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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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We would like to thank reviewer 1 for the comments on our paper and especially the various internet links to valuable additional resources of data, not all of which we were aware of. This has certainly helped with the revision of our paper.

The reviewer's comments are mainly concerning regional-scale issues, while we have taken a global approach to the problem, using data from stations worldwide. We have also used data from Japan, since these data are important also for the global-scale

inversions but we fully share the reviewer's opinion that a regional-scale model with high resolution would do a better job in simulating the dispersion over Japan. However, since the radionuclide transport was often directly to the Pacific Ocean, inversion studies with regional models alone will not be able to constrain the source term for the entire duration of the accident (rather only for short periods of time when the plume was transported over Japan – admittedly, these are important periods). In fact, one of our recommendations at the end of the paper is that studies with nested models shall be conducted, which can provide high resolution over Japan so that better use can be made of the data from Japan to refine the source term, and consequently to study the impacts in Japan.

There is also the important question which resolution is actually needed to resolve the complex topography of Japan. Arguments have been made by Dr. Takemi at the Annual Meeting of the Meteorological Society of Japan that a resolution of 400 m is needed to resolve the complex flow in the area of the NPP, which extends at least to 200 m above the ground. This resolution exceeds considerably what is available from typical regional models. Thus, we think such simulations need to be left to future studies. A detailed evaluation of the results on a regional scale is outside the scope of this study. We have made publicly available our results so that regional modelers can test our source term and explore possible errors.

In the following, we respond to the concrete major reviewer comments point by point. We do not repeat the reviewer comments here, as the individual points are rather long, but we use the same numbering as the reviewer for responding to the various items.

1. As pointed out in the paper, the attribution to the three vertical layers we have used is rather uncertain. Except for a few periods, where we think there is good evidence for emissions in the third layer, the inversion does not separate the three layers very well. But this is not really a focus of our study and we have pointed out associated uncertainties. A regional model may be able to retrieve more information on the vertical emission distribution but we think that this will be a very difficult task even with a

regional model.

Regarding the time resolution, we do not retrieve hourly emission values but only 3-hourly values. We have pointed out in the paper that the individual 3-hourly emission values are more uncertain than the total emissions. Timing errors of emission peaks of some two 3-hour intervals are possible. This has been discussed in detail in Stohl et al. (2011a). We have added some of these discussions in the revised version of the paper and also point the reader to the interactive discussion on these issues.

The statements (1.1-1.4) listed by the reviewer are not speculations but either descriptions of our results or interpretations of the results. We agree that more work will be needed to confirm or reject these statements, but such work (e.g., nuclear reactor analysis) is completely outside the scope of our study. This is rather something left for future assessments that will collect expertise in many different fields of research. These assessments will also benefit from large data-collection efforts which cannot be performed by this author team.

2. Our inverse modeling is based on dispersion model calculations driven with global data. It is not straightforward how regional-scale Japanese (e.g., precipitation) data could be used other than for comparison.

(1) It is true that the comparisons do show differences between precipitation fields, but we assume that also a regional model would not be able to reproduce the observed precipitation perfectly. This is a general modeling problem and we cannot see how we can solve it. Also, we have compared our precipitation maps with the AMeDAS plots as suggested by the reviewer and the precipitation patterns and amounts are actually in quite good agreement.

(Page 28351, lines 4-25): There is also no precipitation in the GFS data in a wide area around FD-NPP on 14 March at 12-15 UTC, certainly not over land. The discussion on page 28351 (lines 4-25) is about precipitation offshore in the Pacific Ocean, near the edge of the radar range. Therefore, in the model we also see very little deposition

over Japan on 14 March (furthermore, the plume was transported out over the Pacific Ocean during most of the day). We have improved the description such that it should be much clearer now.

(Page 28352, lines 4-9): The global precipitation data set from GFS cannot be expected to capture the detailed precipitation distribution over Japan on small scales. Notice, however, that the simulated precipitation is shown for all of 15 March and that the total simulated precipitation in Tokyo on that day is less than 1 mm. Most of the simulated precipitation fell north of the Kanto plain. In fact, the AMeDAS plots do show traces of precipitation extending into Tokyo Bay around 6:30 JST on 16 March (that is, still on 15 March in UTC), so that the simulated precipitation amount of less than 1 mm in the Tokyo area is probably even correct on the scale of the model.

(Figs. 16 and 19): We are not sure what the reviewer means with “The authors should be careful in demonstrating the regional maps based on the coarse-resolution model.“ A regional-scale model would certainly show more details, but our model does capture the main episodes during which radioactivity was transported across Japan, so it makes sense to show these maps. Others will produce regional-scale maps showing more detail. However, as long as the source term is not better determined (for instance, hourly and with better vertical resolution and smaller uncertainties), even these regional maps will probably not be much more valuable.

(2) We have compared our results with the results of the airborne sampling campaign (see top of page 28356). However, because of the quite different resolution of these data sets and the large range of values covering orders of magnitude, not too much can be learned from a visual comparison other than that the general orientation of the plume is in agreement. Access to the actual data files (which we do not have) and re-gridding of the airborne data to the resolution of the model would be needed in order to allow a more meaningful comparison.

