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Atmospheric Measurement Techniques Discussions

Interactive comment on "Impact of meteorological clouds on satellite detection and retrieval of volcanic ash during the Eyjafjallajökull 2010 and Grímsvötn 2011 eruptions: a modelling study" *by* A. Kylling et al.

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Response to interactive comments from F. Prata

F. Prata claims that "This paper suffers some serious flaws that make the results difficult to interpret." We believe F. Prata's comments are based on a general misunderstanding about the scope and purpose of our study. This is reflected in our responses to his comments below.

F. Prata next states that "The problem of simulating clouds in satellite imagery is non-trivial and while certainly worthwhile some quantitative assessment of the simulation is needed if the results are to be believed.". We certainly agree that the simulation of clouds in satellite imagery is non-trivial. However, this study is a model sensitivity study of the effect of meteorological clouds on detection and retrieval of volcanic ash. As such no particular measurement data set is needed. We have chosen to perform this model study using model data for the Eyjafjallajökull 2010 and Grímsvötn 2011 eruptions, both because model data were available for these eruptions and that they have recently received much attention.

F. Prata further claims that "It follows then that assessing the impact of these simulated clouds on ash retrieval is at best only qualitative and at worst meaningless. The authors actually admit this in their paper." We certainly do not admit this in our paper. On the contrary model studies have been used over and over again in a number of studies to meaningfully assess the impact of various processes that affect a given measurement. In our study we assess the impact of ice and liquid water cloud on volcanic ash detection and retrieval by quantitatively comparing simulations with and without clouds.

Specific comments

Specific comments from F. Prata to our manuscript are quoted below in italic font. Our responses to the comments are shown in roman font.

1. *The analysis for Eyjafjallajökull is compromised by the fact that the FLEXPART simulations utilised SEVIRI data in the first place. Although this is not entirely clear in the paper, the authors refer to Stohl et al. (2011) as the source for the simulations. I was a co-author on that paper and provided the SEVIRI retrievals for the inversion. This means that the FLEXPART simulations have already been*

influenced by the SEVIRI observations. The authors mention the fact that only the Grímsvötn analysis is "fully independent", but in so doing they are acknowledging the problem with the Eyjafjallajökull analysis. What is the impact of this? A better approach would have been to use an independent data set, for example MODIS. As these data are at âĹij1 km2 resolution, the effects of sub-pixel cloud could also be examined (see also point 5).

This study is a model sensitivity study, which does not rely on the direct use of any particular measurement data set. The main results of the paper are based on comparison of results from simulations with ice and liquid water cloud and simulations without such clouds. As such, it is irrelevant for our study whether the FLEXPART simulations have already been influenced by SEVIRI observation data, or not. The SEVIRI measurements presented in the study are only used to demonstrate the realisticness of the simulations.

2. *The methodology essentially uses two models: a FLEXPART simulation of ash followed by a radiative transfer calculation. The problem they are addressing is complex and, in their own words "A thorough and complete comparison of the SEVIRI simulated ash retrieval and the SEVIRI measured ash retrieval for the Eyjafjallajökull 2010 and Grímsvötn 2011 eruptions is beyond the scope of this work." But a thorough comparison between model and observation is exactly what is required. Shouldn't such a study be done first, thereby making this analysis more credible?*

   A comparison of models and observations (i.e., a model evaluation study) is something completely different from the model sensitivity study presented here. We do not quite understand why F. Prata advertises performing a model evaluation study while discrediting the value of a model sensitivity study. There is value in both types of studies, but the things that can be learned from these studies are totally different from each other. For instance, it is quite clear that the effect of clouds can never be looked at in isolation in a model evaluation study, since there

are many other factors influencing both the model and the observations, and also because many factors of cloud distribution, liquid water content, etc., are highly uncertain in the observation data.

