Dear Editors:

Thank you very much for your careful reviews. As requested, we have made all necessary changes in our manuscript (acp-2013-910) to address the reviewer's concern and have detailed how the points raised by the referees have been accommodated. From the changes made in the revised manuscript and responses provided below, I hope you are convinced that we have adequately addressed the reviewer's concern and made the paper stronger. If there are any further questions, please feel free to let us know.

Thank you very much.

Sincerely,

Xiaofeng Hu on behalf of all co-authors

2014/08/18

PS. Authors: X. Hu, D. Li, H. Huang, S. Shen, and E. Bou-Zeid

Title: Modeling and sensitivity analysis of transport and deposition of radionuclides from the Fukushima Daiichi accident

Number: acp-2013-910

Answer of the reviews' questions:

The comments from Reviewer 1:

1. (Chapter 3.2.1.) The authors answered that "there is no size distribution information during the period from 3/11 to 4/28 to use as input data for the simulation in this study." Thus, the authors assumed that the average size of ^{131}I and ^{137}Cs is 0.48 μm and 0.67 μm (sampling location: Tsukuba, Japan) and then answered that "the size distribution will not affect the total deposition significantly." In addition, the average size of ^{131}I was quoted from the Chernobyl case. However, some data obtained from 4/11 to 4/21 are available in the reference listed below. The average size of ^{131}I and ^{137}Cs was 0.7 μm and 1.0-1.5 μm (sampling location: Tsukuba, Japan), which may increase the dry deposition rate. Is it possible for the authors to check if the findings of this study are still valid by using the values of Masson et al. (2013)? If not, please state that one could also use particle size values from the Masson et al. (2013) to perform similar simulations.

We thank the reviewer for pointing us that some data obtained from 4/11 to 4/21 are available from the Masson et al. (2013), which is now mentioned in the revised manuscript. However, it is very difficult for us to rerun the simulations with the values of Masson et al. (2013). So based on the reviewer's suggestion, we add a sentence in the revised manuscript to state that one could also use particle size values from the Masson et al. (2013) to perform similar simulations. The sentence is "In addition, one could also use particle size values of ^{131}I and ^{137}Cs as 0.7 μm and 1.0-1.5 μm, respectively (Masson et al., 2013) to perform similar simulations."

2. (Chapter 3.2.1.) I suggest to the authors to state all the assumptions they made to choose parameter values as input data for the simulation.

Based on the reviewer's suggestion, we explicitly state the assumptions that we made about how we choose parameter (including the particle density, the dry deposition velocity, the average size of ^{131}I and ^{137}Cs etc.) values as input data. As for the particle density, we add "one could also use another typical value 4.93 g/cm^3 proposed by Baklanov and Sorensen (2001)"; for the dry deposition velocity, we add "One could also use other typical values as the dry deposition velocity for both ^{131}I and ^{137}Cs (e.g. as reported by Sportisse (2007), the dry deposition velocity of gas-phase ^{131}I could range from 0.1 to 0.5 cm/s, while the dry deposition velocity of ^{137}Cs could range from 0.04 to 0.31 cm/s)"; for the average size of ^{131}I and ^{137}Cs, we add "one could also use particle size values of ^{131}I and ^{137}Cs as 0.7 µm and 1.0-1.5 µm, respectively (Masson et al., 2013) to perform similar simulations."

3. (Line 336) The authors mentioned that they corrected the order of the references from Kaneyasu et al., 2012; Sportisse, 2007 to Sportisse, 2007; Kaneyasu et al., 2012, but I cannot see the changes.

   We are sorry about the mistake. The order of the references is changed this time.

4. (Line 598-601) Please check the sentence: "However, the TOCHIGI, the comparison of the different runs does indicate that the parameterizations of the two methods of deposition have comparable influence on the results when their relative contributions to total deposition are comparable."

   The sentence becomes "However, at TOCHIGI, the comparison of the different simulations of ^{131}I (Fig. 11, upper right panel) indicates that the parameterizations of two depositions (i.e., wet deposition and dry deposition) have comparable influences on the results. This is because their relative contributions to the total deposition are comparable, as can be seen from Fig. 9."

The comments from Reviewer 2:

1. Abstract, L24: The wording seems awkward. Would 'the influence of the characteristics of the emission source …….. on the simulated transport and deposition' be a better wording?

   Based on the reviewer's comment, the sentence is changed to "Subsequently, the influence of emission characteristics (including the emission rate, the gas partitioning of ^{131}I and the size distribution of ^{137}Cs) on the simulated transport and deposition is examined."

2. Abstract, L37 and L40: Could the word 'variations' be replaced by AAD?

   The word 'variations' is replaced by AAD.

3. P4L88: By total deposition – do you mean globally or within a certain domain? Does the Korsakossok et al. (2013) study pertain to a certain domain for the ^{137}Cs total deposition values cited? Also does the present study give the total deposition for the domain and time period considered for this work?

