

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.

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 Eyjafjallajökull 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".

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.

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.

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 misunderstanding 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.