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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.

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We thank the referee for the comments that help improve our manuscript. Although there are some comments that we do not entirely concur with, we would agree that in most cases improvements can be achieved by explaining more clearly what has been done and for what reasons. Here we discuss in particular how we address "1. the amount of mass released per unit time (that is the scaling factor); 2. the duration of the release; and 3. the height of the release".

On p.C13541 the referee mentions: "Considering all reactors Chernobyl equivalent and having the same technology of Chernobyl". We agree that one cannot consider all reactors and technology equivalent, as we state in section 7: "In particular, a better understanding of reactor risk profiles and expected release of radioactivity in case of

a meltdown is required. . ..". However, since no other information about emissions from INES 7 accidents is available than for Chernobyl, we use this source term as a proxy for this first study of this nature because it has been relatively well-documented. Since INES 7 accidents are rare, the "typical" release of radioactivity is not known. Furthermore, although treating all NPPs as Chernobyl is a necessary simplification for this study, it is a transparent assumption that can be easily adjusted with the availability of new and improved information. Unfortunately, even for the INES <7 accidents emission data are close to absent (see Table 1), so that we cannot use those to develop a better impression, until the post-Fukushima analysis becomes sufficiently advanced to provide new data for INES 7 accidents. Furthermore, although we considered various ways to modify the emissions, e.g., the emissions according to reactor capacity, the information needed to do so without introducing even greater uncertainties (and reducing transparency) is also not available. A central message of our work is that the understanding of these emissions should be improved, and based on this initial risk assessment, this appears worth the effort.

Whenever the Fukushima accident will be similarly well-documented as Chernobyl, the calculations could be repeated. To avoid confusion about this aspect we have added the following text to section 7: "Note that the transport and deposition calculations presented here can be regarded as "scalable" because the modelling of 137Cs is a near-linear tracer experiment, as long as the tracer lifetime is significantly longer than the lifetime of the aerosols on which they travel and the statistics of the meteorological conditions are representative. For example, if 50% or 10% of the 137Cs release by Chernobyl would be more representative for modern NPPs, the deposition and contamination risks could be scaled down by a factor of two or ten, respectively. On the other hand, if one wishes to account for additional radionuclides such as 134Cs (the Chernobyl release of radioactivity by 134Cs was about 50% of 137Cs) the 137Cs results could be scaled by a factor of 1.5 based on Chernobyl."

For the discussion about the deposition time period, we refer to our reply to ref#1: By

integrating the model calculated deposition over a year we capture the total deposition of 137Cs. For our risk assessment it is not directly relevant that in reality the deposition occurs over a much shorter period. The total deposition per year at any location is not dependent on the duration of the emission period, but rather on the meteorological conditions. By taking a year we capture all possible meteorological conditions, which is important as major accidents could occur during any season and under any type of meteorological situation. In the revised manuscript we explain this more clearly: "The duration of the emission period is not decisive for these calculations since the total deposition onto the ground is the appropriate parameter for our risk assessment, and by integrating over a year we capture the annual range of meteorological conditions, thus providing a statistical representation of the atmospheric transport and deposition pathways over the different seasons. In reality the total deposition may occur over a much shorter time period, as was the case with the Chernobyl accident. However, for our calculations the total deposition within one year is relevant, rather than the actual time period of any individual accident (below we assess the contamination risk per year)."

The referee has possibly overlooked our description about the tracer release height on p. 31212, l.14-17: "In the model we emit the 137Cs on aerosol particles . . . by introducing them into the surface layer of about 60m depth. We assume that 131I and 137Cs are released gradually and not explosively or by large fires, which would increase the emission height and generally enhance the long distance transport". Indeed, in the case of explosive emissions or by large fires the tracer transport and deposition fields would change. In most cases, we would not expect this difference to be as large as it should be for the Chernobyl accident, since non-graphite reactors are less likely to burn than Chernobyl. In general, the sensitivity to this assumption for the convective boundary layer will be small, but for the stable night-time boundary layer this will be different, and release of the radioactivity above 60 m would cause larger areas to be affected by $\geq$40 kBq/m2 deposition. As noted above for the total emissions, this is a point that clearly warrants further dedicated research by the community.

The critical paragraph on p.C13541/13542, following "Any quantitative conclusion. . .", indicates that the referee has overlooked or misunderstood several points, partly addressed above. Though we agree that tracer transport in deterministic plume calculations can be sensitive to the stages of dispersion, this is much less the case in our integrated, long-term approach. For example, the results of an ensemble of multiple plume dispersion calculations would also not be sensitive to the effects of individual events.

Furthermore, we have performed multi-year tracer simulations to investigate the interannual variability, and find that the deposition maps are quite robust. In the revised manuscript we mention "To test the representativeness of the year 2005 we performed multi-year tracer tests for a sub-set of the tracers, and find that the transport patterns and deposition fields are not sensitive to this assumption". We nevertheless agree that quantitative details can matter, and this is particularly true for the emission estimates. Again, these details are not known, and for that reason we make a number of recommendations to address the reactor risk profiles in section 7.

We have not ignored past studies of the European region like the RISKMAP and flexRISK projects. However, these studies pursued different objectives than the present work. RISKMAP primarily reports radioactivity levels in airborne particulates, surface water, etc. for the countries of the European Union since Chernobyl. FlexRISK provides valuable information about definitions, methodologies and plume modeling tools, but not on radioactivity emissions and deposition risks associated with major accidents. The website mentions: "Publications in scientific journals in the fields of energy, risk sciences, meteorology or radioecology as well as conference participations are planned and will be announced and documented on this website".

The discussion about vertical transport in the tropics, being more intense than in the mid-latitudes, was not intended to corroborate but rather to explain our results.

We agree that "the 1990 probabilities published by NRC are not strictly independent",

which is one reason why we are critical of this report, and why we recommend addressing “Enhanced risk for plants with multiple reactors and shared technical facilities” in section 7. The NRC published many valuable and excellent reports, though did not adequately address the deposition risks after major NPP accidents.

We agree that our study is not conclusive. Much work needs to be done to improve the assessment of exposure risks after major (and also minor) reactor accidents, which primarily requires that the emissions are characterized better.

The minor remarks have all been included. Wrt to the consistent notation we define $\geq$40 kBqm$-$2 as “contaminated”, following the definition by IAEA (2005). We express this more clearly in the revised manuscript.

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