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Discussions

Interactive comment on "Modeling and sensitivity analysis of transport and deposition of radionuclides from the Fukushima Daiichi accident" *by* X. Hu et al.

Anonymous Referee #2

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General Comments:

This paper uses the WRF/Chem model to simulate 131I and 137Cs concentrations and deposition over Japan during March 2011, following the Fukushima Daiichi nuclear power plant accident. Measured wind speed and direction, as well as precipitation and measured radionuclide deposition are compared to simulation results. Following this evaluation of the model's performance, a set of sensitivity simulations are presented, which examine the sensitivity of the simulated deposition to various parameterizations for wet and dry deposition, microphysics, turbulent diffusion, emissions and assumptions related to the gaseous fraction of 131I and the assumed size distribution

for aerosol-bound 137Cs. A certain degree of sensitivity is attributed to all parameters, with the greatest sensitivity attributed to emissions and the least to the dry deposition parameterization.

My major concerns with the current version of the paper are related to the methodology and presentation, as outlined below. The introductions states that an aim of the paper is to identify the combination of parameterizations that yield the lowest error in simulated ground deposition. However not all combinations of the available parameterizations are examined under the current methodology. Additionally, the comparisons with the observations are not clearly and concisely presented (for example, Figures 8, 9, 11 and 12 clearly show the model output for one day, March 21 but deposition values on the other days are not discernably different from zero for most of the panels). There are numerous tables that give results with 4-5 significant figures, which make the tables difficult to follow (particularly, Tables 2-10). Would 2-3 significant figures be adequate? The paper also does not give a well-developed discussion of the present results in the context of previous similar work. In general, the subject of the paper is appropriate for the journal, but the methodology for the analysis and the presentation of the results could be better developed as outlined in more detail below. The following specific points should be satisfactorily addressed prior to publication.

Specific Comments:

1) Abstract: The abstract does not clearly answer each of the research questions presented in Section 1. For example, there is no indication in the abstract about the relative importance of wet versus dry deposition and their respective sensitivity to the parameterizations as outlined in question 2 (only total deposition is mentioned in the abstract). I would also prefer to see quantitative answers as opposed to phases such as 'sensitive' and 'very sensitive'.

2) Abstract: The total deposition has the lowest sensitivity to dry deposition parameterization. Is this attributable to the finding that the dry deposition is a minor fraction of

the total deposition? If so, this should be indicated as opposed to the suggestion that the dry deposition parameterizations are associated with any greater confidence.

3) Abstract: It is not clear what you mean by "The ground deposition of radionuclides can also potentially be captured. . ."

4) P2116, L3: States that ". . . most of the previous studies. . . meteorological conditions are simply taken from. . . models and analysis/reanalysis products". Use of the word 'most' in this statement leaves me wondering if there were studies that did use an approach similar to this study and how these studies would compare. Again P2116, L14 states "most studies. . . the gaseous partitioning of 131I was not considered". This leaves me wondering if there were studies that considered this partitioning in greater detail and how this compared to the present study.

5) P2117, L3 gives the hypothesis that an optimal combination of parameterizations could be identified, which could reduce uncertainties in transport and deposition of radionuclides. It was not clear in the following discussion if this optimal combination was found, particularly since not all of the available combinations were examined (for example, DIF2 as in Table 1, was only ever combined with one of the microphysics parameterizations). The discussion was also not clear about whether the end result of this study was the hypothesized reduction in uncertainty.

6) P2117, L14 also states that an aim of the study is to obtain the combination of parameterizations that gives the lowest error, but this is not represented in the abstract. I think that each of the four questions in P2117, L18-26 should be given at least one sentence in the abstract.

7) P2119, L4 notes that the change in the partitioning of 131I between the gas and particle phase may change with time, but that this is not considered in this study. This is an important point. The gas to particle conversion for 131I typically occurs on time-scales from 2-3 weeks (Masson et al., 2011). This change in the partitioning over time could have a strong influence on the removal rates. The study tries several fixed

partitioning values but neglect of this temporal change in the partitioning could limit how close the agreement can be with the measurements – it might be helpful to note this deficiency in the discussion related to the sensitivity studies in Section 3.3.

