



EGUsphere, author comment AC1
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Reply on RC1

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,
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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 1

The authors demonstrate the value of ensemble meteorology by showing how it can be used to model the uncertainty in the dispersion of material released from a known source. Where previous studies have focussed on long-range dispersion and used meteorology from global NWP models this study examines the use of a high-resolution (2.5km) ensemble NWP model covering a limited area to provide meteorological input to a dispersion model. In addition, the study focuses on modelling the dispersion of material that is regularly discharged from a reprocessing plant and compares the model results to observations over a period of two months. This field campaign along with the meteorological model and the dispersion model are clearly described within the paper.

My main complaint about this paper is that, for me, it covers too many topics. This has two impacts, first I am distracted from the main results and second the secondary topics are not covered in great detail so I am left with too many questions, as can be seen by the length of the specific comments section. There are two main sections which take my attention away from the main results. The first is the work looking at different methods to compute stability. The second is the consideration of how to model dispersion over time period which are longer than a single meteorological forecast. Both of these are interesting topics but, I feel they would be better placed in separate papers where they can be discussed more fully. I have included my specific comments on both of these sections in the specific comments.

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

- **In the Introduction many papers are mentioned and in some cases the work carried out is described. It would also be helpful to understand the results or outcomes of the work in those papers. For example, the authors note that evaluations of dispersion ensembles were performed by Le et al, (2021) and De Meutter and Delcloo (2022) but they don't say whether the ensembles were found to perform well or whether the use of ensembles provided more information. Similarly the authors mention the works of Galmarini et al., (2004a and b) in performing multi-model ensembles but do not say anything about the findings of those works.**

Changes made in the text, Line 59 :

In Le et al. (2021) and De Meutter and Delcloo (2022), an evaluation of the dispersion ensembles was performed by comparison to radiological observations in the environment, *and the results illustrate the added value of the use of weather ensembles for dispersion simulations.*

Changes made in the text, Line 74 :

This approach was extensively investigated in Galmarini et al. (2004a, b) by using a set of different ADM to construct an ensemble of simulations, either with identical or different input data, to represent the modelling uncertainties, and the results showed that the ensemble simulations allows to reduce the uncertainty related to the deterministic simulation.

- **Line 50: I'm not sure "coarse" is the appropriate word to use here as one of the studies referenced in the previous paragraph used meteorological data at a resolution of 2.5km which is not generally considered to be a coarse resolution.**

Changes made in the text, Line 51 :

All these studies were carried out at long distance and the ensembles used to represent weather uncertainties had coarse spatial and temporal resolution, except Leadbetter et al. (2022) who used also fine-scale weather ensembles with a horizontal resolution of about 2.5 x 2.5 km and 70 vertical levels.

- **Line 82: Is it possible to define "reasonable" in reference to the ^{85}Kr release? Is the error on the release rate known?**

The quantity of ^{85}Kr released to the atmosphere is measured by the operator with a

temporal accuracy of 10 minutes (confidential data) and an uncertainty of about 10% on the activity measured (calculated by the difference between the data obtained on 2 measurement channel during the period of release).

Changes made in the text, Line 86 :

The main sources of the ^{85}Kr in the atmosphere are reprocessing plants of spent nuclear fuel, from which the ^{85}Kr release can be known with accuracy (described in section 2.2).

Paragraph added, Lines from 148 to 151 :

The activity in ^{85}Kr released from the factory by the stacks (confidential data) is known with a time step of 10 minutes and an uncertainty of measurement of the order of 10% in period of release (two channels of measurements for each stack). The discharge being intermittent, this 10-min time step ensures a precision indispensable for atmospheric dispersion studies. From 2019 to 2021, annual releases of the ^{85}Kr varied from 294 to 379 PBq/year (Orano, 2021).

- **Line 115: Is it possible to provide an approximate activity concentration for the amount of ^{85}Kr naturally present in the environment or a ratio of the ^{85}Kr present in the environment to the amount of ^{85}Kr released by the reprocessing?**

Changes made in the text Lines from 120 to 124:

Background levels of ^{85}Kr in the atmosphere, excluding an industrial plume, are currently below 2 Bq.m⁻³ (Bollhofer et al, 2019). In nearby fields in the plume around the RP of La Hague (about 0-2 km), activities can reach 100,000 Bq/m³ (Connan et al 2014). At distances of the order of 20 km, the maximum measurable activities are generally less than 10000 Bq/m³ and beyond a few tens of km of RP, the activities in ^{85}Kr are too low to be measurable in real time (Connan et al 2013).

