EGUsphere, author comment AC2
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Reply on RC2

Youness El-Ouartassy et al.

Author comment on "Combining short-range dispersion simulations with fine-scale meteorological ensembles: probabilistic indicators and evaluation during a ^{85}Kr field campaign" by Youness El-Ouartassy et al., EGUsphere, https://doi.org/10.5194/egusphere-2022-646-AC2, 2022

Acknowledgements

We thank the reviewers very much for their constructive comments which helped to improve the quality of the paper. In this letter below, we answer to all comments and explain how they have been addressed in the revised manuscript. We hope that this new version may be accepted for publication in Atmospheric Chemistry and Physics.

General Comments reviewer 2

The paper describes a probabilistic approach to study effects of meteorological uncertainties on atmospheric dispersion prediction at a scale of 2–20 km from source. A case study is performed using data of a two-month measurement campaign of the noble gas Kr-85 released from a reprocessing plant. These data are employed to evaluate a dispersion model driven by results of a high-resolution numerical weather prediction (NWP) model run in ensemble mode. The results of the study emphasize the value of introducing a probabilistic approach in dispersion modelling as compared to deterministic modelling. For the evaluation, two probabilistic scores are used, and for the dispersion modelling, two stability classifications are employed, and results are compared. It could be argued that the study on the two stability formulations is outside the main focus of the paper; however, I feel that it is still interesting to compare the results based on them.

The paper is well written, relevant and interesting both from a scientific and an application point of view

General Comments – answer from authors

In the revised version of the paper, these comments have been addressed in two ways:

1/ less emphasis has been made on the comparison between the Gaussian standard deviation formulas (Pasquill vs. Doury). In order to make the reader less distracted, and given that the main objective of the paper is the evaluation of the ensemble predictions, the section on statistical results (section 4.3.2) has been lightened by focusing only on the Pasquill method.

2/ the explanation of how and why we model dispersion over a time period which are longer than a single meteorological forecast has been improved. We believe it is an

important aspect of the simulation set-up, that is addressed in a short section of the manuscript.

Both aspects are covered in detail in the specific comments, and we hoped that this addresses the reviewers' concerns.

Specific comments

- **I think the abstract needs to be rephrased. In general, the standard of English language in the paper is good; however, this does not apply fully to the abstract. In addition, certain parts of the abstract are incomprehensible unless one has in fact read the paper, and thus the abstract does not comply with the intention that an abstract should be self-explanatory. As an example, the abstract contains the following sentence: "The results show that the stability diagnostics of Pasquill provides better dispersion simulations." Better than what? Furthermore: "In addition, the ensemble dispersion performs better than deterministic one, and the optimum decision threshold (PSS maximum) is 3 members." Members of what? Please rewrite the abstract to ensure that it is self-consistent.**

The abstract was rewritten according to the reviewers' comments. It is now self-consistent.

- **At a few places, reference is made to the work by Galmarini et al. using a multi-model approach. A brief discussion would be in place on the difference between using such approach and the probably more systematic approach constructing a dispersion model ensemble by using an NWP model ensemble.**

The following sentence is added in the text, Line 78:

However, this multi-model approach differs from the more systematic method based on meteorological ensembles, in the sense that the latter are built for each member to have the same probability.

- **In section 1.1 Uncertainties and ensemble simulations, reference is given to earlier work on the use of ensemble techniques for atmospheric dispersion modelling including the work by Sørensen et al. (2016, 2017 and 2019). It would be appropriate, e.g. in lines 33 and 48, to include also, or as appropriate to replace by, the paper:**

Sørensen, J.H., Bartnicki, J., Blixt Buhr, A.M., Feddersen, H., Hoe, S.C., Israelson, C., Klein, H., Lauritzen, B., Lindgren, J., Schönfeldt, F., Sigg, R. Uncertainties in atmospheric dispersion modelling during nuclear accidents. J. Environ. Radioact. 222 (2020) 1-10. https://doi.org/10.1016/j.jenvrad.2020.106356

Changes made in the reference.

- **In section 2.1 Case study, lines 116-117, it is mentioned that the release rate of Kr-85 is known with good accuracy. Please elaborate on this. What was the actual release rate, how was it measured, and how was the associated uncertainty estimated?**

Clarification was provided in the text in 1.2 and 2.1 paragraph.