   The 'total deposition' means a sum of dry deposition and wet deposition within a certain domain. The study of Korsakossok et al. (2013) pertains to a certain domain for the total deposition of ^{137}Cs within 80 km of FDNPP over land. We add some statements for this to the paper to make things clear (…the total deposition (sum of dry and wet deposition within a certain domain; in this paper, it is used interchangeably with ground deposition) from the Fukushima accident of ^{137}Cs (over land and within 80 km of FDNPP)…). The study of Morino et al. (2011) gives both dry and wet depositions for the same time period and over a similar domain, but the model used by Morino et al. (2011) as well as the goals of their studies are quite different from this work. In this work, we also mentioned some of the results from the Morino et al. (2011) study (see Section 3.2.1) as a comparison to our study.

4. P12L284: What are the dimensions of the deposition rate in Eq. 17. Is this dimensionless? If so, this is a rate or a removal fraction? Also is this applied for scavenging both in and below clouds?

   The dimension of the deposition rate in Eq. 17 is s^{-1}, and the dimension of the rain intensity is mm h^{-1}, but a and b are not dimensionless. The deposition rate is applied for both below-cloud and in-cloud scavenging.

5. P13L301: Please specify if this is applied in clouds or below clouds or does this include both?

   The wet ground deposition is applied for both below-cloud and in-cloud scavenging.

6. P13L313: Could there be a comment about the potential influence of the assumption that the emissions are only released into the lowest level? For example Giorgi and Chameides (1986) and Croft et al. (2014) show that the altitude of injection can strongly control lifetimes with respect to removal. Also Stohl et al. (2012) suggest that injection heights may have extended up to 1 km.

We agree with the reviewer's point. As mentioned by Korsakissok et al. (2013) and Stohl et al. (2012), the release height ranges from 20m to 150m even extended up to 1 km. However, the accurate data on the vertical distribution of the emissions is still absent, which remains a source of uncertainty when simulating the transport and deposition from the Fukushima accident. But due to the lack of data, we placed all the emissions at the lowest level so that this uncertainty cannot be addressed in this study. Based on the reviewer's suggestion, we add a sentence about the potential influence of this assumption to the paper, as follows: "The emissions are only released at the lowest level which is about 25 m. The release height reported in the literature primarily ranges from 20m to 150m but sometimes extended up to 1 km (Korsakissok et al., 2013; Stohl et al., 2012). However, accurate data on the vertical distribution of the emissions is still absent, which remains a source of uncertainty when simulating the transport and deposition from the Fukushima accident."

7. Section 2.4.2: Throughout this section, could the case numbers be replaced with the simulation names? For example, instead of 'Cases 6,7,8 and 9', the use of the simulation names from Table 1 would make this discussion easier to follow.

Based on the reviewer's suggestion, the case numbers are replaced with the simulation names defined in Table 1.

8. P18L436: The circles on the panels are helpful. Should the first panel also have a circle?

We add a circle to the first panel of Fig.6.

9. P19L447: This maybe more of a style issue, but I tend to avoid directives to the readers. Consider replacing ‘see Fig. 5’ with ‘as shown in Fig. 5’.

   We agree with the reviewer’s point and ‘see Fig. 5’ is replaced by ‘as shown in Fig. 5’.

10. P20L478: Could there be a mention of why the two example stations are chosen?

Based on the reviewer’s suggestion, we add this sentence to the manuscript: “Both of the two stations have typical terrains of Japan, but they are different: The YAMAGATA station is located in the north of Japan (to the northwest of FDNPP), surrounded by mountains, while the CHIBA station is located in the Kanto Plain (to the south of FDNPP).”

We add a sentence: “Both of the two stations have typical terrains of Japan, but they are different: The YAMAGATA station is located in the north of Japan (on the northwest of FDNPP), where surrounded by mountains, while the CHIBA station is located in the Kanto Plain (on the south of FDNPP).”

11. P20L496: In regard to the influence of subtle differences in the wind fields at YAMAGATA generating large differences in precipitation, how also might the inhomogeneous terrain in this grid box contribute to the noted differences in precipitation and deposition? Could a comment be added to address this?

We add a sentence: “In addition, the terrain may also influence the deposition indirectly. For example, the YAMAGATA station is located in an area surrounded by mountains, so the inhomogeneous terrain might not be well described by the coarse resolution (3 km, domain 2) simulations (that is, the WRF model is unable to resolve the subgrid-scale topography), which may be an important reason that large biases are witnessed in the simulated precipitation. Considering the complex terrain, subtle differences in the wind fields in this grid box may have larger influence on the precipitation than that in the plain, which then generate large differences in precipitation and deposition.”

12. Table 4 onwards: Could the table caption mention where the definitions of SR and GR are

given?

We add the sentence “The definition of GR and SR is shown in Section 3.1.2.” in the caption of Table 4 -10.

13. P22L546-551: There is a mention of under- and over-estimations here. Could there be a comment about the possible reasons for these differences at the various stations?

There are multiple possible reasons leading to the discrepancies between the observed and simulated data as stated in the Introduction Section of this paper. The under- and over-estimations are partly due to the uncertainties in emissions and partly due to the meteorological conditions including wind fields and precipitation. Moreover, these factors are not independently. For example, as stated in the previous reply, small differences in wind fields may generate large differences in precipitation and thus influence wet deposition.