- **Line 117: Similar to line 82; is it possible to define “reasonable” in reference to the ^{85}Kr release? Is the error on the release rate known?**

See comments above

- **Line 125: In describing the terrain around La Hague as complex is it possible to provide values for the maximum and minimum elevations to provide meteorological readers with a reference point for how the terrain might affect the wind speed and direction?**

Changes made in the text, Line 133 :

The North-Cotentin peninsula of La Hague is a rocky area of approximately 15 km located at 190 m a.s.l above cliffs, surrounded by the sea less than 5 km in most directions (Fig. 1).

- **Line 136 and 137: For me the availability of data at a 10-minute resolution doesn't, on its own, constitute an accurate and reliable source term. I would be interested to know the uncertainty on the measurements relative to the amount of material released.**

The following sentence is deleted: Line 136 and 137 [The sum of the amounts of ^{85}Kr released from UP2 and UP3 units, over regular 10 minutes..... and reliable source term.] Measurement uncertainty relative to source term was added in Line 149 (see comment

above).

- **Table 2: Would it possible to add the temporal resolution of the met data to the table? I think this is mentioned later on in the text but it would be helpful to include it in this table too.**

Changes made in the table 2.

- **Section 3.1: There are a large number of different skill scores which could be used for the verification of both deterministic and ensemble predictions. Would it be possible for the authors to include an explanation of why bias and spread-skill were chosen?**

The choice is based on subsequent work by the meteorological community. The most commonly used scores for evaluating the reliability of ensembles (the ability of the meteorological ensembles to represent realistic uncertainties) are the spread-skill ratio and rank diagrams (not shown in the paper), which are complementary scores. On the other hand, the bias allows to identify the systematic errors of the weather predictions, as explained in the text.

Changes made in the text, Line 267:

For this purpose, two common scores, among others, used by the meteorological community for the evaluation of ensemble reliability, have been calculated based on the observations of 3D-wind speed and direction...

- **Figure 3: In the text the authors mention that there is a diurnal cycle in the bias, but I find this difficult to see because the bias shares the same axis as the mean values. Would it be possible to place the bias on a separate axis to the mean values?**

Changes made in the Figure 3.

- **Section 4.1.1: I am very surprised that it is necessary to use more than one 24-hour forecast for this study. The furthest observation point is situated <20km from the release location and assuming direct transport it would only take more than 24 hours to travel this distance if the mean winds for the whole 24 hours were less than 0.8m/s. In addition, 9 further hours of the first forecast were also still available so it would be possible to carry out a 36-hour forecast without needing to combine meteorological data from different days.**

The release of krypton-85 from the source is almost permanent. Thus, the dispersion calculation is done over long continuous periods where the plume goes over the measurement sites (c.f. Table 5). In order to have continuous weather forecasts that cover the whole calculation period, several forecasts starting from different initial instants must be combined successively. Different choices can be done to combine weather forecasts. The method used in this paper is to cover every day by a single forecast by taking into account real-time running constraints, since operational warning is the final purpose of such study. Some details are provided in the two comments below. The section (4.1.1) that described this aspect has been modified to better explain the choice done in the study.

- **Line 319: I am curious to understand why the first 8 forecast hours were skipped? Is this a recommendation of the developers of AROME-EPS or is it due to the location of the release relative to the edge of the meteorological model domain?**

In addition to the explanations given in the previous comment, the first 8 forecast hours are skipped to take into account the availability and transfer time of AROME-EPS data, which take on average about 6 hours to be available from the beginning of the run. Thus, to get closer to an operational situation, it is interesting to choose the most "recent and available" forecast to the beginning of a day D (00h), which is the forecast of 1500 UTC of the day D-1.

