

# ***Interactive comment on “Improving volcanic ash predictions with the HYSPLIT dispersion model by assimilating MODIS satellite retrievals” by Tianfeng Chai et al.***

## **Anonymous Referee #2**

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Review of ‘Improving volcanic ash predictions with the HYSPLIT dispersion model by assimilating MODIS satellite retrievals’ by Chai et al

### General comments:

The paper presents an inversion method for diagnosing emission rates for volcanic eruptions, applies it to the 2008 Kasatochi eruption, and conducts a range of sensitivity tests to assess various modifications to the approach. This adds value to previous similar studies through testing a variety of plausible approaches and by applying the method to a volcanic eruption that has not been studied in this way before. The latter aspect is especially welcome as previous studies have only used a very small number of eruptions and it is unclear how widely applicable the conclusions are. The paper is

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suitable for publication as a discussion in ACP both in terms of scope and in terms of scientific soundness.

Specific comments:

1) It would be nice to have a little more discussion about the meaning and limitations of satellite derived ash cloud top. In many retrieval systems, for optically thin clouds, this may be more like the mean ash cloud height.

2) Page 5, lines 11-13: I guess the significance of the different sizes is that the particles have a fall speed – it would be good to clarify if that is correct. Also, while the size distribution chosen seems very sensible, it would be good to say what the basis of the distribution is, e.g. perhaps it's based on some particular measurements. If it's just expert judgement, that's fine.

3) The convention that cloud top height is infinite when there is no ash cloud (p 6, line 8-9 and p 13, line 31) seems strange. If one thinks of it as the height above which there is no ash, then zero seems more appropriate than infinity. In any case I think the convention is not needed in the paper – would anything change if infinity was replaced by zero? If not it would be simpler to just talk about no ash regions and not mention a cloud top height for such regions.

4) I think that, if the zero ash observed values are not used (i.e. from ash free regions or values above and below the ash cloud), emissions which don't contribute to the chosen model diagnostics because they are much higher than the observed ash top, are not constrained by the observations. These emissions will then be set to the *a priori* values. This only works because the *a priori* is chosen to be small. Assuming this is correct, it would be good to explain this.

5) Assuming a single model layer for the model diagnostic and imposing zero values above and below this layer will clearly give results that are sensitive to errors in the observed ash cloud top. E.g. if the top is in error and the winds at the true and

observed heights are in different directions, the method will not work very well (as is seen). I think it would be useful (but not essential) to give more discussion of these sorts of aspects rather than just presenting the results and noting which methods work best.

6) Page 14, line 27: The idea of a cylindrical source is interesting, but readers won't be able to assess this without a little more information about the Kasatochi eruption. In particular, was there a significant umbrella cloud generated by the eruption? Probably this is discussed by Crawford et al (2016), but a few extra words would help the reader.

Technical corrections:

These are mainly requests for clarification or minor corrections.

7) Some of the options are not easy to understand from the presentation in the abstract (lines 10-17). This may be inevitable to some extent given the space restrictions, but it would be nice to give a little more information. For example I think the 'three options' are not really options for the matching method but for the choice of model diagnostic, so that, in the 'integrating over three layers' option, the model result over three model layers is compared with the satellite total column – there's no attempt to retrieve column load over just three model layers from the satellite. Also when using the three model layers option and enforcing no ash above/below the observed ash top, I assume that this is not enforced in the top/bottom of the three layers, so that 'above/below the cloud' is interpreted in relation to the chosen model cloud diagnostic. These aspects are clearer on page 6, but the last aspect is still not completely unambiguous.

8) Identifying 'no ash detected' with 'ash free' (p 3, lines 22-24 and p 6, line 8) is explained later as being applicable to Kasatochi where there is little meteorological cloud (p 6, lines 21-24), but is not necessarily applicable in general. It's worth considering if something can be said earlier to avoid readers thinking that the authors have made an incorrect identification.

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9) Page 7, lines 22-24: It sounds as though these zero values are used in all the inversions, but in fact this is only true in some of the approaches used. Might also be worth clarifying on p 9 whether the zero values are used in fig 3 and 4 (and also in section 4.2). It becomes clear in section 4.3 that the zeros are considered in 4.3 and hence weren't included before, but this could be made clear earlier.

10) Page 7, line 30: I guess the approach used and the alternative described are equivalent, in that e.g.  $q_{ij} - q_{ij}^b$  in (1) is replaced by  $\exp(l_{ij}) - q_{ij}^b$  with  $l = \log q$  and with  $l_{ij}$  being adjusted, so the same quantity is being minimised (rather than replacing  $q_{ij} - q_{ij}^b$  in (1) by  $l_{ij} - \log(q_{ij}^b)$ ). Could avoid any doubt here by saying that the alternative method is equivalent or should give the same answer or is an alternative way to solve the same mathematical problem.

11) It might be clearer to emphasise a bit more that ash cloud top usually means observed ash cloud top (and not the unknown true ash cloud top or a model value). E.g. fig 2 caption and page 9, line 24.

12) Page 11, line 15: I don't think it is correct to say that the authors find the method which *improved* the forecasts the most, since they don't compare with an approach that doesn't use any inversion modelling. Instead they just find out which method is best.

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