

Interactive comment on “Long-term assessment of airborne radiocesium after the Fukushima nuclear accident: Re-suspension from bare soil and forest ecosystems” by M. Kajino et al.

Anonymous Referee #3

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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.

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

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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-137 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.

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.

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

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.

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.

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