The quantity of 85Kr released from the stacks was provided by RP at a measurement time of 10min. These data are confidential and cannot be described in detail in the paper. The

monitoring and control of the procedures leading to the provision of release data for the 2 stacks of the 2 plants in operation are subject to checks by the French authorities (ASN; Nuclear Safety Authority), and only annual aggregate data are public in the plant's annual environmental reports.

What it is possible to specify to you:

The release varies according to the industrial activity according to the years related to the tonnage of reprocessed fuels: so from 2019 to 2021 (Orano, 2021), the annual quantity of 85Kr released was between 294 and 379 PBq/year, which leads on average to activities in average of the order of 9.3 $10^9$to 1.2 10^{10}Bq/s. However, it should be noted that the release is intermittent even during periods of activity and can be completely stopped for several weeks. Having the data "in real time" at a 10-minute time step therefore justifies the term in the text "release with good accuracy".

In terms of uncertainty, the difference between a period without release (with the plant in operation) and a period with release is extremely clear, with a ratio of about 2 orders of magnitude on average (factor 100 to 150). It will be possible to go from value of the order of 3 $10^8$to 3 10^{10}Bq/s in a few minutes for a stack for example between 2 periods of fuel shearing. For each stack in each unit, 2 measurement channels are in place. The mean error between the two measurement channels varies from 7 to 10% for both units when ^{85}Kr is detected.

If the plant is completely shut down for several weeks (maintenance, failure), the release will be measured at even lower activity of the order of 10^6 Bq/s.

- **In the first paragraph of section 4.1.1, a way to build a continuous time series of NWP model data from consecutive forecast series is described involving skipping the first eight hours of a forecast series. However, I fail to see the point in the proposed method. In my understanding, modern data assimilation techniques ensure that NWP models are initialized very well and thus consistent also at short forecast lengths. I encourage the authors to argue for their method.**

The first 8 forecast hours were skipped to take into account the availability and transfer time of AROME-EPS data, wich take on average about 6 hours to be available since the run. Thus, to get closer to an operational situation, it is interesting to choose the most "recent and available" forecast, which is the one of 1500 UTC of the day D-1.

Changes made in the text, Line 330:

In other words, to simulate a release occurring *from 00h to 23h of a day D,* the AROME-EPS forecasts starting from 1500 UTC of the day before (D-1) are used.

- **In lines 335 and 336, the method used to diagnose the ABL height is mentioned supplemented by imposing a minimum of 200 m. However, no reference is given. Please, add a reference or elaborate on the method.**

In addition to the explanations given in the text about the ABL from AROME, the time series of the ABL from AROME-EPS (c.f. figure in attachment) confirms that there are times when it reaches unrealistic levels down to below 10 m. However, values below 200 meters are reached only a few times within the two-months period of interest, which means that the threshold value of 200 m should not significantly alter the simulations. This parameter is often not very influential on the pX simulations at short distance, because it is only used in cases where there are reflections on the inversion layer (not in stable situations), and only if the plume is sufficiently developed on the vertical.

Therefore, this threshold is only set to ensure that there are no cases where the release is above the ABL, because it would then be considered in the pX code that the ground concentration is zero.

There is no reference in the literature to justify the chosen value (200m). It is a usual threshold used operationally at IRSN for the above considerations. This value was found to be consistent with AROME data (see figure in attachment).

- **In Figs. 9, 10 and 11 appear a number of abbreviations, e.g. pc_mb1, stab_mb1, …, dd_mb3, ff_obs, …, mb3_stability5. Please explain these in figure captions.**

Clarifications was provided in the captions of Figures 9, 10 and 12:

- **In section 5. Conclusions and perspectives, line 498, is mentioned: "(…) allow them to correctly represent the uncertainties within ABL". Please elaborate on this. What is meant by "correctly represent"?**

Changes made in the text, Lines from 510 to 513:

For this reason, the meteorological ensembles were evaluated in terms of these two meteorological variables in 25 vertical levels within the ABL. The results of this evaluation showed that the AROME-EPS ensembles represent the wind in the ABL with a very acceptable accuracy, despite the slight systematic errors present in the lower layers.

- **In line 549, a mathematical equivalence is presented introducing a new mathematical function φ. This seems unnecessary to me. Please rephrase.**

Changes made in the text, Line 560 and Equation B2.

Please also note the supplement to this comment:
https://egusphere.copernicus.org/preprints/2022/egusphere-2022-646/egusphere-2022-646-AC2-supplement.pdf