

## ***Interactive comment on “The effects of topography and soil properties on radiocesium concentrations in forest soils in Fukushima, Japan” by Misa Yasumiishi et al.***

### **Anonymous Referee #1**

Received and published: 18 November 2020

Dear Authors and Editors!

I with great pleasure read manuscript entitled “The effects of topography and soil properties on radiocesium concentrations in forest soils in Fukushima, Japan”. At first, please let me apologies for slow reply and postponing in review process. I had a seasonal flu. The Fukushima Dai-ichi nuclear accident lead to a great contamination by radioisotopes of a large area at Honshu island (Japan). The article describes a distribution of Cs-137 in forest soils at Fukushima prefecture. Such data is useful for current monitoring and forecasting of radiocesium concentration in study area. The paper fits to the “Earth Surface Dynamics” journal scope, but I have some suggestions and com-

C1

ments. Please see them below. 1. In Abstract, 1 sentence. When the soil samples were collected? Better to add the years. As I understand from text, they are: 2016, 2017 and 2018. 2. 3 line of Abstract. Instead “accumulation” better “distribution”. 3. LL 45. “Once released, Cs-137 takes two pathways in the environment. It may be dissolved in water (Iwagami et al., 2015; Osawa et al., 2018; Sakuma et al., 2018; Tsuji et al., 2016) or adsorbed into soil particles”. I suggest change to “Once released, Cs-137 may be dissolved in water (Iwagami et al., 2015; Osawa et al., 2018; Sakuma et al., 2018; Tsuji et al., 2016) or adsorbed into soil particles”. 4. Section 2.1. In my opinion, at the Site description the additional information is required and need some rewriting. In the beginning, better to show clearly which type of trees are covered the study area (in present version is could be found only in the middle part of section) and their Latin names, their average height and density; which types of soil are presented in study site; the angles of slopes (their range and values), as well m.a.s.l. 5. LL. 120. I guess “however, the FDNPP is not visible from this ridge” could be avoid. The FDNPP difficult to see from 35 km. 6. Fig. 5c. On some of the contour lines need to add the values. Also if possible - the Bergstrich lines. Much better if the maps are shown with a coordinate grid or put in the corner of maps the coordinates. 7. LL. 175. “. . .dried in an oven for about 24 hours at 105°C.” I hope that this time were enough for drying. You could add that the samples were dried till constant weight. 8. LL. 195. “In the top layer, the average water content percentage was above 100 % because some samples were very moist.” Probably is a mistake. For example, the field capacity of soil is average 30-50% and depends of soil texture. The field capacity is a moisture of “wet” soil, 2-3 days after rain or irrigation. 9. Table 4. In the name of table “. . .in this article” could be avoid. The sentence could be change to “Soil properties of the studied samples by depths.” 10. Table 4. Average water content (%). Does the values are correct? 1.22% is very dry for “wet” soil. It is near to hygroscopic moisture of soil.

The parts of article with statistical analyses and using of models seems excellent. I not specialist in modeling thus, I recommend to contact with researchers who are working in such topic. I found some articles, which are very close: - Uncertainty assessment

C2

method for the Cs-137 fallout inventory and penetration depth. - Modelling the extent of Cs-137 soil contamination patterns at the Kostica River basin (Bryansk Region, Russia). - Detailed study of post-Chernobyl Cs-137 redistribution in the soils of a small agricultural catchment (Tula region, Russia) It will be very nice if they (Drs. Papadakos, Linnik, Zhidkin) could give some recommendations on your paper.

Wish good luck in your present and future research work.

---

Interactive comment on Earth Surf. Dynam. Discuss., <https://doi.org/10.5194/esurf-2020-72>, 2020.