

Reply to the editors final comments.

Topical Editor Decision for “Vertically-resolved probabilistic volcanic ash analysis using the chemical part of the Ensemble for Stochastic Integration of Atmospheric Simulations (ESIAS-chem) version 1.0” by P. Franke, A. C.. Lange and H. Elbern.

I completed two initial Topical Editor reviews of this manuscript (beginning and end of April 2021), a version with revised Abstract and Introduction proceeding then to expert peer review in May 2021.

Three reviewers posted comments during the review period (May to July), with the authors uploading a revised manuscript, ATC version and replies to the reviewers comments at the end of September.

I can see that the authors have replied comprehensively to each of the reviewers’ comments, in each case their comments minor in nature, and the manuscript has improved substantially as a result.

All three expert reviewers recommended publication once the required revisions were made, and I can confirm that the authors have addressed these appropriately, both in their replies and in the revised manuscript.

Upon reviewing the revised manuscript however, I did notice two areas where I still feel some aspects are not sufficiently explained.

I am therefore recommending publication once 4 further issues are remedied, or explained, this additional Topical Editor review only for these few minor comments however.

We thank the editor for his final remarks and the thorough revision of the manuscript. We have answered the editor’s comments below in blue:

1) On page 13 (lines 340-341), the authors explain:

“Column mass loading of volcanic ash in [gm^{-2}] is extracted as fictional observation data y_i , every 6 hours, from a ‘nature run’, simulated by the forward model of EURAD-IM. “

I understand the basis for this approach, in using the higher resolution EURAD-IM dispersion model, for two alternative scenarios of a sub-Plinian Eyjafjallajokull eruption, to provide synthetic observations that can then be used to test the ESIAS-chem simulations with the the data assimilation.

What I don’t understand though, is that the text states (line 340) that these “fictional observation data” are extracted from the model only every 6 hours, this being presented then as a proxy for a data stream representative of the SEVIRI geostationary satellite.

Clearly the geostationary satellite will provide measurements at a much higher temporal resolution than “every 6 hours”, with data every 30 mins or 1 hour being how the system will then be able to adjust/weight its ensemble predictions with the technique described.

I’m assuming that “every 6 hours” must be the authors referring to the data-flows, with perhaps a block of 12 sets of 30-min resolution data extracted every 6 hours. That might well have been obvious to the expert reviewers, but as currently worded, that’s a confusing mis-match to the approach for the synthetic observations representative of a geostationary satellite’s monitoring of the volcanic plume/cloud’s progressing dispersion.

Related to this point, reading the reviewer’s reply to the 1st of reviewer 1’s comments, to revised the Abstract sentence previously beginning “The system validation”, now revised to “Thus, the proposed system” (lines 5-6), I’m suggesting also to highlight the benefits of the geostationary datasets, that primarily being the high temporal resolution that can then constrain how the model predicts that the plume/cloud develops in the initial days (e.g. with the ash particles sedimenting alongside any co-emitted sulphur dioxide oxidising to sulphate aerosol, and the evolving wind shear etc.).

I’m not suggesting to add those specifics of the particles involved, but simply to add “2D high temporal resolution” before “column mass loading data” within that new merged sentence of the Abstract. Also, the word “imagery” (at the end of that sentence, line 6) can be deleted as the word “data” provided earlier already communicates this sufficiently.

Specifically, I’m requesting the authors need to revise that sentence in section 3.1 (page 13, lines 340-341) to state the temporal frequency with which the model data is being used to test the model (being representative of dataset to be provided from geostationary satellite).

And proposing they also make that edit to the new merged sentence on lines 5-6 of the Abstract: -- the suggested edit to insert “2D high temporal resolution” before “column mass loading data”.

We thank the editor for his remark. We would like to take the chance to explain our choice of observational data more comprehensively. We have extracted observations only every six hours from the nature run. As we have added in the text (line342-343), we are aware that this time step is significantly larger than for current geostationary satellites. However, we would like to emphasize that the fictional observations, which we use for assimilation, are in general very idealized:

- 1) We take observations from the full domain, including ash-free areas;
- 2) We omit any effect of clouds that may influence/hinder the retrieval of a geostationary satellite.

With this setting, the volcanic ash cloud is well observed at observation time. Thus, additional observations in between the six hourly time interval provide no substantial information to the assimilation system. In real applications we have a situation, where both volcanic emissions and domain-wide observations are quasi-continuously given, impeding any attribution of time-height ash emissions to later ash column retrievals. In fact, while the restriction of the data flux

to 6 hour intervals in our analysis reduces the information used, it helps to attribute column mass loading observations to older and recent emissions within the chosen resolution. The coarse observational interval is therefore a mere practical and easily alterable approach, not suggesting any abandonment of observational data streams.

We have added the following text at the end of the respective paragraph:

“Quasi-continuous data streams of emissions and observations, as available in real applications, prohibit any attribution of time-height ash emissions to later ash column retrievals for test purposes. In fact, while the restriction of the data flux to 6 hourly time intervals in our test scenarios reduces the information used, it helps to attribute column mass loading observations to older and recent emissions within the chosen resolution. We found a six hours interval for column mass loading data supply practicable. For later operational purposes the use of the full high frequency data supply is readily adaptable.”

