



EGUsphere, referee comment RC2  
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## **Comment on egusphere-2022-646**

Anonymous Referee #2

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Referee comment on "Combining short-range dispersion simulations with fine-scale meteorological ensembles: probabilistic indicators and evaluation during a <sup>85</sup>Kr field campaign" by Youness El-Ouartassy et al., EGU Sphere, <https://doi.org/10.5194/egusphere-2022-646-RC2>, 2022

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### **General comments**

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

### **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.

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.

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>

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?

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.

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 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.

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"?

In line 549, a mathematical equivalence is presented introducing a new mathematical function  $\varphi$ . This seems unnecessary to me. Please rephrase.