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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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This paper suffers some serious flaws that make the results difficult to interpret. 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. This is missing in this paper. 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. The following observations make me

quite sceptical about the results presented here.

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 $\sim$1 km2 resolution, the effects of sub-pixel cloud could also be examined (see also point 5).

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?

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.

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.

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.

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.

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).

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.

Reference

Rogalski, A., 2011, Infrared Detectors, CRC Press, Taylor & Francis Group 6000 Broken Sound Parkway NW, Suite 300, ISBN: 978-1-4200-7671-4, 876pp.

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