8) Section 2.3.2 discusses the dry deposition velocities. How closely do the velocities calculated by the resistance method and simple parameterization agree with the values used in the constant deposition method?

9) P2125, L10-12: How uncertain are the parameters used to represent the increased decay rates due to soil activity and how does this affect your comparisons with measurements?

10) P2126, L8: The emissions are only released at the lowest level. How might this influence your results and conclusions? Indeed there are many confounding factors and this does prevent the authors from making a strong conclusion and creates issues with the possibility of error cancellations yielding a better result but for the wrong reasons. This was acknowledged in the conclusion but could be addressed earlier in the discussion and abstract if an optimal combination of parameterizations were to be presented.

11) P2126, L22: It is not clear what you mean by 'simple aerosol treatment'. Does the model have a bulk aerosol mass scheme as opposed to size-resolved aerosol simulation?

12) P2127, L16: It is not clear what type of size distribution is assumed here. I would guess log-normal.

13) Section 2.4.2 presents the metrics used for evaluating error, comparing the model and observations. Can you explain why you chose these metrics over others such as mean fractional bias (that allows for error also in the observations) as outlined in Boylan and Russell (2006)?

14) Section 3.1: I was not clear why in searching for an optimal combination, not all

combinations of the various parameterizations were examined. For example, would DIF2 combined with MP2 or MP3 give a better result? And likewise for all the various combinations of wet and dry deposition parameterizations.

15) P2133, L10-15: This discussion seems rather hand-waving. Are you able to provide model results that show that there are differences in the turbulence, upstream winds or precipitation in response to these small differences in the wind fields.

16) P2134, L9-10: Case REF is noted to have the lowest global rank and it is suggested that the microphysics scheme WSM6 is superior to the other two schemes. However this is only tested for one combination of wet and dry deposition parameterizations. Could the result be different with different combinations of deposition parameterizations and microphysics schemes?

17) Section 3.2.2: How was the one additional wet deposition parameterization chosen and why was this one chosen?

18) Would a log scale on the y axis of several of the figures (such as Figures 8, 9, 11, and 12) help with visualization of the deposition for days other than March 21.

19) There are a limited number of stations available for measurement data. Did you include all available deposition data for Japan such as in Hirose (2012)?

20) P2139, L20 suggested that the optimal fraction of gaseous 131I is 30-60%. However this is should be clarified as being only for this model setup. Additionally, this fraction is in agreement with the work of Momoshima et al. (2012), which could be added as a reference here.

21) P2139, L20. In regard to the log-normal size distribution for 137Cs, it is not clear in the text how the size distributions interact with the deposition parameterizations or other parameterizations to yield changes in the deposition. Please expand on how this is represented in the model.

22) In general, could the results section include more discussion to put this work and

these findings in context of previous and similar work as also noted by the first referee.

23) P2141, L25-26. The text uses the words 'subtle' and significant' where more quantitative discussion would be helpful.

24) Tables in general: a reduction in the number of significant figures is suggested,

25) Figure 6: difference contours for the lower 4 panels might help to display the comparison better.

26) Figure 7: Caption should state the simulation presented here and likewise for Fig. 10.

Technical Corrections:

1) P2135, L2: remove word 'see' before Fig. 1

2) P2141, L10: change 'are validated' to 'are evaluated'

3) P2141, L15: Change acronym from 'AADE' to 'AAD' to be consistent with previous text

4) P2141, L22, remove brackets around Talbot et al., 2012

References:

Boylan, J.W. and A. G. Russell: Atmos. Environ., 40, 4946-4959, 2006.

Hirose, K.: Journal of Environmental Radioactivity, 111, 13-17, 2012.

Masson, O., et al. Environ. Sci. Tech., 45, 7670-7677, 2011.

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