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Atmospheric Chemistry and Physics
Discussions

Interactive comment on "Uncertainty assessment and applicability of an inversion method for volcanic ash forecasting" *by* Birthe Marie Steensen et al.

Anonymous Referee #2

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General comments:

This paper concerns the forecasting of volcanic ash plumes, and presents a case study of the 2010 Eyjafjallajökull eruption. The main issue dealt with in the paper is the optimal estimation of the "source term", that is, an estimate of the injection of ash as a function of height and time, based on a combination of a priori information, an ash transport model (fed by meteorological reanalysis data) and satellite retrievals of the ash plume. While other papers have pioneered the data assimilation technique used to estimate the ash emission, this work focuses on a forecasting scenario, where the assimilation is used as an initialization for an ash transport forecast simulation. The inversion technique which is used to estimate the volcanic emissions relies not just on

the a priori information and satellite retrievals, but also on estimates of the uncertainty in these values: in general, any optimal estimation or assimilation technique weights different sources of information based on their relative uncertainties. The major focus of the study is on the impact of different uncertainties on the source term estimation, and on the results of the forecasts.

The subject of this work is appropriate for ACP. In general, the organization, structure, and writing could be improved to help the reader understand the major results and conclusions of the work. References to prior works appear to be appropriate, and the methods are described enough that it is possible to follow the logic of the numerical experiments. The conclusions are generally supported by the results (some comments below), and the study appears to be a tangible step towards robust volcanic ash evolution prediction systems.

One of the major results of the paper appears to be that uncertainty in the width of the log-normal ash size distribution assumed in the satellite retrievals is the major uncertainty in the source term estimation technique–at least that this uncertainty is much more important than so called "other-than-size" uncertainties in the retrieval. This is presented as a rather general conclusion. I see two problems with this conclusion.

Firstly, and generally, it is hard to make concrete conclusions about the impact of different uncertainties when the ranges of uncertainties used in the study for the different parameters are rather arbitrarily, and not uniformly sampled. Uncertainties in "other-than-size" parameters in the retrievals are sampled at 0-200%, while the uncertainty in the a priori is sampled from 25-100%. Ash size distribution widths are sampled at a set of discrete values, and it's not clear how any of these ranges compare to the fundamental uncertainties in these physical parameters. It is therefore hard, or impossible, to draw conclusions about the relative importance of different uncertainties in the results of the inversion.

Secondly, it is hard to believe that changing the uncertainties in the retrieved ash mass (the "other-than-size" satellite parameters) from 0 to 200% (Pg 7, line 1-2) will have such small impact on the estimated source term (as in table 1). The authors note that part of the explanation for this may be because the a priori used in the emission estimation is rather large compared to all the a posteriori results. So, if the uncertainty used for the a priori is too small, then the assimilation might be too strongly constrained by the a priori, and the satellite observations have a similar impact on the result no matter the uncertainty assigned to them. If this is the case, then it is a result which is very specific to this case–the opposite result might occur if the a priori is very realistic. This point needs to be very carefully considered in the abstract, results, and conclusions. It would actually be extremely useful to repeat the analysis using the 0.1 fine ash fraction, which would apparently greatly improve the accuracy of the a priori.

The text is often hard to understand, partly because many different names are used interchangeably for the same things. For example, the ash emissions as a function of height and time which are estimated by the "inversion technique" are referred to as the "source term", the "source emission term", the "source estimate", the "emission estimate" and so on. If the same thing is referred to in each case, then the same name should be used.

Specific comments

Pg 1, l7: Data assimilation techniques are not a development in and of themselves, but the application or use of them in this field may be.

Pg 1, l7: The aim or advantage of data assimilation is a little more subtle than just bringing model results in close agreement with observations. When used properly, data assimilation results provide more value than the model results or observations individually.

Pg 1, l19: Varying assumptions in the satellite retrieval only translates to uncertainties in the estimated emissions if those variations correspond to actual uncertainties in the satellite retrieval, ideally quantified in some systematic way.

Pg 1, l20: It's not clear what "weighting of uncertainties" means here. Uncertainties are used by data assimilation to weight the relative contributions of different sources of information, but the uncertainties are not themselves weighted.

