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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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We thank Dr. Seibert for her constructive and helpful comments.

We note that ACP allows the category of "peer-reviewed commentaries". The questions and criticisms are submitted formally and will remain on the ACP website, e.g., to provide additional insight into controversial issues with respect to the original publication. The revised FAQ will not receive the status of an ACP paper. We will clarify this in an introduction, which is requested by referee #2.

Reply to Ad Q1: We acknowledge that "consensus about the emissions by Chernobyl" needs reformulation. In the revised FAQ we will add "The emissions by Chernobyl have

been adopted in many previous publications and were presented as “revised estimates” by IAEA (2006). Furthermore, Davoine and Bocqet (2007) re-assessed the Chernobyl source term by inverse modeling and found that their results are in good agreement with the latest reported emission estimates with only a minor difference in their temporal representation. Nevertheless, it may be assumed that substantial uncertainties remain”.

Davoine, X., and Bocquet, M.: Inverse modelling-based reconstruction of the Chernobyl source term available for long-range transport, Atmos. Chem. Phys., 7, 1549-1564, 2007.

Reply to Ad Q2: For Fukushima the number of publications about emissions is much less, though we acknowledge that a comparison about uncertainties of emissions by these two accidents should be avoided. This will be reformulated into “The emissions from Fukushima are currently associated with a high degree of uncertainty, and are subject of scientific debate”.

Reply to Ad Q3: Although it is difficult to address the statistical significance, we have extended the discussion in Q9 (see below).

Reply to Ad Q4: We agree that the threshold value we used is reasonable but that a more comprehensive risk assessment would also account for other levels. Discussing their significance would be very valuable, but we feel that this needs additional expertise and study. We actually refer to “a discrepancy in the literature that needs to be resolved”. We also mention “If we would apply this definition of IAEA (2005) strictly, we would have to add the deposition of substances such as 131I (though with much shorter half-life) and 134Cs, which would increase the calculated risk of contamination accordingly”.

Reply to Ad Q5: We acknowledge that the release height is an issue. We have discussed it in the original publication and in Q5, notably also after debating it with the reviewers. We will add a reference, mentioning “This assumption leads to a conserva-

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tive estimate of long-distance transport, which we tested in previous work (Kunkel et al., 2012)".

Reply to Ad Q7: We agree that the answer to the question is not clear, though partly due to the formulation of the question. We will change it into "Q7. Have risk assessments of major nuclear accidents been performed previously? We emphasize that we do not address the environmental consequences. We will complement the text under Q7b: "For individual reactors a probability risk assessment (PRA) is performed, which is a standard procedure in nuclear engineering; however; the results of PRA's are not available to us (i.e., not publicly available)". We use PRA (not PSA) to be consistent with the recommendations of referee#2.

Reply to Ad Q8: The performance of PSAs has been added to Q7. We agree that nuclear engineering is not within our field of expertise, therefore we do not address the issue in detail. Much would be gained if more information about the safety conditions of individual nuclear power plants would become available to the public, or at least in the scientific literature.

Reply to Ad Q9: We have addressed the issue of statistics in the discussion with Ehlermann, indicating that a parametric distribution based on extreme value theory would be desirable. For this purpose we are currently collecting additional information, hoping that we can improve this aspect of our study in future. We will also add the following text to clarify our answer to Q9: "One must also be aware that three of the 5 active reactor blocks (6 in total) in Fukushima suffered a meltdown, so it would also be incorrect to treat them all as a single unit. We could have also computed the statistics using plants as the unit, rather than reactor blocks, counting Fukushima as a fractional plant meltdown. Then scaling by capacity as we have done would give us principally the same answer, but be less transparent in the computation. It is worth noting that NRC (1990) also assumed that reactors are independent; i.e., following the method by NRC (1990) leads to an underestimate of the risk of nuclear power plants with multiple reactors."

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Reply to Ad Q10: We agree that we do not present new arguments, but feel that this is not needed. The model results presented in figures 8 and 9 of our original publication illustrate that the differences between annual and weekly emissions (same total source) are small. It would not be useful to perform the same test with hourly emissions, although the point that this may be more realistic is well taken. These types of assumptions, needed in a risk assessment (similar to the choice of emitting the radioactivity in the lowest layer), are not critical but must be documented clearly to enable reproducibility. We agree that in individual cases the exceeding of the 40 kBq/m2 threshold may differ greatly. We also agree that the patterns are the same for different thresholds, e.g., illustrated by figures 2-5 in our original publication, thus allowing that different thresholds are represented by a color scale. We feel that we have transparently explained our assumptions and the method applied, but are open to constructive critical suggestions in case it is perceived that these explanations could be made more understandable to colleagues from other related fields.

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