(3) It is not true that our model generally overestimates the Cs-137 surface concentra-

tions and deposition values in Japan and underestimates Cs-137 concentration elsewhere. Figure 11 in the paper shows that the model does not generally overestimate deposition values in Japan. Figure 9 in the paper shows that the concentrations in Japan (these are basically all the high concentration values) are also not systematically overestimated. Notice also that our estimate for the fraction of Cs-137 deposited on Japanese land (19

The discussion the reviewer refers to is about one specific episode. We admit that it looks like the model overestimates the Japanese concentration and deposition values on 15 March. But even for that episode the concentration overestimate is seen only at three stations, whereas there is an underestimate at a fourth station. The three stations where the model overestimates are all relatively close to each other (at the scale of our model), so if the model overestimates at one station, it is likely that it also overestimates at the other two. Still, we admit in the revised version of the paper that an overestimate is possible for this important episode.

3. The description of the algorithm is indeed very brief but this had a good reason. The algorithm has been described fully in previous papers in the peer-reviewed literature, partly also open-access, and we did not see a need to provide the same description again. However, we have added some more information in the revised version of the paper, including the equation of the cost function. The observation error is a combination of the measurement error (taken directly from the measurement data files where available) and the model error. The model error was taken as the standard deviation of the model ensemble we have run. This should also be better explained in the revised version of the paper.

4. Regarding the early release of Xe-133, we think that we have discussed this extensively in Stohl et al. (2011a) and we have added some of these discussions in the revised version of the paper. We are not sure what the reviewer means with the statement that we should demonstrate that the earlier emission start can lead to the improvement of consistency with the observed Xe-133 concentrations. This early start

is the result of the inversion, not the assumed a priori information. Thus, it comes necessarily from an improved agreement with observation data.

5. Regarding the possible releases from spent-fuel pool of unit 4, we do not "insist" that the late emissions come from the spent-fuel pool. The sentence cited partly by the reviewer started with "We believe that. . .", which suggests that this is not a direct finding of our study but an interpretation, and we write "this result WOULD confirm that the spraying was an effective countermeasure", again indicating that this is not a direct finding of our study. We have tried to phrase this even more carefully in the revised version of the paper. Recently, independent evidence based on observed nuclide ratios has been presented showing that the caesium release must have included a significant contribution from moderately aged fuel, i.e. from spent-fuel pool 4 (Kirchner et al., 2012).

(1) We do not agree that the environmental dose rates in the data file provided by the reviewer show that our emission drop from 19-20 March is unreal. In fact, it seems to provide further evidence for a strong drop in emissions. The reported dose rates decreased from values close to 4000 μSv/h around 3 UTC on 19 March to below 2500 μSv/h on 20 March around 15 UTC. During this period, the wind speed decreased from some 5 m/s to 1 m/s. If the environmental dose rates were due entirely to radionuclides residing in the atmosphere and the wind direction and dispersion conditions other than the wind speed reduction remained the same, this would suggest a drop of emission rates by about a factor 8 during that period (concentration in the plume inversely proportional to the wind speed). However, the real drop must have been much stronger since ground shine has certainly strongly contributed to the measured environmental dose rates. A strong drop of environmental γ-radiation dose rates measured close to the ground is not possible once the soils are heavily contaminated, not even if the emissions from FD-NPP have ceased completely. For this reason, but also because of the unknown and variable position of radioactive plumes relative to the monitoring sites these measurements are not suitable to deduce quantitatively the release rates from

the damaged plant. If anything, they are supportive of a strong emission drop.

(2) The measurements in Tokyo Metropolitan area highlighted by the reviewer are interesting. We do not know where exactly these measurements were taken and a direct comparison is not possible, since no radionuclide concentrations are reported. However, we believe that our source term is very well compatible with these data. A movie of our dispersion simulation can be found here:

http://zardoz.nilu.no/~sabine/FUKU/Cs_FUKU_analysis.gif

As can be seen, the "Tokyo episode" is very well reproduced by the model, despite the strong decrease in the Cs-137 source term from 19-20 March. In the paper (Fig. 12), we have also shown a comparison between simulated and observed Cs-137 deposition in Tokyo. Notice that the observed deposition peak on 21 March is reproduced to within a few per cent by the model! We believe this is an amazing accuracy and this also demonstrates that the model has quite some skill at simulating regional-scale dispersion. Your suggestion that with our source term there would not be a "hot spot" episode in Tokyo is not true. Our source term is entirely compatible with this episode – and we have described this in great detail on page 28353 of our paper.

"Individual comments"

1) We write caesium instead of Cs-137 here, since this statement is also true for other caesium isotopes.

2) We do not have information on contamination problems at other Japanese sites. That's why we write that this ***might*** be the case. However, a recent paper concludes that there must have been important resuspension and re-distribution of radionuclides from highly-contaminated to less-contaminated areas (Yamauchi, 2012). We have added this reference to the revised version of the paper.

3) No, as we write in the paper, these values were chosen only where no information was available. It is an informed guess based also on uncertainties from stations where

we had information, so the numbers should be reasonable, at least.

4) We checked the unit. It is correct.

5) Xe-133 is a noble gas. It is not deposited at all.

6) We have performed more sensitivity tests in Stohl et al. (2011b), following up on the comment by M. Chino.

7) Because of the large range of measured and modeled concentrations, simply comparing averages is not meaningful. These would be dominated by a very few (maybe even a single) very high values (e.g., the peaks at Tokai-mura for Cs-137). The model may over- or underestimate these few values, but this would not be a reliable indication that the emissions are over- or underestimated. However, regarding the Xe-133 emissions, we have performed a separate study (to be submitted for publication soon), where we estimate the total emissions based only on measurement data as well as by comparing the model with measurements done in April, May and June. This independent study confirms our total Xe-133 emissions.

8) Thanks, the plume indeed didn't reach Osaka, which is too far south. We will change the description to read "Shizuoka prefecture".

References:

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