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Atmospheric Chemistry and Physics
Discussions

Interactive comment on "Satellite data assimilation to improve forecasts of volcanic ash concentrations" *by* Guangliang Fu et al.

Anonymous Referee #1

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1 Recommendation

Due to major shortcomings I suggest to **reject the manuscript**. I encourage the authors to resubmit their important and promising approach after a general revision of the study.
Here are my main concerns:

1. There is one critical assumption made in the manuscript. The authors assume

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that layers of volcanic ash are **always thicker** than 500 m (called T_{low} throughout the manuscript). While this might be true for mean values, this assumption is not at all justified in general. There are papers reporting on layers which are **typically smaller than 400 m** (Prata et al, 2015), and Prata and Prata (2012) report on volcanic ash *"vertically localized in thin layers of 200 – 1000 m depth"* during the airspace closure, and to the reviewers point of view, there is no reason why layers with an extent of, e.g., 50 m should not be possible (cases with strong wind shear). Also keep in mind that in general airborne measurements are likely biased towards thicker layers (as the flight pattern is not selected randomly and thicker and more prominent layers might be preferred in flight planning). At first glance one might think that this is a minor shortcoming. Unfortunately the minimum vertical thickness plays a key role throughout the manuscript and in the concept of the Satellite Observational Operator (SOO). Therefore this issue can not be solved by a "simple" revision of the manuscript. It might require a rerun of the simulations and probably a general revision of the SOO conceptual setup.

2. The second main criticism is that the results shown in the paper are far from being sufficient to support the general conclusions. The performance of the SOO and the assimilation method is tested **only for one specific day** of the Eyjafjallajökull eruption phase. There is a plenty of other days available with volcanic ash in the European airspace, from the Eyjafjallajökull period as well as the Grimsvotn eruptions in 2011, or also the Etna, and for many of these days there are airborne measurements available (Weber et al.,2012; Marenco et al., 2011; Schumann et al., 2011). **A one day case study is not an appropriate basis for the very general conclusions** (*"... improves the forecast... ", "... significantly improves the quality of the advice ..."*.)

3. The authors don't compare their results with **current** state-of-the-art VADTMs like the ones **currently implemented** in the VAACs. The only benchmark the authors take into account is their own model output without assimilation. This

is not sufficient for their general conclusions like (".. quality of advice given to aviation over continental Europe is improved").

2 General comments

In this section the questions listed in http://www.atmospheric-chemistry-and-physics.net/peer_review/review_criteria.html are addressed.

Does the paper address relevant scientific questions within the scope of ACP? Does the paper present novel concepts, ideas, tools, or data?

The manuscript describes a method to translate 2D fields of volcanic ash concentration, particle size distributions and top altitude of ash layers, as derived from satellite passive remote sensing, into three dimensional fields of the ash concentration (mass per area –> mass per volume). 3D information including vertical distribution is important, as VATDMs need 3D information of volcanic ash concentration for assimilation in order to allow accurate forecasts of volcanic ash. The SOO allows continuous assimilation of the model forecast. The authors claim that using this 3D information allows for significant improvements of the quality of volcanic ash forecasts. Therefore the paper deals with an important field of research, with high relevance to remote sensing specialists, VATDM modellers, and other key player like VAACs and NMS, and the scientific issues are within the scope of ACP. The concept is novel and worth to be investigated.

Are substantial conclusions reached? Are the scientific methods and assumptions valid and clearly outlined? Are the results sufficient to support the interpretations and conclusions?

