Review of

Satellite data assimilation to improve forecasts of volcanic ash concentrations: a case study on the 2010 Eyjafjallajökull volcanic ash plume

by Fu et al.

The paper describes a case study when SEVIRI-derived volcanic ash mass loadings and heights of the top of the volcanic layer are used for model calculations/forecasts of ash concentrations. The SEVIRI output is transformed into concentrations, adapted to the model grid (LOTUS-EUROS) and then used in the framework of an EnSR data assimilation scheme. This approach is applied and discussed for a short period of May 2010 and compared to airborne measurements of 5 hours durations. The authors show that the model results with data assimilation compare considerably better with the measurements than results without assimilation.

The paper provides a contribution to the 'hot topic' of the prediction of volcanic ash dispersion and how airlines (and the responsible authorities) can be advised to avoid critical flight routes. A shortcoming certainly is the limited set of data, so it is not possible to draw conclusions that are generally applicable. Insofar I appreciate that it is now clearly stated (even in the title) that the manuscript covers a case study only. Further studies are definitely necessary.

However, before being published section 3.1 needs to be improved. The description is not as clear as it could/should be and the nomenclature is sort of strange. It must be possible to describe the methodology in an unambiguous way from a set of equations! Furthermore, the conclusions and outlook are too much focussed on passive space borne remote sensing; I am sure that ground based networks (active remote sensing) will contribute significantly to the 'volcanic ash topic' and should be mentioned in this paper when discussing future perspectives.

Comments on Section 3.1

I have not understood the concept of the procedure. Maybe other readers will have similar problems, remember that this topic was also raised by the reviewers of the first submission of the manuscript. I don't state that the approach is

wrong, but the description is confusing and obviously not all required information are given: In particular it is not clear, what is prescribed/fixed and what is variable. So I encourage the authors to rephrase this section in such a way that nothing remains unclear or might be misunderstood, with text/equations/figure being consistent. Expressions as '*extraction layer*' can hardly be associated to an atmospheric feature, so a more intuitive name should be chosen to facilitate the understanding. Clarity of this section is essential as it is one of the most important parts of the paper and covers the novelty of the study.

The following remarks/comments/attempts of interpretation may illustrate my confusion:

From SEVIRI mass loading is derived. This is converted to the vertical column by a simple (but sufficiently accurate) approach using Eq. (1). Thus, the remark on page 2/line 35 ('*the path can be a line or a curve...*') can be omitted, especially as it is more confusing than explaining.

The second parameter derived from SEVIRI is H_{top}. Here, a typical accuracy of this value should be given. Then it is stated that '*the thickness of the plume is investigated*'. What does this mean? '*Investigated*' implies that the thickness is varied and a specific/best value is selected by a clearly defined independent criterion. However, neither a definition and an explanation is given. T_{low} and T_{high}, and the incremental thickness T are prescribed. Thus, all other values (T_i, C_i) are also fixed and depend on the measured ML_{vert} in the same way in all cases. C_i does not depend on H_{top}; that indeed can to be understood.

What is the reason to use different symbols (H and T) for lengths, and not similar symbols for similar quantities; especially T should obviously be something like ΔH. The current nomenclature leads to unnecessary confusion. The same is true for ML_{vert}. This should be M_{vert} or m_{vert}, two characters for one quantity are not common in physics, especially as part of equations (it cannot be distinguished from a product of M and L_{vert})! By the way: to be consistent with a range of possible thicknesses between 0.5 km and 3.0 km, N_s should be 51, not 50.

Eq.(4) assumes that each thickness (0.5 km, 0.55 km, 0.6 km, ...,2.95 km) has

the same probability (cf. line 18 on page 5): is this meant by the authors? Is a consequence of this that the vertical distribution (expressed as C_i) is unchanged during the dispersion of the ash-cloud? A variable thickness of the ash-layer (i.e. realistic conditions) is only possible, if T_{high} (i.e. N_s) is changed. Is this indicated by the authors' remark '*Corresponding to the sampled thickness...*'? However, it remains open what '*the sampled thickness*' is and how it is derived.

The paragraph starting at line 27 (page 5) is also difficult to understand. How can it be inferred from Eq. (3) that the concentration in the 'yellow region' is zero? Where is an information like '*the cloud thickness is 1 km*' derived from? It seems contradictorily if the thickness is stated to be 1 km, but the ash concentration is allocated to a 0.5 km layer ('*between 7.5 km and 8.0 km*'). Fig. 2 suggests that the ash layer is always T_{top}=0.5 km thick.

If parts of the procedure are only used to estimate the 'extraction error', it should be clearly stated here.

I hope that a revision of this section is possible to get a convincing description of the methodology! Maybe it is sufficient to add two 'carefully formulated' sentences, especially on the selection/derivation of the 'thickness of the ash layer', and all problems are solved.

Minor/technical comments

1. page 2, line 8: '*over a long distance*': be more precise and give typical ranges (hundreds of kilometers).

2. page 2, line 27: '*there are 3712 ...*': This information is not relevant, more interesting is the spatial resolution (as given later in the paper).

3. page 2, line 28: '*Nowadays ... as well as the ash cloud top height*': An indication of the accuracy is welcome.

4. page 3, line 10: '*mostly from hundreds...*': please rephrase; the thickness is certainly not hundreds of kilometers.

5. page 3, line 20: '*From a modeling perspective...*': The model used in this study should be introduced here (at least earlier than in the current version of

the manuscript) and the resolution (and other key parameters) should be mentioned here.

6. page 3, line 32: '*Secondly, using the extracted ...*': What is meant by this statement? What kind of in-situ measurements are used, and where are they from?

7. page 4, line 6: '*...geographic area affected by the Eyjafjallajökull volcanic ash*': The ash affected a larger area than shown, e.g. Spain (Navas-Guzman et al., 2013), Greece (Kokkalis et al., 2013) or Romania (Nemuc et al., 2014). There is also an 'European overview' given by Pappalardo et al. (2013). So, the sentence should be more general.

8. page 4, lines 11 and 14: The statements concerning the error can be combined.

9. page 4, line 21: '*...resolution to the VATDM resolution*': Give numbers for illustration.

10. page 4, line 23: '*However, the correction cannot...*': What is meant with correction; be more precise.

11. page 4, line 24: '*...due to the insufficient vertical resolution in satellite data*': This only holds for passive remote sensing. In case of active remote sensing the contrary is true. A comment on this would be welcome.

12. page 6, line 3: It would be appreciated if another name for the '*extraction error*' is applied. This name does not support the understanding.

13. page 10: In the conclusions two very general comments on future needs/activities are made: '*This effective time period probably...* ' and '*How to determine the reasonable ...*'. This should be improved by more specific comments. To my knowledge a large number of/most(?) national weather services recently have implemented ceilometer networks, mainly for monitoring the dispersion of volcanic ash clouds (Weigner at al., 2014). These data set will be (and in part are already) available in near real time and

will provide excellent information about the (horizontal and) vertical distribution (with some restrictions due to cloud cover; but this is also true for space borne observations). It seems to me that they could be very promising candidates for data assimilation as well, and can complement satellite data.

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