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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Xenon-133 and caesium-137 releases into the atmosphere from the Fukushima Dai-ichi nuclear power plant: determination of the source term, atmospheric dispersion, and deposition" *by* A. Stohl et al.

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Dear colleagues,

We thank you for the time you have taken to read our paper and comment on it from an engineering perspective. The paper has indeed received a lot of media attention, following the news article published in Nature, at a time when this paper has not been reviewed yet. This may be seen as a weakness of the open peer review system but the fact that we can discuss and consider for the final version of the paper your arguments,

coming from a very different discipline than our own, also shows the strength of this system, which invites comments from all qualified scientists, regardless of discipline.

In the following, we address your individual points:

- Regarding the total release of Xe-133: Our results suggest a total Xe-133 release of 16.7 EBq, and inversion experiments with variations of the a priori and information and the meteorological data showed variability of about 20%. That would indicate a release between 13.4 and 20 EBq. The best estimate is considerably higher than the total Xe-133 inventory which according both to our own calculations and the Japanese Government Report is 12.6 EBq. In the paper, we admit that "this may indicate that our inversion overestimates the emissions. This could occur, for instance, by a broadening of emission peaks while retaining peak emission rates to match particularly high measured concentrations." However, our lower estimate, considering the uncertainties, of 13.4 EBq is only 6% higher than the presumed inventory. Apart from the obvious explanation that the uncertainty of our results just needs to be estimated a little larger to remove this inconsistency, we suggested as possible alternative explanations recriticality (admittedly, this is unlikely and it would not explain 4 EBq) or emissions from other nuclear power plants. It is not entirely clear whether on-site radiation monitors at other NPPs would have captured such releases. Winds were westerly during the first few days after the earthquake and releases from power plants in eastern parts of Honshu would have been carried out to the North Pacific Ocean quickly, possibly without being metered. We want to emphasize that we do not believe that these releases have to be of the order of 4 EBq. Considering our uncertainties, the amount under discussion could be about 1 EBq or even less and this might well have escaped the general attention. We can't and don't want to claim that our results prove releases exceeding the FD-NPP unit 1-3 inventories, but we want to point out that there is some evidence. Finally, this discussion assumes that the inventory is exactly known, which is not the case. The exact core

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inventory, which should be known by the plant operators has not been published as far as we know. A change in the core inventory would translate into a corresponding change in the a priori. If such a change does not exceed the order of 10% (what we believe to be the uncertainty due to the fuel history), we believe that this would not alter the a posteriori significantly (i.e., beyond its confidence interval). However, it could explain some of the differences between our current a priori and the a posteriori.

- The different burn-up for units 2 and 3 suggested by Grasso et al. in their comment (23000 MWd/tU instead of the 30000 MWd/tU we have used) increases the estimated Xe-133 inventory of units 2-3 by less than 3%. In fact, using 94 tU instead of 98 tU for the units 1-3 leads to a decrease in the Xe-133 inventory, so that final numbers are just 1% lower than what we specify in Table 2. This will have an entirely negligible effect on the inversion, since even a 50% reduction of the a priori emissions leads to a change of our a posteriori estimate of only 4%.

  The impact on the Cs-137 inventory is larger, leading to a reduction of about 27% of the inventory for units 1-3 (but notice that SFP inventories remain the same unless one would assume a different maximum burnup as well). In Table 6, we have shown that a 50% reduction of the a priori emissions leads to a corresponding reduction of the a posteriori emissions by 14% (Table 6). Interpolating this result linearly, a 27% reduction of the a priori emissions would impact the inversion result by 8%, but because of non-linearity, it would actually be less than that.

  For the final version of this paper, Cs-137 inversions will probably be re-done, also in the light of corrections by MEXT to a few deposition measurements. We also expect the deposition data changes to have minor effects on the inversion, but they would rather lead to emission increases.

- Regarding the early start of Xe-133 emissions, see our detailed reply to the comment of Horst-Michael Prasser. In essence, the paper never claims Xe-133 re-

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leases in the time interval of the 45 min between the earthquake and the arrival of the tsunami (this is impossible to do with the data and method we apply; the 3-hourly resolution at which we derive our emission data alone would prevent us from saying something about events on time scales of one hour). This was only an interpretation by some of the mass media, especially those who covered our study without giving us an opportunity to correct such mis-interpretation. However, we do suggest that emissions have occurred early, in the sense that they must have started before the time when the first venting was done. We will try to make that more clear in the revised version of the paper, including also some of the additional evidence presented in our response to H.-M. Prasser.

- The emission drop is a robust finding. As shown in Fig. 6, it is obtained regardless of the measurement data set used (Japanese deposition data, Japanese concentration data, data outside Japan, or all data sets combined). It is also obtained with different meteorological data. Again, we cannot derive the timing with an accuracy of one hour, of course, but rather of some 3-9 hours. From our study we cannot distinguish between emissions from different reactor units or reactor cores and spent fuel pools. Our suggestion that emissions from the spent-fuel pool of unit 4 are responsible comes from the striking temporal coincidence of the decrease of emissions with the spraying and the fact that this pool had the highest loading. Other explanations for the continued emissions on 16-19 March may be possible but need to be given by engineers. However, regarding your argument that there is no visible damage to the fuel elements in the spent fuel pool of unit 4, it has also been suggested that the photographs published by TEPCO provide no proof that no emissions have occurred (Christoph Müller, personal communication; see www.tec-sim.de/images/stories/spf-fa3.pdf, pages 57-60).

Finally, we would like to remark that our results are based on ambient measurements and atmospheric transport modeling. In the paper, we offer some possible explanations for our finding of, for instance, early start of Xe-133 emissions and higher total

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Xe-133 emissions than the inventory suggests, or the drop in Cs-137 emissions from 19-20 March. Some of the explanations may seem unlikely from an engineering perspective (e.g., recriticality, emissions from other power plants) but, to our knowledge, no better explanations are currently available. We encourage engineers to take our results serious, as we believe they are robust to the extent explained. Hopefully, this will eventually also lead to a better understanding of the accident events.

Kind regards,

The authors

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 28319, 2011.

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Interactive Comment