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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Modelling the global atmospheric transport and deposition of radionuclides from the Fukushima Dai-ichi nuclear accident" *by* T. Christoudias and J. Lelieveld

Anonymous Referee #3

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The authors modeled the global atmospheric dispersion and deposition of the Fukushima emissions by using the EMAC model. They compared the simulations at two spatial resolutions: one simulation was carried out with a spectral truncation of T255 and the other one with T106. Their source term was composed by 3 isotopes: I-131, Cs-137 and the noble gas Xe-133. They used the Xe-133 release rate assessed by Stohl et al 2012; the I-131 release rate estimated by Chino et al 2011 and they considered the Cs-137 release rate proposed by Stohl et al as well as the one from Chino et al. Simulated air concentration near the ground are compared to CTBTO measurements and results are discussed. Finally, they modeled the consequences of the Fukushima accident due to the deposition and to the inhalation of radioactive

materials. The paper is very close from other work like the Ten Hoeve and Jacobson (2012 Energy and Environmental Science) paper. The work need to be enhanced and expanded before publication.

1- p24533 The authors wrote “both radionuclides are released as gases, caesium has a low volatility and partitions into ambient aerosol particles, whereas iodine largely remains in the gas phase.” They wrote it again p24535 . . .

This statement amazes me; do the authors have any reference to support this assertion? My understanding was that a large part of the Iodine and Caesium were rejected as CsI aerosol. Moreover, non negligible amount of iodine as aerosol was observed. It means that wet deposition of iodine occurred and has to be considered to assess the consequences of the accident and the doses.

2- About the Stohl et al and Chino et al source terms

- The authors describe in detail the method used by Chino et al to estimate the source term. They discuss the uncertainties and show that their source term is highly uncertain. They should do the same and highlight as well the high uncertainties of the Stohl et al source term. One of their main concerns about the Chino et al source term is that the assessment is “limited by the use of Japanese station data only and a regional simulation domain”. I do not understand their argument since the use of a large amount of Japanese data is the best way to retrieve the release events dispersed above the Japan land. On the other hand, one of the difficulties encountered by Stohl et al was the small number of Japanese station data available to be used with their method. Most of the data were far away from the Fukushima power plant and they could not efficiently help to improve the source term (observed concentration were too low to have any impact). Therefore the Stohl et al source term is also highly uncertain.

- The source term proposed by Chino et al was assessed just after the Fukushima accident. They improved it in Terada et al 2012 (Journal of Environmental Radioactivity). The authors should use it instead of the Chino et al one.

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3- About the description of the simulations

The authors should give the references of the parameterisation they use for the vertical diffusion. What is the horizontal diffusion coefficient they use ?

4- About the model to data comparisons

- The simulations are compared to the CTBTO measurements. The authors should recall that the CTBTO measurements have been used to assess the Stohl et al source term. It certainly helps to obtain a good agreement for the model to data comparison. To validate their simulations, the authors should compare their results to measurements not used to assess the source terms. If not possible, they need to enlarge their comparison to other data and especially to Japanese data: air concentration and daily deposit measurements wherever data are available, airborne deposition. Unfortunately, the authors can not use dose rate measurements because of the lack of source terms for key isotopes like I-132, Te-132, Cs-136, Cs-134 . . . They should also discuss how their model fit with the observations compared to other model to data comparisons (Terada et al; Stohl et al; Hoeve and Jacobson, 2012 Energy and Environmental Science. . .).

- Do the authors think that a better resolution may improve their results in the vicinity of the source and even at the Japan scale?

- Figures 2-3-4 are too little and should be enlarged. A solution would be to shorter the time scale and to zoom in on the period where radioactive materials were measured. They should use a time scale with dates. I do not think that the logarithm scale used for the scatter plots is suitable.

- The discussion on the CTBTO data is very interesting but requires references.

- The authors should precise which source term has been used especially for the Cs-137 simulations. The simulations done with the Chino et al source term should also be plotted and results discussed.

5- About the deposition assessment

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- p 25541 the authors wrote “131I, which has a low solubility, so that convection effectively redistributes this gas into the free troposphere where the wind speed is typically higher and transport distances larger”. Do they think that it is coherent with the physical behaviour of iodine in the atmosphere? If so, they need to add a reference to support this statement. Do they think that this behaviour may explain the underestimation of the iodine concentration they show Fig.4? If so, they need to discuss such hypothesis. I wonder when and when convection events occurred?

- The authors can not neglect iodine wet deposition.

- The authors should discuss the validity of their meteorological fields and in particular the validity of the rain fields.

- The authors should compare their deposition assessment to other studies, for instance to the one from Morino et al. . .

6- About the doses assessment

- The doses are assessed by considering 2 isotopes only. The impact of other isotopes should be taken into account to assess realistic doses (i.e. Te, other iodine isotopes, other caesium isotopes. . .). I understand that the authors have no source terms for those isotopes but they should at least discuss their potential impact.

- The authors assess the doses “around the Fukushima” power plant. What is the distance from the plant they consider? Do they think that the spatial resolution of their simulation is sufficient to allow an analysis of the consequences “around the Fukushima” power plant? The highly contaminated zone in the North West of the plant covers an area of about sixty kilometres long which is less than 2 meshes in their simulations. In the model simulations, emissions are diluted in the release mesh so the concentrations computed in this release mesh are usually not used. Several meshes around the release mesh should also be removed from the analysis. What do the authors do to assess the doses within the 150-200 km around the plant? If they use the simulated

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concentrations they should at least discuss this point and show by using a model to data comparison that the computed concentration are realistic enough to be used in the doses assessment.

- I wonder what would be the impact of the given order of magnitude of the doses assessment on the Japan population. The authors should at least highlight the uncertainties of their study. When reading the paper I understand that the assessment minimizes the doses. In what proportion is that so?

- The authors should compare their results with similar studies already published like the one from Ten Hoeve and Jacobson (2012 Energy and Environmental Science).

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 24531, 2012.