3. *A major part of the analysis is missing: simulating meteorological clouds is at least as demanding as simulating ash. There are as many disagreements as agreements between the simulated imagery and the SEVIRI imagery in the Figures provided. At best, the comparison is qualitative; otherwise one would expect the authors to have presented some statistical data to support their analyses. (It is not sufficient, in my view, to refer to a previous paper, as each meteorological situation is different; the authors acknowledge this). How many times does the simulated imagery show cloud, when SEVIRI imagery also shows cloud? Clearly this is demanding but must be done if the results are to have any meaning.*

   The ice and liquid water cloud data are from ECMWF. Other version of these data are used as basis for weather forecast by numerous meteorological stations worldwide. Clearly the ECMWF gets their clouds right a large portion of the time. Otherwise the weather forecasts would be useless. Comparing simulated and measured SEVIRI images for clouds would be interesting, but the ECMWF data contain SEVIRI information in the first place as SEVIRI (and IASI) information is assimilated into the ECMWF data. Such a comparsion would thus be between two dependent data sets. Furthermore, as mentioned previously, the purpose of this study is by no means getting best agreement with the observations, or even comparing observed and simulated distributions of clouds or ash. There are numerous studies on this already, so this would not be of great value anyway.

4. *I have examined one of the cases the authors provide in some detail. It is very clear from the different satellite measurements that the FLEXPART simulation is quite poor in some places and quite good in other locations. For example, along the west coast of Norway (Figure 1), none of the satellite measurements support the observation of ash there. For this case, there are certainly sub-pixel*

effects at the resolutions the authors deal with. The "hole" in central southern Scandinavia looks suspicious and by examining high resolution (250 m) MODIS data it seems that there is surface snow/ice and low cloud over the region. There are places where ash and cloud co-exist and places where cloud is overlying ash (and possibly vice-versa). The radiative effects are different in these cases, but no attempt is made to address this. The SEVIRI pixels are large; such that many of the pixels contain ash/meteorological cloud/clear areas, in different proportions. Such an effect overwhelms the dispersion model to RT model cloud comparison presented here.

The case mentioned is presented in Fig. 3 of the manuscript and the radiative effects of clouds are addressed in the text accompanying the Figure. As is clearly shown in Fig. 3, the introduction of clouds reduces the number of pixels identified as ash (compare number of green pixels in middle and left plot, Fig. 3). Qualitatively the cloudy simulation agree better with the SEVIRI detected ash (left plot, Fig. 7, revised manuscript). However, we do not do any quantitative comparisons between the model simulations and the SEVIRI measurements as that is outside the scope of the paper and the logic would also be circular. The mentioned "hole" in Scandinavia (mostly the Jotunheimen area) is a high mountain area in Norway and is usually snow covered in April. That the satellite does not detect any ash does not outrule the presence of ash.

5. *The difference in spatial resolutions of satellite sensors (they also use IASI as well as SEVIRI) coupled with the larger grid size of the model is important. Sub-pixel cloud is a very important effect and its consequences are not discussed. I suspect by far the largest source of discrepancy is due to "mixels", pixels that contain more than one component (e.g. ash and cloud, or ash and clear areas, or all three). Since the SEVIRI data (3 km x 3 km at best), the FLEXPART model (25 km x 25 km) and the RT model (28 km x 16 km) all employ different spatial resolutions the authors should quantify the effects. It is not clear from this paper*

whether sub-pixel cloud is properly accounted for since the scales utilised are a bit large.

The Flexpart data is not on a 25 km x 25 km grid but on a $0.25^\circ \times 0.25^\circ$ grid as mentioned in the manuscript. A $0.25^\circ \times 0.25^\circ$ grid roughly corresponds to a 28 km x 16 km resolution for the latitudes covered by the simulations. We are not quantitatively comparing SEVIRI and model simulations. We are comparing model simulations with and without meteorological clouds which both have the same spatial resolution. Sub-pixel variability is of course an outstanding research topic in itself, but as our model results all have the same horizontal resolution this is not an issue here. Still, F. Prata makes an important point here that is worth studying, but which would require a different approach, i.e., the use of high-resolution (basically, eddy-resolving) models that can separate ash and cloud at high resolution. This could then be compared with a case where the resolution is artificially degraded. But it is clear that this is totally out of the scope of the present paper.