Unfortunately the manuscript **fails to reach substantial conclusions**, as the scientific assumptions used by the authors are not appropriate; they are even wrong (namely the assumptions that volcanic ash layers are always thicker than T_{low}=500 m). Furthermore, the analysis only covers one specific day; therefore the results are not at all sufficient to support the very general conclusions (”..significantly improves the advice given to aviation..”)
In addition, the authors only compare their own model runs (one run without and one run with assimilation). This experiment is not adequate in order to support the conclusion that their method improves the quality of advice given to aviation in Europe. For this conclusion, the authors have to compare their results with a benchmark representing the current state of the art, e.g. the VATDM standard products of the VAACs in London or Toulouse) and their assimilation scheme. Otherwise the author's general conclusion is not justified.
Furthermore, the authors only discuss the rather simple situation that there is one ash layer in the atmosphere. The state of the atmosphere is not always that easy. There are situations with water and ice clouds above/below/within the ash layer, and situations where high clouds move above the ash layer. In such situations the ash layer can't be detected by the satellite algorithms, leading to wrong assimilation input. I expect that in such a case the model run without assimilation will perform much better than the assimilated one. I completely miss the research on and the discussion of effects like these in the manuscript. Reducing the ”effective time duration” from 15 h to 12 h is no adequate approach in order to account for atmospheric variability (page 9, line 16-19).
Also, the authors only discuss on cases where one singular layer of ash is present. However, there are many cases reported in the literature with several isolated volcanic ash layers in the vertical (e.g., Schumann et al., 2011). The authors do not discuss these cases and the implications of multi-layer volcanic ash for their SOO concept.

Is the description of experiments and calculations sufficiently complete and precise to allow their reproduction by fellow scientists (traceability of results)? Do the authors give proper credit to related work and clearly indicate their own new/original contribution? Does the title clearly reflect the contents of the paper?

The title is too general. I would suggest to either add ".. a case study" or to extend the analysis to much more cases if the authors want to keep the generality of their conclusions. The author should also give a better overview on the state of art regarding the assimilation procedures in other VATDMs.

Does the abstract provide a concise and complete summary?

Beside the criticism already mentioned, it is ok.

Is the overall presentation well structured and clear? Is the language fluent and precise?

In general the structure is ok, and the quality of the presentation is good. Some sentences which are not that clear are listed in the "specific comments" section. One shortcoming is that there are several typos which can easily be detected and corrected by any standard word processing software. This is not the job of a reviewer; it should be done by the authors before submission of a manuscript (e.g. "satallite","atmoshere").

Are mathematical formulae, symbols, abbreviations, and units correctly defined and used?

Yes.

Should any parts of the paper (text, formulae, figures, tables) be clarified, reduced, combined, or eliminated?

In general: ok. See specific comments below.

Are the number and quality of references appropriate?

Several references used by the authors are not appropriate and/or do not really fit the statement they should support in the manuscript. For example, the authors report on the economic damages due to closure of airspace, and they cite Bonadonna et al., 2012 in this context. However, Bonadonna et al., 2012 did not at all investigate the economic damage; they just cite a paper on the economic damage. See below for more examples.

Is the amount and quality of supplementary material appropriate?

N/A

3 Specific comments:

Page 1, L 15: Be more precise! EASA (2011) **is not the current regulation**. It is a **recommendation**. Also, the reference is outdated; please use the current version 2010-17-R7 instead of 2010-17-R4. Furthermore, the document does not state "... that the highest concentration an aircraft can endure is 4.0 mg m^{-3}" as claimed in

the manuscript. The reference to Fu et al. (2015) it not useful here in the context of regulation.
Page 1, L18: Prata and Prata (2012) states that eruption phase is **until May 25** 2010. Should be synchronized at least for papers with the same author.
Page 1 L 19: Bonadonna et al., 2012 is not appropriate here, cite original literature dealing with economic issues (I suggest "Oxford Economics, The economic impacts of air travel restrictions due to volcanic ash, report, 12 pp., Abbey House, Oxford, UK (2010)")

