Review of the Manuscript:

Hu X., Li D., Hong H., Shifei S., Bou-Zeid E. (2014) "Modeling and Sensitivity Analysis of Transport and Deposition of Radionuclides from the Fukushima Daiichi Accident". *Atmospheric Chemistry and Physics Discussions*, 14 (2), 2113-2173, 2014

This manuscript is focused on simulating the atmospheric transport and deposition of radionuclides emitted from the Fukushima nuclear plant accident using Weather Research and Forecasting/Chemistry (WRF/Chem) model and comparing simulation results with observations. Half of the manuscript is focused on the validation of the WRF/Chem model, including the reference simulation, and the rest reports results of the sensitivity analysis of the deposition to various parameters, such as dry and wet deposition. General and specific comments about this manuscript are given as follows.

General comments:

1. The focus of the manuscript is relevant and appropriate for the Journal.

2. The volume of the work reported in the manuscript is larger than in normal cases.

3. An established model (WRF/Chem) is employed to simulate the Fukushima case. A similar model was used by other investigators, and the parameterization procedure was very similar. Detailed comments are given below in the specific comments.

4. As the authors stated in the conclusions, this manuscript has some limitations. This is because, although they studied the atmospheric transport and deposition of radionuclides, which were highly influenced by their size, the input data of the size distribution used in this study may not be appropriate. Details are given below.

Specific comments:

1. (Abstract) The authors stated that one of the objectives of this manuscript was "to assess the skill of Weather Research and Forecasting/Chemistry (WRF/Chem) model in simulating the atmospheric transport and ground deposition of radioactive isotopes" in contrast to Srinivas et al. (2012) who tried to validate the WRF model in terms of meteorological conditions for their study and performed a statistical analysis similar to the work of the authors. Were the results of the WRF model obtained by the authors quite different than those of Srinivas et al. (2012)?

2. (Abstract/Introduction) As stated, one of the distinguished features of this manuscript is the simulation the Fukushima case using the WRF/Chem model. However, Huh et al.(2012, 2013) already used the WRF/Chem model to verify the transport of radionuclides from Fukushima to Taiwan. It would be nice if the authors could add a discussion of the papers below to their manuscript.

Huh et al.(2012) Fukushima-derived fission nuclides monitored around Taiwan: Free tropospheric versus boundary layer transport, EARTH AND PLANETARY SCIENCE LETTERS, Vol 319–320, 9-14.

Huh et al.(2013) Regional Dispersal of Fukushima-Derived Fission Nuclides by East-Asian Monsoon: A Synthesis and Review, AEROSOL AND AIR QUALITY RESEARCH, Vol 13, 537–544.

3. (Chapter 2.3.2.c) For the constant deposition velocity method, the authors assumed $v_{particle,I-131}$=0.1 cm/s, corresponding to the dry deposition velocity of SO_4 suggested by Baklanov and Sorensen (2001). Using the mean size of SO_4 measured at the same sites by Kaneyasu et al. (2012), can the authors calculate $v_{particle,I-131}$ and compare it to the value of 0.1 cm/s?

4. (Chapter 2.4.1.) As shown in some equations (e.g., eq 13 in Chapter 2.3.2), size may be an important parameter gauging the contribution of the dry and wet deposition to the total deposition and transport. The average size used in this study was 0.48 μm (for I-131) and 0.67μm (for Cs-137), respectively. The average size of I-131 represents activity median aerodynamic diameter (AMAD) while that of Cs-137 is the retrieved second mode. Why did the authors choose the value of the retrieved second mode as the average size of Cs-137 although its AMAD (0.53μm) was available in the same reference?

5. (Chapter 3.2.1.) The authors discussed the simulation results of the dry, wet, and total depositions using observational data obtained from 3/18 to 3/30 (e.g., Figures 9, 11, and 12). Despite studying the Fukushima case, the authors used the size distribution obtained probably from the Chernobyl case (for I-131) and the data (for Cs-137) measured by Kaneyasu et al. (2012) after 6 weeks from the Fukushima accident (4/28 to 5/12). Kaneyasu et al. (2012) reported that the size distribution of Cs-137 obtained after 5/12 was different from that before 5/12. This implies that the data, quoted from Kaneyasu et al. (2012) may not represent the initial size distribution of the Fukusima case. In addition, as the authors stated in the introduction, a heavy rainfall event also occurred between 3/15 and 3/17. Could the input data used in this manuscript represent well the Fukushima case?

6. (Chapter 3.2.2.) The authors simulated WET2 to analyze effects of relative humidity (RH) on the wet deposition. Without considering hygroscopic growth of radioactive particles, is it possible to perform and discuss the sensitivity analysis of the ground deposition to the parameterizations of dry and wet deposition?

Other specific comments:

7. (Line 233) The authors used 3.5 g/cm^3 as the density of I-131. Is this a typical value? For example, Baklanov and Sorensen (2001) proposed 4.93 g/cm^3. Does this affect the simulation results of the dry deposition?

8. (Lines 248-249) The authors wrote that "We use some typical values for of 131I and 137Cs that are found in the literature". Remove either "for" or "of".

9. (Line 250) The authors used some typical values for $v_{gas,I-131}$=0.5 cm/s and $v_{particle,Cs-137}$=0.05 cm/s. However, Sportisse (2007) also reported some values (e.g., $v_{gas,I-131}$ = 0.1 to 0.5 cm/s; $v_{particle,Cs-137}$ = 0.04 to 0.31 cm/s). It seems that the authors used the maximum value for $v_{gas,I-131}$ and a near minimum value for $v_{particle,Cs-137}$, which implies that the simulation results may be overestimated or underestimated. The question is how different the results would be for a different set of velocity values?

In addition, these values depend on the land use coverage, where radionuclides are deposited, or chemical forms (e.g., elemental iodine or organic iodine). However, it may be hard to find some assumptions about these points. It would be nice if the authors could clearly state their assumptions with justifications.

10. (Line 314) In this manuscript, the abbreviation, "WSM 6" was frequently used but its full definition was not given.

11. (Line 320) Because of the flow of this sentence, the reviewer suggests changing the order of the references: from (Kaneyasu et al., 2012; Sportisse, 2007) to (Sportisse, 2007; Kaneyasu et al., 2012).

12. (Lines 392-394) The authors stated that "the subtle differences in the wind fields generated by using two different horizontal diffusion schemes can result in significant differences in the ground deposition of radionuclides". According to the statement, understanding the horizontal diffusion schemes may be required, but some additional explanation is needed for general readers?

13. (Lines 574-577) The authors wrote that "However, the TOCHIGI comparison does show that the parameterizations of the two methods of deposition both have comparable influence on the results when their relative contributions are comparable". It would be better if the word "both" was removed.

14. (Lines 696-697) In "4. Conclusions", the authors noted the difficulty of simulating the wind field using Talbot et al. (2012) which was cited for the first time in this manuscript. Would it be better if the authors discussed this in "1. Introduction"?

15. (Lines 705-708) This is similar to the above comment. If this part is important, would it be better if this statement and the reference, Li et al. (2013) were moved to the introduction?