6. *At least three assumptions in the RT model are made without justification and will lead to erroneous results. These are: (1) no water vapour correction for the retrieval, (2) a constant water vapour field for the whole domain, and (3) ash clouds with a uniform thickness of 1 km. A change from 1 km thickness to 2 km thickness increases the mass loading by 100%, for the same ash concentration. Thicknesses of 3-4 km were observed for Eyjafjallajökull. For assumption (1), the original SEVIRI retrievals (Stohl et al., 2011) were corrected for water vapour. It is not clear what the impact of this is over the entire data set, but their subsequent use is inconsistent. As a guide, the water vapour corrections used for Stohl et al. (2011) were of the order 0.2 to 1.0 K on the brightness temperature differences. Such a correction can massively affect the ash mass retrieval. As for a constant water vapour field, this is clearly not justified. There will be large spatial variations in the IR brightness temperatures due to water vapour, without any clouds or ash.*

The assumption may be justified in the visible part of the spectrum, but it is not justified in the infrared region.

(1) As stated in the manuscript no water vapour correction was made. This applies to both the cloudy and the cloudless simulations. The main findings of the manuscript are based on differences between the cloudy and the cloudless simulations, which are similarily (not equally) affected by the lack of water vapour correction. We do not anticipate that the lack of water vapour correction does have any major impact on the results presented. It is also noted that the magnitude of the water vapour effect is small at high latitude due to the lower water vapour amounts compared with lower latitudes. Again, the comment of F. Prata is motivated by a misunderstanding of the purpose and scope of a model sensitivity study versus a model evaluation study. For the latter type of study, the water vapour correction would of course be much more important!

(2) As stated in the manuscript the use of a constant water vapour profile is due to a limitation of the radiative transfer model used. This has been further discussed in the revised manuscript.

(3) The vertical distribution of the Flexpart ash clouds, used to simulate the images, is as in the Flexpart output, that is variable for each simulated pixel. The Flexpart ash clouds input to the radiative transfer models thus have variable vertical thickness. For the retrieval, however, a 1 km thick ash cloud is assumed which is not uncommon. This is all explained in the manuscript.

7. *There are also some errors in the m/s. The statement concerning the effect of water vapour on the brightness temperature difference is ambiguous. The statement that the standard deviation of the simulated brightness temperature is "slightly" greater than the actual NEDT of the channels, when in fact it is a factor 2 times greater needs correction. NEDT is a fundamental measure of instrumental performance so a factor of 2 is highly significant. A better primary*

source of reference for NEDT (rather than Wikipedia) would be one of a number of scholarly published works on the subject, e.g. Rogalski (2011; pg. 667).

The discussion about the water vapour effect has been rewritten in response to comments from the referees. The root mean square of the standard deviation of the Monte Carlo statistical noise of the simulations (=0.15 K for both channels), is a better number to be compared to the NEΔT. For the simulations this number is of similar magnitude as the actual SEVIRI NEΔT (0.11 and 0.15 K for the 10.8 and 12.0 μm channels respectively). The manuscript has been changed accordingly. We have removed the footnote.

8. *They conclude that their results really only apply to these two events and the meteorological conditions for them. Since the results appear flawed for one of the events and there are some serious problems with the RT implementation (constant water vapour field, no water vapour correction, uniform 1 km thickness ash cloud) can we learn anything from this modelling study? The message from this paper is that clouds can confound ash detection–but this is already well known.*

In summary, the motivation for this work is certainly good but the implementation is less so. Clouds in satellite imagery have been studied for many years and there is a large body of work dating back 30 years describing methods on how they can be detected (imperfectly) in satellite imagery. Thus I would not agree that this methodology is any better than an experimental approach that uses state-of-the-science cloud detection methods to identify cloudy and cloudless pixels in real observations. It would be better, in my view, to start by assessing how well the simulated cloudy imagery performs against standard cloud tests in a quantitative way (using actual observations). Once these are quantified, a study like this one should follow, but with some consideration of the independence of the simulations from the data and a more considered approach to the RT modelling.

Stohl et al. (2011) and this paper make a conclusion that is sensible: the use of model simulations with properly characterised observations provides a better *description of ash cloud detection and transport than either of these alone.*

We do not agree that the results are flawed for one of the events as argued above in response to comment 1. The rest of the comment reflects a different approach, and is of course as valid as the one adopted here, to study clouds and their effects.

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