Interactive comment on “Detailed source term estimation of the atmospheric release for the Fukushima Daiichi Nuclear Power Station accident by coupling simulations of atmospheric dispersion model with improved deposition scheme and oceanic dispersion model” by G. Katata et al

Anonymous Referee

GENERAL COMMENTS

The authors present an ambitious article on the analysis of the Fukushima accident in the light of all the work that has been done on the subject. The paper addresses various topics: source term estimation and analysis of the release events; verification of the source term at local, regional and global scales. They partly raise the issue of the sensitivity to the atmospheric dispersion model, to the meteorological data and to the deposition parameterizations.

The analysis of the accident (impact of the release events, connection between the release events and the events that occurred in the plant, comparisons with observations) is highly comprehensive. The supplement of the paper is also very useful. The uncertainties associated with the source term estimations due to the assessment method and to the meteorological data need sometimes to be refined and reminded in the text (section 3.1 in particular). The part concerning the importance of improving the modeling of the deposit is less convincing and need to be further developed (without increase the paper size) or addressed more fully in another paper. Given the current state of the study, the authors have to be much less clear-cut on the advantage to use a more sophisticated deposition scheme.

The paper is very long. The authors can probably shortening some parts. Some figures may be moved to the supplement of the paper. Some suggestions are given.

MAJOR COMMENTS

Section 2

Section 2.2: Reverse estimation method over the land

The method used is described more precisely than in previous papers, but it remains unclear.

1. P 14735 What do the authors mean when they explain that they used peaks values from continuous time series of air concentration? Why is it better to do so than to use the full set of data?
2. P 14735 With dose rate measurements, only the observed air dose rate from ground shine is used to assess the source term. If so, how the timing of releases (beginning and end) is estimated?

3. The isotopic composition is assumed to assess the source term by using observed dose rate from ground shine. Release rate for noble gazes is not assessed and computed dose rate signal does not take into account the contribution of noble gazes to the plume dose rate. Nevertheless Chiba observations showed that for some release events a large part of the total dose rate was due to the noble gazes contribution when the plume was detected. How do the authors interpret the comparisons between total air dose rate (including plume contribution) simulated and observed shown Figures 16 and in the supplements? What is the impact of their assumption on the source term assessment and its evaluation?

<u>Section 2.3: inverse estimation method over the ocean</u>

1. Except if I missed something, the method used to assess the source term over the ocean is not based on inverse modelling techniques. To avoid any confusion, the authors should not call their method "inverse estimation method".
2. This part of the paper has to be improved: the goal and the method have to be clarified.

<u>Section3</u>

<u>Section 3.1 Source term estimation and local-scale dispersion analysis</u>

1. The reliability and uncertainties of the meteorological data should be given for the various release events.
2. What monitoring data are used to reconstruct each release events? Their number has to be given (it could be given Table 5). The relevance of the various emissions has to be discussed. This requirement is at least needed for the release events showing the main discrepancies between the actual study and the previous one.
3. What are the specific reasons for the new release assessment (especially on March 15-16)? This point is partially discussed Section 4.1 and need to be completed.

<u>Section 3.2</u>

Model to data comparisons must be completed:

1. The main release events that are different from the previous study could be analyzed in more details. Does it give a better agreement by comparing simulations with dose rate measurements and/or air concentrations measurements? Monitoring dose rate comparisons shown in the supplements may be used to explain the impact of the new source term compared to the previous one.
2. <u>Regional deposition</u> :
    a. Authors claim that "both improvements resulted from the enhancement of the scavenging coefficient by including in-cloud scavenging in the modified wet deposition scheme" the demonstration is not conclusive and it is difficult to precisely identify what is the specific contribution of the new deposition scheme. The authors should compare simulations done with the new source term and the previous deposition scheme with simulations done with the new configuration and source term. Those simulations could be compared in the various tables. Moreover, it seems surprising that the in cloud scavenging has a large impact. Indeed, the plume was

probably situated in the lower layers of the atmosphere at the regional scale and below cloud scavenging may have been dominant.

  b. Comparisons done table 6 are not homogeneous. It is not always the same simulations that are compared with the "New-land" one. It has to be more homogeneous.

3. Local air dose rate
   a. A table giving the statistical indicators for air dose rate comparisons should be added.

Section 4 Discussion

Section 4 does not work.