- **Line 320: In table 2, the authors state that AROME-EPS is run four times a day, so I was wondering why model runs which are 24 hours apart are stitched together to build a continuous time series rather than model runs which are only 6 hours apart. My feeling is that using model runs which are 6 hours apart would reduce jumps at the forecast joins.**

In general, forecasts are not available immediately, they take on average about 6 hours to be available since the beginning of the run. Thus, to cover a day D, taking into account the accidental context, it is interesting to choose the most "recent and available" forecast the most which is used to anticipate the next 24 hours, which is the period when decision-making is required. Moreover, recently we tested the method using the 4 forecasts with a change of the ensembles every 6 hours, and the results were not better than the method used in the paper.

- **Line 333: Were the comparisons made in section 3 carried out using the unprocessed or processed meteorological fields?**

The meteorological evaluation was done on unprocessed data. Here, the word "processing" refers to the process of projecting and interpolating the meteorological data (which are defined on a lon/lat regular grid) onto a Cartesian grid so that they fit the form readable by the dispersion model used.

- **Line 337 and 338: Are the authors able to comment on the impact of setting the minimum ABL height to 200m and/or provide evidence that this is a reasonable minimum ABL for the study area?**

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.

- **In Figure 7 and 8 I find it difficult to determine where the peaks in the ensemble are as the grey lines overlap a lot. Would it be possible to plot the ensemble as a shaded area rather than individual grey lines?**

Changes made in the Figure 7 and 8.

- **Line 362. The authors state that the use of a stack height of 100m does not allow them to accurately predict concentrations at 2km from the source in stable situations. Please could they expand on why stable conditions are problematic for the dispersion model they are using.**

Stable situations were found to be more tricky to reproduce in previous dispersion studies in this area (Connan et al, 2013, Korsakissok et al, 2016), due to the difficulty of the model to accurately simulate vertical plume spread and the fact that building downwash effects are not taken into account.

Changes made in the text, Line 376:

In our simulations, the use of the stack height (100m) as release height does not allow to accurately predict significant ground concentrations at this distance, *due to approximations made in the Gaussian model that does not include building downwash effects*. This is especially *the case when using Doury standard deviations, ...*

- **Line 365: What does the word “this” refer to in the sentence which begins in this line?**

Changes made in the text Line 378:

This phenomenon, that characterizes pX-Doury simulations in stable situations, was specifically shown in the case of La Hague RP.

- **Line 375, figure 9 and table 4: Reading table 4 I think that peaks 2, 3 and 4 are much smaller than peaks 1 and 5. For me it would be helpful for this to mentioned in the text.**

Changes made in the text Line 390:

Table 4 summarizes the five observed peaks (*with peaks 2, 3 and 4 are much smaller than peaks 1 and 5*) from 08 Dec. 2020 to 12 Dec. 2020, when the ensemble behaviour is studied.

- **Line 381: I think, in this sentence, the authors are arguing that the peaks are small because they are located close to the edge of the plume where the concentration gradients are high. It would be helpful to see a figure showing this. In addition, the authors appear to be suggesting that the solution to the underprediction is simply to increase the width of the plume which could be done by changing the stability category. Firstly, I would be interested to see why the authors believe that the inability of the model to predict the peaks is due to the stability and not to the wind speeds and directions along the path the puff has taken from the source location. Secondly, increasing the spread of the plume may help the model to capture the peaks where they are located at the edge of the plume, but this will be at the expense of the magnitude of the peaks where they are located at or close to the centre of the plume. Finally, given the emphasis placed on the stability within the second half of the paper I would be interested to see comment in the first half of the paper on the meteorological variables which impact on the calculation of the stability.**

The requested figure is added (Fig. 11), and the following sentence is added in Line 397:

Figure 11 illustrates this issue in the case of the 3rd peak of member 1 in Fig.9 and Table 4. This peak underestimates the air concentration because it is located close to the edge of the plume where the concentration gradients are expected to be high.

Firstly, it is assumed that the model failures are probably more related to the stability than to the wind, because we find that the wind forecasts are sufficiently accurate, and that the wind direction values given by the different members are very similar while the stability is more variable (Fig.9 and Fig.10).