We add a sentence to the paper to briefly discuss the reasons: “The reasons of differences between the observed and simulated data are multiple and sometimes dependent on each other. The discrepancies are partly due to the uncertainties in emissions and partly due to the uncertainties in the simulated meteorological conditions including wind fields and precipitation.”

14. P23L571-573: Are the authors able to give an indication about why the different radionuclides are affected differently by wind and rainfall. Does this have to do with the gas to particle partitioning of ^{131}I.

From Fig. B1, it is clear that the pattern of concentration of ^{131}I is quite similar to that of ^{137}Cs, but the values are different. The main reasons are: firstly, the emission rate and the total amount of these two kinds of radionuclides are different; secondly, different nuclides have different dry and wet deposition velocities, so the depositions during the process of tranport are different, despite of the same wind fields and rainfall fields. Similarly, different gas/particle partitioning of ^{131}I are also affected differently by wind and rainfall, because the deposition parameterizations of gaseous ^{131}I and particulate ^{131}I are also different. These reasons are now stated in the revised manuscript.

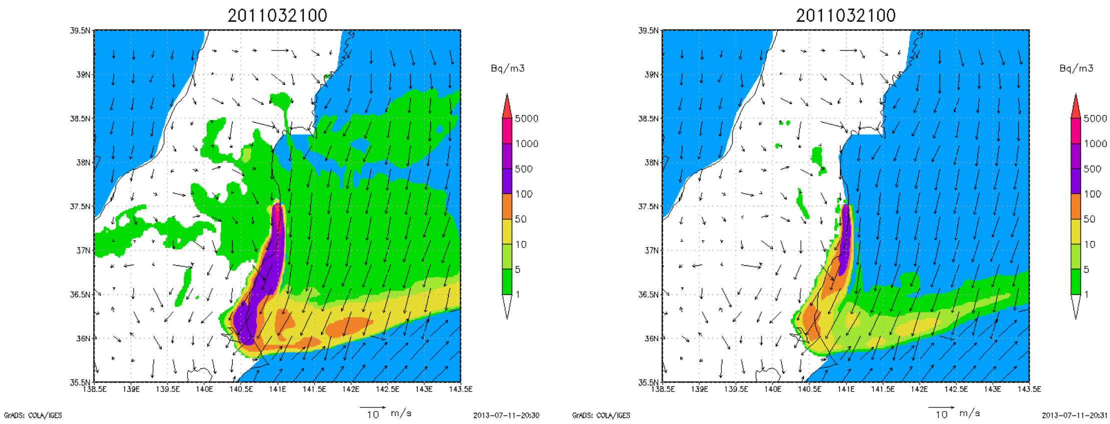

Fig. B1. The near-surface concentration of ^{131}I and ^{137}Cs at 00 UTC on March 21. The left panel shows the distribution of concentration of ^{131}I and the right panel shows the distribution of concentration of ^{137}Cs. The results are from the simulation REF.

15. P24L599: Would the terminology 'simulations' be better than 'runs'?

    We agree with the viewpoint of the reviewer. The word 'runs' is replaced by 'simulations'.

16. P25L609-614: Dry deposition seems to depend on the gas to particle partitioning. Would the conclusions about the performance of the dry deposition parameterizations for ^{131}I be different if a different fraction for the partitioning was used? Likewise would this influence the conclusions about the wet deposition scheme that performs best for ^{131}I as in P25L615-620? Perhaps this could be mentioned here.

We agree with the reviewer. In Section 2.4.1, we stated that the partitioning of ^{131}I at the source is chosen to be 80% gas as recommended in several studies (Korsakissok et al., 2013; Morino et al., 2011), so the conclusion here may depend on this parameter, which may also influence the conclusions about the wet deposition scheme that performs best for ^{131}I. In this paper, we aimed to investigate the sensitivity of WRF simulated results to a series of physical parameterizations so we didn't conduct simulations with all possible combinations of physical parameterizations. A sentence is added to the paper to mention this: "Note the gas partitioning of ^{131}I at the source is chosen to be 80% as recommended by several studies and changing this value may alter our results."

17. P28L699: What is the definition of Error_ref and Error_sens in Eq. 23:

As stated in Section 3.4, $Error_{REF}$ is the error in the reference case and $Error_{SENS}$ is the error in the specific sensitivity case, where the error can be either Percentage Bias (PBIAS) or Percentage Root Mean Square Error (PRMSE) as defined in Section 2.4.2. So when we use AAD, we indicate whether AAD is defined in term of PBIAS or it is in term of PRMSE, as can be seen from Table 11.

18. Conclusions: For consistency and to help readers to read the conclusions independently of the entire text, consider re-stating here the terminology for the AAD ranges that apply to your categories of 'very sensitive' and 'moderately sensitive' and use these terms consistently in the conclusions as opposed to P31L764, which uses the terminology 'quite sensitive'.

We thank the reviewer for this suggestion. We state the AAD ranges again in the Conclusion Section and as for the terminology 'quite sensitive' in P31L764 is now replaced by 'very sensitive'.