Pg 2, l6: It's not clear how the use of the a posteriori emissions reduces uncertainties connected to the satellite observations.

Pg 2, l7: isn't the forecast of real interest that of the ash transport? Forecasting the ash emission seems like a very different, and difficult problem.

Pg 2, l14: "source term" should be well defined in its first use.

Pg 2, l26: It wasn't clear to me at first that the "zero" and "constant" a priori estimates were two different things. Also, this result likely depends on the uncertainty assigned to the a priori, and the size of the assumed "constant" a priori.

Pg 3, l1: this sentence seems to contradict the previous sentence.

Pg 3, l15: It should be made clear that this list is not exhaustive, certainly one could use other estimates in the forecast, including an upper limit.

Pg 3, l17: It's not really clear to me why or how using the average of the past few hours in the forecast "limits the uncertainty" of the emissions: this implicitly assumes that the volcanic emissions are most likely to persist at a relatively constant rate. If this can really be shown to be the best assumption, then it would be an interesting result.

Pg 4, l15: Is there a justification for this threshold?

Pg 5, l32: Are the satellite measurements hourly means of many satellite measurements, or single "snapshots"?

Pg 7, l1-2: If it is true that the so called "other-than-size" uncertainty is simply an uncertainty associated with the retrieved ash mass, it would be clearer to refer to it as so ,i.e., "mass loading uncertainty" or so. I see that the assumed size distribution also impacts the mass loading, and so there is an argument that the term "mass loading uncertainty” may not be exactly unique, but if it more clearly describes what it actually is, then I think the process will be easier for the reader to understand.

Pg 7, l27: This is confusing, if the satellite observations are used as the observation field, won’t this analysis compare the model forecast with the satellite observations? And if so, isn’t the answer obvious, that is when the uncertainty assumed for the satellite observations is relatively small compared to the other uncertainties, then the assimilation will put more weight on the satellite obs and produce a closer agreement?

Pg 9, l18: In Figure 5b, it’s not clear which color refers to which a priori uncertainty value.

Pg 9, l19: Does this last sentence of the paragraph refer to Fig 5a or 5b? And does the spread produced by using different size assumptions in the satellite retrieval represent the full a priori uncertainty, or could other sources of uncertainty also affect it?

pg 10, l7: I don’t think that using a range of uncertainty values in the operation forecasting is ideal: in fact, the ideal forecasting should use the most accurate estimates of the real uncertainties as possible: the result of the forecast should then produce the most accurate result.

Pg 10, l13: What does “typical” mean in this case, is it similar to the overall ensemble mean?

Pg 12, l14: The term “optimized field” is not easy to understand, is this simply the forecast model result?

Pg 16, l15: This wasn’t really an operational forecast setting, more a kind of hindcast scenario.

Pg 17, l21: I don’t think the paper really shows that the use of the emission inversion technique produces improved confidence in the satellite data.

Editorial comments

Pg 1, l11: exploits->explores

Pg 1, l13: it may couple measurements from a satellite instrument, but not the satellite itself.

Pg 2, l10: should probably be clear you refer to airplane windshields.

Pg 2, l12: current->instantaneous

Pg 2, l21: suggest: “weight their relative contributions to the inversion results”

Pg 2, l32: A more accurate emission term is. . .

Pg 4, l15: “Emitted” from an emission? Perhaps “resulting from a unit ash emission” is closer to the mark?

Pg 5, l32: I’m not sure what “forward mean” means.

Pg 7, l13: 0.4 is

Pg 9, l28: “trough”?

Pg 9, l 32: “left-most”

Pg 11, l23: the change in a posteriori emissions. . . is similar. . .

Pg 11, l24: April and May periods

Pg 16, l15: The start of the conclusions shouldn’t reference “the inversion method”âĂŤa reader might not have necessarily read the preceding sections in detail.

Pg 16, l20: The observed ash cloud. . . is shown to be difficult. . .

Pg 16, l25: The retrieval does not really “decide”, maybe “distinguish” is a better word choice.

Pg 17, l11: I don’t think the “times” are reduced.

Pg 17, l17: . . .the model. . . has more ash (although, the model doesn’t really “have” anything).

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