. Even a week is not the typical time scale for the peak of emission in a nuclear accident, which is rather on the order of a few hours, though this may vary.
3. If authors average their weekly results over the whole year, they are just repeating the main calculation with a different discretisation strategy. Apart from numerical uncertainties, results *must* be identical, and that is what they showed in their Figures 8 and 9 (relative deviation) of the previous paper: agreement plus some stochastic fluctuations. The real concern, which is probably not understood well, is that the contamination threshold of 40 kBq/m2 should be applied to individual cases, not to the mean over all possible cases. The "risk" of the original paper is not a risk in the sense of a probability for a defined damage nor as a product of damage and probability. The patterns would be the same if, instead of 40, 10 or 1000 kBq/m2 would be used, as the value is basically used for scaling. The practical meaning of this main output parameter is not well characterised, neither in the main paper nor in the FAQ.

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 19303, 2012.