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Comment on acp-2022-383

Anonymous Referee #2

Referee comment on "Spatio-temporal variation of radionuclide dispersion from nuclear power plant accidents using FLEXPART ensemble modeling" by Seyed Omid Nabavi et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2022-383-RC2, 2022

This is an interesting study on the uncertainty in modelling dispersion and deposition of radionuclide related to the use of: i) various driving meteorological input (forecast, analysis, downscaling) ii) two turbulence formulations in the dispersion model and iii) two formulations of the wet deposition. A hypothetical nuclear accident is considered, and two radioactive compounds are simulated characterized by very different half-life times. The paper is in my opinion suitable for publication on ACP and will be of interest not only within the community interested in nuclear accidents simulation but to all readers generally interested in dispersion modeling. However, I have several comments that the authors should consider before the paper can be published on ACP.

A general comment is that the authors use short (24h) releases (for 365 days) and follow these for 96h. If I understood correctly, in evaluating the impact of these releases the authors look for i) the (seasonal/annual) median of the maximum concentration over each period (24h release with 96h tracking) and ii) the median of the maximum (total over 96h) deposition over each period. However, usually the release from a nuclear accident has a longer (than 24h) duration.
The authors should clearly discuss in the paper how these results may be used to understand what happens in a real case (multiple days release). For example, can this be considered a sort of median daily worst-case scenario? Could it be converted linearly in a season/yearly median worst case by considering it over a longer release period in any season or over the year?

Other comments

- 1) Line 201. The authors write “The relatively lower spatial resolution of CFSv2 caused a smooth distribution of its simulated air parcel ages that is close to the average of other distributions”. This does not seem correct to me. For example, in fall, spring and summer (2A-all intensities) the value of CFSv2 before 25h is generally higher than all

other ensemble members (therefore cannot look like an average). Moreover, "by eye" the smoothness does not seem different to me (2A-all). I suggest avoiding this statement as it is not necessary for the discussion.

- 2) Line 210, "age distribution produced by FNL-WRF was found to be more similar to the one produced by ERA5-WRF than by FNL". This is very difficult to see from figure 2A-all in my opinion, it is somewhat clear in figure 2A-high. Did you use a metric? or is this a "by eye" evaluation?
- 3) Similarly, to (7) and (8) above, "Although the base model used for the production of FNL, the Global Forecast System (GFS), is also the atmospheric component of CFSv2, FNL age distributions look closer to those from ERA5- and FNL-WRF". From the figure 2A-all it is very difficult for me to see these claimed similarity/difference. Perhaps you need to add a distance metric that may objectively evaluate what distributions are closer to each other.
- 4) A general comment is that the discussion (lines 209-215) related to figure 2A-all (see point 7-9 above) comparing the age distributions over the whole 96h age interval seems not objective and perhaps not needed.
  I think that Plot 2A-all is useful for finding/pointing to specific differences that are obvious for a specific age intervals, e.g. the large peak in FNL and ERFA5-WRF in Winter at about 10hours, or e.g. what pointed out by authors in "air parcel ages are distributed in a wider range in all seasons in FNL (note the location of the first and last peaks", and afterward find the reason for the difference/similarity with a further analysis.
  On the other end the attempt to compare the full extension (all ages) and evaluate the overall similarity among (two or more) lines crossing repeatedly seems to me very difficult by eye (if not impossible). This comparision would need a specific metric objectively evaluating the overall distance between the lines.
  Concluding, in my opinion the authors should remove the discussions of 2A-all comparing curves over the whole extension or alternatively add a metric to evaluate the overall similarities/differences among the age distributions.
- 5)Please define exactly the density plotted in Figure (2.B-up) and their normalization. Obviously, particles released later in the day have a shorter travel time, i.e. particle released at 24 hours can only travel for 96h-24h=72h. What is the integral under the curves in 0-6h, 6-12h, 12-18h, 18-23h?
- 6)Line 256/ figure 3. The authors should add the formula used to define the deposition as plotted in figure 3. The current explanation (by words) lacks clarity, and the exact mathematical formulation should be added. Also include the definition of upper and lower bounds of the green shaded interval.
  Also, may you explain why the median in figure 3 occasionally decreases? Given the 30 years half-life of 137Cs, I would expect that in any grid cell the deposition increases toward its maximum at 96 hours. Therefore, the median should always increase.
- 7)Line 257, the authors write "To perform analysis related to radionuclide concentrations, the average of the simulations in the lowest four layers of the model between 5 to 100 meters has been used". Please add the mathematical definition. Are these layers evenly spaced? If not the average over the four layers should be defined accordingly to the different vertical extensions of the layers (please specify).
- 8)Figure 10, S10, S11. The quality of these plots is poor.
  8.1) I think that the full year should not be overlapped with the seasons.
  8.2) There is a lot of empty space on the right of the diagonal that can be used for plotting the full year separately.
  8.3) On the diagonal, the colored areas should be replaced with lines so that all the seasons can be clearly distinguished. What is the title of the vertical axis?

Minor comments

1) Eq. 2, dW_i should be dW_ j.

2) Line 100-101. “Wiener process with mean zero and variance dt”, the “dt” is missing.

3) Line 108. I think that Cassiani et al (2013) should be (2015) as the reference.

4) In table 1, add a further column indicating the deposition scheme used in FLEXPART (10.4 vs 9.02).

5) Line 202-203 the phrase “could not be so great ….” is unclear. Please rephrase it.

6)Line 203 what do you mean with “base concentration” ?

7) Line 239, may you clarify what do you mean with “to the further parts of the study area…. “. In relation with the peak at high particles age in the spring.

8)Figure 3, add axis titles on both the axes.

9)Figure 4 and 5, add the grid spacing in the axis and explain the units of the contour lines.

10) Line 354, “less than above thresholds” seems awkward language to me.

11) Figure 6A and 6B, in my opinion it would be better to use a unique (for all models) color scale here.

12) I think that Figure S11 should be included in the main manuscript since it is discussed in many details.

13) Line 474, what do you mean with “iteratively” here? The models were simply run for the 365 days in the year.