As kindly suggested by the editor, we have changed the sentence (line 5-6) in the abstract to: “Thus, the proposed system addresses the special challenge of analyzing the vertical profile of volcanic ash clouds given only 2D high temporal resolution column mass loading data as retrieved by geostationary satellites.”

2) The phrase “nature run” is used throughout the paper, a term I was not familiar with.

I would have expected the terminology “synthetic observations” (or similar term) to convey the fact these are proxy for measurement data, whereas the authors provide a term that refers back to the model run that produced them.

Since none of the 3 expert reviewers have queried this term, I’m not proposing to change that, it obviously not critical to the presentation of the methodology, which particular name is given for a specific aspect.

However, I noticed this term “nature run” is used twice on Page 11 without introduction (section 2.2, lines 290 and 291), whereas it’s use later in the manuscript (section 3.1, page 13, line 341) does have a brief introductory explanation of the term.

Please provide an initial definition for that term, and consider whether to change to using “synthetic observations” rather than “nature run”, reserving the latter term for where the text is specifically referring to the simulation the generated it.

The editor is right. We have used the term “nature run” before providing a definition, which has now been amended. In general, the fictional observations pose only a small fraction of the data that is used to compare the volcanic ash simulated by the analysis ensemble with the volcanic ash of the nature run (which is considered the truth in this experimental setup). Thus, it is more appropriate to use “nature run” instead of “fictional observations”. We have added an explanation of this at the beginning of Section 2.2:

“As ESIAS-chem is tested by identical twin experiments (cf. Section 3 for more details), results of the analysis are compared to a "nature run". In this experimental setting, the nature run is

considered to represent the truth. Synthetic observations are simulated by extracting volcanic ash column mass loading data from this nature run. These synthetic observations show only a small fraction of the data that is used to validate the analysis ensemble. Thus, the following test procedures compare volcanic ash simulated by the analysis ensemble with volcanic ash simulated by the nature run rather than only with the extracted observations.”

3) Further edit to improve the revised caption to Figure 2 (page 13)

Reviewer 3 requested to simplify this caption (first bullet point of their comments), and I think they were referring to the “dependence on the assimilation window” which seemed not to be relevant to that Figure. However, the new shortened title, could be improved to better communicate the relevance of the emission profile shown in the Figure.

The main part of the new revised Figure 2 caption currently says “Hovmoeller plot of the nature run emissions profile used in this study”.

Related to comment 2), if the authors prefer to keep to the “nature run” terminology, I’d suggest this Figure caption could be an opportunity to re-iterate that this run is actually providing a dataset to be representative of measurements provided from geostationary satellite – possibly with a descriptor mentioning the specific instrument cited in the Introduction (i.e. “SEVIRI-like” or similar).

Also, even if the “nature run” terminology is retained, including that term prior to “emission profile” is poor grammar, the object of the sentence being that “emission profile”.

Specifically I’m suggesting to move “nature run” to instead be after “used in” and “before this study”, inserting “the” before it, and “providing the SEVIRI-like synthetic observations for” after it.

Suggest also maybe change “study” to “ESIAS system tests” at the end of the sentence

I mean to revise that first sentence of the Abstract from:

“Hovmoeller plot of the nature run emissions profile used in this study”.

Instead to

“Hovmoeller plot of the emissions profile used in the nature run providing the SEVIRI-like synthetic observations for the ESIAS system tests”.

or similar wording that will help the time-pressed reader scan the paper to understand the unpacking of the “nature run” term.

We thank the editor for this helpful comment. We have changed the figure caption as suggested:

“Hovmoeller-like plot of the volcanic ash emission profile used in the nature run to generate the synthetic observations simulating SEVIRI-like column mass loading data for the ESIAS-chem system tests.”

4) Page 11, section 2.2, lines 291-294 --- Text here not clear re: explaining the pcc cases.

The word “disjoint” is used in lines 291-292 in reference to a correlation coefficient of zero, but that term seems somehow inappropriate for this case of two uncorrelated datasets.

Furthermore, the follow-on sentence re: how to interpret the $pcc < 1$ case seems incorrect, unless I misunderstand the explanation here in relation to the experiment described.

The current text says “indicates that the analysis contains volcanic ash either in model layers or at times, where no volcanic ash is emitted in the nature run”

That doesn’t make sense at all to me – Figure 2 shows the emission profile for the “nature run” and clearly the majority of the re-constraints will be after volcanic ash has been emitted, as the plume is dispersing and the geostationary satellite will be able to identify where the cloud is dispersing to.

I understand that applying a weighting based on comparison to a test dataset can then weight the predictions towards an optimised input for the predictions. But this pattern correlation coefficient text here seemed to be describing something else here.

Please re-word these 2 sentences to better communicate the interpretation of these pattern correlation coefficient metrics.

We apologize that our description of the pcc score is insufficient. We have changed the sentence containing “disjoint” to:

“If ash cloud covers of the nature run and of the ensemble mean match nowhere, the pattern correlation coefficient equals 0.”

Further, we have removed the sentence, in which we discuss the case $pcc < 1$, from this paragraph. We used the pcc score to validate the extend of the volcanic ash cloud and not to make any further judgments on the underlying emission patterns. Thus, the specific sentence is of minor relevance for the study, and we removed it to avoid further confusion.