

General comments

The article presents clearly an important methodology concerned to radiological emergency preparedness. This methodology adopts cheaper resources than the traditional air concentration measurements. The work should be published.

Specific comments:

Following are the specific comments addressed to the work:

- 1) The Shershakov et al. (2019) reference on Table 1 is uncompleted. Please, complete or remove this reference from the Table.
- 2) Line 210: Table 1 shows highly different source terms. Why was the source term from Saunier et al. (2019) chosen? Were the others also tested?
- 3) Table 5 wasn't called anytime in the text. Please, identify each acronym and explore better those scores. What does explain the better correlation for the rain water experiment? Lines 244-245.
- 4) Line 311: "***These releases thus have a small effect on the deposition values***". Is the released quantity the only effect? Could the atmospheric conditions like wind speed and direction or atmospheric stability simulated by the model impact this result? Could the first partial release detected by the inversion method related with the wet deposition (Sep, 25th), in truth, refer to the release of the 2 previous days? Could the same release result on significant deposition values for a less favorable dispersion condition?
- 5) Line 392: "***We found an unexpectedly large impact of the resolution of the meteorological data in Sweden***". Why unexpectedly? Since higher resolution modelling can simulate better the physical processes, especially within the Planetary Boundary Layer, the improvement is totally expected. Please, rewrite this phrase.