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Discussions

Interactive comment on "Model-based aviation advice on distal volcanic ash clouds by assimilating aircraft in-situ measurements" *by* G. Fu et al.

Anonymous Referee #2

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General comments:

The article represents a nice study treating the application of an Ensemble Kalman Filter on the prediction of volcanic ash plumes distal to the emission source, exploiting the observational information of aircraft-performed mass concentration measurements. This is innovative in terms of volcanic ash dispersion forecast improvements for aviation advice because it exploits real observational in-situ data by assimilation. The authors discuss the results as well as validate the assimilation performance using independent measurements. However, especially the discussion of assimilation impacts on subsequent ash dispersion predictions and thereon based aviation advices suffer from imprecise definitions of influenced areas. Further, some stated terms, interpretations,

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and conclusions (see below) are deceptive so that reformulation and clarification of these issues are requested, while additional calculations are not required. Furthermore grammatical corrections are necessary in some sentences.

Specific comments:

2.1: It should be clarified why the influence of ESPs weakens with the distance to the volcano.

2.9: The difficulty of satellite data assimilation should be discussed carefully. Satellite lidar instruments like CALIOP or CATS provide highly resolved aerosol profiles. The challenge of satellite data assimilation is that the observations are often no direct measurements of the quantity of interest, but optical property measurements. Therefore the aerosol quantity need to be derived by a retrieval process or a complex observation operator.

2.12: Measurements "close to the eruption plume", this formulation might be misleading, since the article focuses on distal ash clouds.

2.16: Lidars do not provide vertical integrated column data.

2.19: It remains unclear how research flight operators know where to actually obtain the "most relevant" volcanic ash concentration.

2.25: It should be formulated what kind of estimates are highly influenced by "plume height and mass eruption rate".

4.2: "for this type of measurements in well calibrated cases." To me it is unclear what "well calibrated cases" are and whether you have those during the performed flight.

4. 29 (and Chapter 2.3 in general): To me it is not clear how the forecast error covariance matrix looks and especially how the localization is performed here.

5.12: You describe that there are no ash emissions prior to 9:00 UTC 14 April 2010, but it remains undiscussed how the volcanic ash is released during the first guess forecast

(PH, MER, VMD).

5.17: The ”plume height detection data” has to be described more precisely.

5.30: “... approximates the measurements with a high accuracy” -> If I did not see the corresponding figure, I would expect the analysis to be identically equal to the observations.

6.21: “. . . continental Europe is all simulated in a low concentration level . . .” -> This sentence needs some reformulation, since the analysis does not apply to all continental Europe, e.g. in Northern Netherlands there still appear high ash concentration values.

6.23: “Thus in a fairly large domain, the state change at measurement location also influences state variables in the surrounding areas.” -> To my understanding this is caused by the chosen influence radius, which is not discussed in this study. Further, the downwind direction includes influenced states due to the transport of earlier corrected ash concentrations, especially regarding forecasts later than the assimilated time steps.

7.4: “Fig. 4 shows the difference of a number of . . .” -> There is no difference plot in Fig. 4. Please be more precise, e.g. “the comparison of Fig. 4a and 4b. . .”. And it remains unclear which “number” you refer here. Aren’t these just two forecasts?

7.6: “initiated with Fig. 3c” -> Fig. 3c includes results of an assimilation process as well. Later, the forecasts using these initial values are always designated to be ”without assimilation”. Please resolve this issue more clearly.

7.11: It is unclear where the area of “downwind direction” is, since the wind field is nowhere described or visualized.

7.18 - 21: “. . . at times between 13:20 and 14:00 . . . are far away from the measurement track” Actually these observations are not in the area, which is influenced by the assimilation (see Fig. 3e/3f). Therefore, it is unclear how these observations can be utilized for validation. For validation independent data has to be chosen, but it must be

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selected carefully with respect to the influenced area. Additionally, these observations are in a much better agreement with the measurements than the observations taken before 13:20 UTC and after 14:00 UTC. This could lead to the interpretation that the first guess (without any assimilation) corresponds much better with the observations than the forecasts with assimilation. Please reconsider the choice of observations for validation.