Page 2, L2: Reference (Mastin et al., 2009) is not linked to a statement; maybe it should be shifted to the end of the sentence?
Page 2, L 12: Zehner [Ed.](2010): it is not clear which statement should be supported by this reference. Zehner 2010 is a proceeding of a ESA-EUMETSAT workshop (with a plenty of participants. Please specifiy the work you refer in this context. Give either page numbers or (better) use original literature.
Page 2, L 18: I suggest to use the past: "can be performed" -> "were performed", as it better fits to the next sentence reporting on the past events.
Page 2, L18: "including occurrence of the ash plume and the nature": What does "..and the nature" mean? I thing this part of the sentence should be skipped or rephrased.
Page 2, L21: I suggest replacing "not always available" by "usually not available". At least from a global perspective. Maybe you can add the aspect of NRT availability of the data. General availability of airborne measurements, for example, does not necessarily mean that these data are available for NRT data assimilation.
Page 2, L 27: Again, Zehner (2010) is not a good reference here. Specify the algorithms you refer to, or use Prata and Prata, 2012
Page 2, L24: I strongly recommend not to use this sloppy coverage from 70 N – 70 S, 70 W – 70 E, as the visible latitude range depends on the longitude and vice versa. The field of view of a geostationary satellite is fully defined by the longitude of the sub-satellite point.

Page 3, L3: One might add that the maior ”shortcoming” of CALIOP is the low ”temporal resolution” (polar-orbit) and the data processing and delivery is not designed for NRT applications). It is not only the spatial coverage.
Fig 2(a): hard to interpret. What does *”where you are”* mean? Where is the volcanic ash layer? In the satellite, as indicated by the arrow? Why is the sun included in the figure?

Page 3, L7: please specify what you mean here with ”entire volcanic ash plume”. Specify the eruption(s) and the volcano you are talking about here, and the period of time. Marenco et al, (2011) is based on six flights between May 6th and May 22th 2010. Christopher et al. (2012) does not provide additional information on the thickness of ash layers (they just cite Marenco et al., 2011). Please also note that Marenco et al., 2011 reports on **typical ash layer depth**, one number for the whole flight (see description of table 3 in the paper)! This is something like an overall average but does not at all mean that ash layers are in general always thicker than this given value. Also look to the lidar pictures in Fig. 3 of Marenco et al. (2011) which should give you a good impression of the variability of the vertical extent of ash layers. Prata and Prata (2012) concludes that layers *”that ash was horizontally widespread, vertically localized in thin layers **of 200** – 1000 m depth”*. Prata et al.,2015 reports (1) not on the Eyjafjallajökull but the Chaiten eruption, and (2) found values of the vertical extent **clearly below 500 m**.

Page 4, L 14: ”the observed values by SEVIRI”: SEVIRI does not observe ash loadings, SEVIRI is a radiometer. Ash loadings are derived by algorithms applied to SEVIRI data. Please rephrase.
Page 4, L 27: T_low to T_high 0.5 – 3 km should be adapted to a realistic range. I suggest an lower limit of 50 – 100 m.
Page 4, L 5-11: One reference to Marenco et al., 2011 is sufficient; no need to cite it

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three times in five lines.
Page 4, L 5-11: regarding the thickness estimation, see comments above (Page 3, L7)
Page 4, L 15: ”The observed values ... ranged between 0.2 and 5.0 g/m^2”: specify the period in time you are talking about.
Page 4, L 19: Vicente et al, 2002, is dealing with parallax corrections. You are talking about simple geometry. Did you made any parallax correction?
Page 4, L 25: I didn’t find any statement in Prata and Prata (2012) that ash plumes can be considered as box-car distribution functions. In contrast, Prata and Prata (2012) states that *”because of the spatial heterogeneity of the ash, revealed in the SEVIRI retrievals, sole reliance on lidar measurements for monitoring ash clouds and for validating dispersion models may provide misleading conclusions on the concentration and homogeneity of ash in the atmosphere.”* Please clearify.
Page 4, L 29: As discussed above, there is not at all a 100 % certainty that the blue layer is containing VA; T_low = 500 m is not the lowest possible thickness.
At this point I stopped evaluating the manuscript, as all the following steps are based on the assumption T_{low} = 500 m.

4 Technical comments

Page 3, L 14: check spelling of Eyjafjalla (several times, check throughout the text)
Page 3, L 18: ”N” (North) is missing
Page 3, L 30: funding information should be in the acknowledgement Page 3, L 32: ”(The registration is needed)” sounds strange; I suggest (Registration required)

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