We agree with the reviewer's statement that, if the stability diagnosis leads to an increase the spread of the plume, the intensity of the peaks detected near the plume's centreline will decrease. Although we have a fair spatial coverage of the area, our network density does not allow us to determine whether a sensor located within the plume would be simulated with less accuracy, should the plume spread increase in this particular case.

Finally, in the evaluation of the meteorological ensembles, particular attention was paid to wind data, since an extensive database of wind measurements is available. On the other hand, for the temperature (which is a key parameter in the stability calculation), we have no in situ measurements to justify the temperature gradient on the vertical in the study area. Thus, we think it is all the more interesting to highlight the importance of the stability diagnosis since it is difficult to evaluate a priori the ensemble's quality on this respect – this leaves room for future work on this subject.

- **Line 403: Within the literature there are a number of different techniques proposed for the assessment of the performance of ensembles. Would it be possible for the authors to briefly explain why they selected the method of Querel, 2022 which is designed for the assessment of deterministic simulations?**

In our case, we are particularly interested in the evaluation of the capacity of the ensembles to anticipate the exceedance of a given threshold. In this case, there are no other scores that can be used other than the scores based on the contingency table, which are extensively described in Wilks, 2006. The choice of some of the many scores based on these tables depends on the purpose of the study, and the particular features of the ensemble that we wish to evaluate. In the case of deterministic simulations, the method presented in Quérel, 2022 is the most suitable for our application, except that it is not possible to implement it in the case of ensemble simulations for the reasons described in the paper (section 4.3.1). Thus, we chose to use similar scores, but within a framework adapted to probabilistic forecast. To our knowledge, there is no other work in the literature that proposes other methods to evaluate the atmospheric dispersion ensembles in terms of occurrence/non-occurrence of threshold exceedance.

- **Line 462, 463: Can I just check that the statement made on these two lines refers to the assessment carried out with parameters $\Delta T=3h$, $\tau=2h$?**

Yes.

- **Lines 523-525: Suggest removing this paragraph or expanding it substantially. Clustering has been tried with dispersion ensembles (Klonner, 2013) and was not found to be useful with the boundary layer.**

The paragraph was removed.

Technical Corrections

Line 13: Replace "As first step" with "As a first step"

The abstract was rewritten.

Line 19: Replace "than deterministic one" with "than the deterministic one"

The abstract was rewritten.

Line 60: "demonstrate" rather than "examine"?

Changes made in the text

Line 60: "skillfully" rather than "skillful"

Changes made in the text

Line 146: Suggest replacing "which means it does not generate chemical or physical reactions" with "it is not chemically or physically reactive".

Changes made in the text

Line 166: Suggest adding "(in the absence of deposition)" between "shown that" and "3-D wind field".

Changes made in the text

Line 281: The range -0.2 to 1.75 m/s doesn't appear to match the range in Figure 3.

Changes made in the text : *0.71 to 1.45 m/s.*

Line 285: +10 and -15 don't appear to match the minimum and maximum values in Figure 3.

Changes made in the text : *-13.3 and 7.8 °.*

Figure 4: In the y-axis labels what do the "dd" and "ff" mean?

Explanation added in the caption of Figure 4: "ff" and "dd" means the wind speed and direction, respectively.

Table 3: Please could the authors separate the bias and spread-skill columns and place separate wind direction and wind speed labels above them as I'm not sure what each column represents.

Changes made in Table 3.

Line 399: I think this reference should be Wilks, 2006 not Daniel and Wilks, 2006.

Changes made in the reference.

Line 406: Replace "because the" with "because there"

Changes made in the text.

Figure 10: Would it be possible to explain the meaning of "dd" and "ff" in the figure caption?

Explanation added in the caption of Figure 10.

Line 419: For clarity I suggest using the same language here as in the definition of "TN" in line 421

Changes made in the text.

Line 592: As mentioned previously I thin this reference should be Wilks, 2006 not Daniel and Wilks, 2006.

Changes made in the reference.

Line 653: Leadbetter, Jones and Hort has now been published and can be found here:
<https://acp.copernicus.org/articles/22/577/2022/>

Changes made in the reference.

Please also note the supplement to this comment:

<https://egusphere.copernicus.org/preprints/2022/egusphere-2022-646/egusphere-2022-646-AC1-supplement.pdf>