Section 4.1

1. Chapter 4.1 could be moved to the beginning of Section 3 in order to better highlight the specificity of the new source term estimation.
2. The authors claim that the release of March 15-16 is assessed because of the new data set: what data are useful to reconstruct this event?
3. I do not believe that the modified wet scavenging scheme could explain the new timing of the release event. It can help to decrease the release rate on March 15 pm but it cannot explain the increase of the release rate in the evening.

Section 4.2

1. As previously said, it is difficult to precisely identify what is the specific contribution of the new deposition scheme and I do not believe that the authors should end their paper with this section. This discussion could be dispatched partly in section 3.1 and partly in section 3.2.
2. What is the relative contribution of below cloud and in-cloud scavenging (especially at the local scale)?
3. The authors should be less conclusive on the beneficial contribution of the new deposition scheme considering the various uncertainties (meteorological data, iodine speciation…). Moreover Table 7 shows that the model to data comparison may be less sensitive to the MLDPO deposition scheme than to the meteorological conditions (NAME simulations) and to the source term.
4. The authors present the fog deposition scheme as an important improvement. What about the quality of the fog and drizzle simulations with MM5? What about the fog observations? Light rains are not detected with radar observations. Are they with rain gauge?
5. The relevance of the precipitation data should have been discussed before the end of the paper since it has a huge impact on the release assessment. For instance, what is the impact of the over-estimation of the rain data on March 20?

Appendix: below cloud scavenging

1. Nucleation scavenging rate is a process to be considered for in cloud scavenging and not below cloud scavenging. What is the point of the authors?

2. How the below cloud scavenging is parameterized since you do not consider aerosol-hydrometeor coagulations scavenging? What is the relative contribution of below cloud and in-cloud scavenging at the regional scale?
3. At the local scale, the plume may be situated below the cloud. Therefor below-cloud scavenging cannot be neglect compare to in cloud scavenging.

Paper organization

The paper organization can be improved and sometimes shortcut. Example

1. Section 2.4.1: the reverse estimation method is partially described in section 2.4.1 instead of section 2.2. The observations used in the study were partially described in section 2.2.
2. See suggestions for Section 4.

MINOR COMMENTS

Section 1: Introduction

Introduction has to be improved:

1. P 14730 the argument developed following « First, the estimation… » has to be clarified. Too many things are discussed.
2. When explaining the source of discrepancy they need to add the uncertainties in the meteorological data (wind, rain…).

Section 2.2: Reverse estimation method over the land

1. Assumptions regarding the ratios of I2, CH3I and particulate iodine have to be specified together with the isotopic composition of the release. The authors need to evoke the strong uncertainties due to the isotopic composition of the release and a fortiori of speciation of the iodine, the behavior of iodine into the atmosphere. This discussion can be done section 2.4.4 if more appropriate.

Section 2.4.4: Simulation settings

Section 2.4.4 needs clarification.

1. What meteorological data were used and when? The authors should precise the method they used to choose the more appropriate meteorological data for each release event.
2. Are the meteorological data different from the previous study? What are the differences?

Section 3.1

1. Section 3.1 is very interesting but it is sometimes difficult to discriminate between what is known for sure and what is due to the analysis of the results/model outputs. For example, P14745 “the light rain or drizzle”. Is it observed? The text has to be carefully re-read in order to avoid any ambiguity.

Section 3.2

Model to data comparisons must be completed:

1. Statistical indicators must always be the same in the various tables and in the text. For instance, p 14752 FA 10 is used; p 14754 FA 5 is used. You should use always FA5 for instance.
2. Validation using several models : this part can be shortened

Section 5

1. Modifications have to be done in accordance with the previous remarks (uncertainties, impact of the new cloud scheme…).

Tables

1. A table similar to table 3 could be added for dose rate observations used in the reverse method
2. Table 5: description of the last column is missing. Does it give what monitoring data are used to assess the source term?
3. Tables 6-7-8: please give the same list of statistical indicators (add FA2, FA5, FA10 in tables7-8 and NMSE, FB, FMS, KSP, Rank in table 6)

Figures

1. Generally Figures are too small.
2. Some figures are not essential and may be removed if the paper is too long for publication. For instance
    a. Figure 1 can be suppressed.
    b. I am not sure that Figures 5-6 and 23 are required.
    c. Figures 8 are too small to be useful.
3. There is a problem with the blue curve on Figure 11c.
4. Bands within a factor of 10 have to be added on Figures 18-20-21-24.

Appendix

1. The authors should give more information on the initiation of the various parameterizations and the rain threshold used.
2. The authors should talk about Iodine particulate instead of restricting it to particulate I-131.
3. L22 p14770 has to be modified.

A careful reading is required to avoid typos.