Dear anonymous referee #3,

We very much appreciate your constructive comments, useful information and your time for RC2. Especially, your suggestion on the changing definition of p_{20} from mass fraction to surface area fraction was really helpful. Thanks to your review, our manuscript was substantially improved. Point-by-point responses to your comments are written in blue in this letter.

Best regards,
Mizuo Kajino

General comments:

This paper estimated significance of the resuspension/deposition processes in the Cs-137 budget at the ground surface in a wide area in the northern part of Japan. Although the conclusion that the resuspension is insignificant in changing the contamination distribution is somewhat too obvious, the procedures and discussions that result in this conclusion are pertinent and informative. It is also interesting that the different sources were found to account for the air concentration variations in the different seasons. Since technical comments were already made in the previous reviewing process, the reviewer raises some points for discussion here.

Thank you for your evaluation.

Specific comments:

1. The resuspension scheme in this paper (p.8, line 8-24) assumes that the Cs-137 flux is in proportion with that of the soil mass. This obviously is too crude an assumption to make in contrast to other sophisticated formulations for resuspension since the activity concentration is usually much higher in a fine particle fraction due to its larger specific surface area. This assumption may result in considerable underestimation of Cs-173 resuspension, and is highly probably one of main causes necessitating the unphysical parameter of 10 (p.8, line 31). Discussion on this point must be included in the text.

- I fully agree with your point. We made re-calculation by changing the definition of p_{20} from mass fraction to surface area fraction. Because the dominant soil texture over the contaminated area was sandy loam and the ratio of surface-area-based p_{20} (=0.45) to mass-based p_{20} (=0.09) of sandy loam was approximately five, the re-calculated ^{137}Cs emission from soil increased by a factor of five for the whole region and time. On the other hand, a bug has been found in the previous simulation: $\rho_{b,soil}$ in Eq.(3) was calculated as porosity (0.4) times dust particle density (2650), but correctly, $\rho_{b,soil}$ is 1 minus the porosity (=0.6) times the density (2650), and so the corrected concentration decreased by 0.4/0.6 = 0.6666. Therefore, the final dust concentration is the previous dust concentration times 5 times 0.6666 = 3.3333. Consequently, the previous dust concentration times 10 is equivalent to the final dust concentration times 3. Because the previous concentration times 10 slightly underestimated the observation, the "unphysical parameter" of 5 was chosen for the final revised simulation. Please see that the observed and simulated medians with the parameter of 5 are consistent with each other as shown in Table 3, a new table in the revised manuscript. All the figures (i.e., 7-12) and quantities in the text have been replaced, accordingly.

2. For the same reason, the statement (p.14, line 1) "the flux might be a maximum estimate" seems impertinent. If the authors determined the rule-of-thumb value of 10 for the above-mentioned parameter to have reasonable air concentration values, the flux might not be a maximum estimation.

   As replied previously, we made the re-calculation using a surface-area based fraction. Now the new simulation is a maximum estimation in a sense that it does not consider any suppression effects due to precipitation, snow cover and land surface processes.

3. The results of sensitivity analysis in Table. 2 (the "range" line) is not informative. The reviewer cannot tell what kind of sensitivities exists from the ranges of statistical values.

   Yes, exactly. But the point here is the range of statistical values after optimization becomes generally smaller compared to the ranges after optimization, indicating that the optimization is successful excluding deposition parameters with worse performances. As to the "a kind of sensitivity exists in the ranges of statistical values", the relationship between the input parameters (deposition parameters) and statistical measures was elaborated from line 26 of page 12 in the previous manuscript as “Generally speaking *R* became higher …”. The author judged that the statement on the relationship was hardly reflected in Table 2, and so we just showed the ranges in Table 2 and described the relationship in the main text, separately. Thank you for your understanding.

4. There are statements that the surface air concentration has positive correlation with the surface wind speed (p.15, line 26 and p.26, line 15). However, there is no evidence of it. At least a statistics (e.g. correlation coefficient) is necessary.
   I added the correlation coefficients to the main text.

5. The discussion in Appendix C should be presented in the main text since it is substantial in discussing the significance of resuspension quantitatively. However, it is recommended that the last part of this part (p.27, Line 11-15) be changed or deleted since research has been done extensively; for example a paper by Kimiaki Saito and Nina Petoussi-Henss, Journal of Nuclear Science and Technology, 2014, Vol. 51, No. 10, 1274–1287, http://dx.doi.org/10.1080/00223131.2014.919885 has discussed the migration-dose rate relation. The group headed by Dr. Kimiaki Saito also conducted extensive field measurements on the dose rate trend and in-soil concentration distribution.
   Thank you for your suggestion and introducing Dr. Saito’s work. I moved Appendix C into the main text as Sect. 5.3 and changed Figure C1 to Figure 14. I deleted the corresponding paragraph and modified the sentence referring their work as well as Evrard et al. (2015) as follows: “Evrard et al. (2015) summarized that significant transfer of particulate-bound radiocesium occurs during major rainfall and runoff events (e.g. typhoons and spring snowmelt). Together with the relaxation depth – dose rate relationship provided by Saito and Petoussi-Hess (2014), the decreasing rate due to land surface processes such as downward migration, runoff, and erosion could be quantified and thus the decontamination effect could be separately extracted.