7.23: Declaring the forecast results with assimilation to be "much closer to the measurements than the forecast without assimilation" for the time steps between 12:30 UTC and 13:20 UTC might be too optimistic.

Comment on Chap. 4 and 5: It remains unclear to me how the length of integration time after the assimilation can influence the forecast results. It should be discussed how the area influenced by the assimilation temporally propagates.

7.32: It has to be clarified if Antwerp and Brussels are even in the area, which is influenced by the assimilation.

8.1: In my opinion there are no continental passenger flights that operate in an altitude of 3 km. 3 km might be of special interest regarding take off and landing.

Comment on the aviation advice: Clarify that you give the aviation advice only on the strength of the results in 3 km height. Generally all model levels must be analyzed for real cases. And it should be pointed out for which exact area and which time frame the aviation advice is given.

8.21: "most of the flights in East direction" I think it is not a matter of the flight direction; it affects all flights in the ash penetrated area.

8.24: "the whole of Europe" This has to be reconsidered. Not all Europe is analyzed.

8.32: "... the assimilation impact will last at least one day." Such conclusions have to be discussed carefully. This conclusion is only valid for the areas, that are influenced by the assimilation, which changes with time, and as long as there are no high concentrated plumes transported into the area of interest. You might have to reconsider this statement especially for regions upwind to the assimilated observations.

8.34: “Only small differences are detected in the Northern part of Italy.” In general I recognize only small differences at all cloud edges.

9.10: “using only one or two measurements can not produce accurate results.” How do you know that at the number of measurements you used for your analysis is sufficient? Using aircraft measurements the number of observations only influences the area which can be analyzed. This does not mean, that the assimilation of one or two measurements is not valuable for a certain region.

9.25: “aviation advice can significantly benefit from the ensemble-based data assimilation process” Here it should be pointed out again, that this is only true within the influence radius/the assimilation influenced area.

9.27: “we suggest the frequency of the measurement campaign to be once per day.” This cannot be said without loss of generality. Be attentive with the area that is influenced by the assimilation and the temporal change due to wind induced transport.

Comments on Figures:

Fig. 1b and 1c: could be left out, since there is no special meaning to the study or their meaning should be pointed out in the text.

Fig. 2 and 3: Which level/height is shown? Why is this level chosen?

Fig. 3e/3f: For me it is unclear how the differences of a-c and b-d can be a constant absolute value of 5. I would expect less difference between forecast and analysis at the edges of the influenced region. In addition, it is of special meaning if the assimilation induces reductions or increases of volcanic ash. Therefore, I suggest to define an extra color table for Figure 3e and 3f, which resolves the variation of the differences.

Fig. 4a/4b: The chosen colors of the aviation track might be misleading because they are included in the color table.

Fig. 6: This figure shows a different domain compared to Fig. 1a,2,3,4. Be aware that not the whole domain was influenced by the assimilation.

Technical corrections:

I suggest writing UTC after every time statement.

2.22: “It has been shown. . .” should be corrected to “It was shown. . .“

2.24: “near to the eruption” -> Suggestion: “close to the eruption location”; “In this study” should be changed to “In that study”

2.30: “an Iceland eruption” -> Suggestion: “a volcanic eruption in Iceland”

3.16: “volcanic ash simulations” -> Suggestion: “volcanic ash dispersion simulations”

3.28: “All of the measurement flights were. . .” -> Suggestion: “The measurement aircraft was..” (corresponding to the reference of Fig. 1b).

4.16 and 4.21: “N state” -> Suggestion: “N states” and “N volcanic ash state” -> Suggestion: “N volcanic ash states”

4.29 and 5.5: define “ ‘ ” to represent the transposed of the matrix

5.17: PH is already defined on page 3 line 18 and again on page 5 line 18

5.27: “of some of the ensembles” -> Suggestion: “of selected ensemble members”

5.29: “but the overestimation vanishes by the assimilation process” -> Suggestion: “but the overestimation diminishes by the assimilation process significantly”

7.7: “a better forecast will be. . .” -> Suggestion: “a better forecast is expected . . .”

9.10: “can not” should be corrected to “cannot”

10.2: “in satellite data.” -> suggestion: “in certain satellite data.”

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