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Interactive Comment

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 Referee #2

We thank the referee for the careful reading of and constructive comments to our manuscript. The referee's comments are repeated below in italic font. Our responses to the comments are shown in roman font.

I assume the authors are using Channels 6 and 7 (shown in eg http://nwpsaf.eu/downloads/rtcoef_rttov11/ir_srf/rtcoef_msg_3_seviri-ir_ysrf.html); one sees the response of Ch 6 begins to tail off in the 1000 cm^{-1} *region, where dust absorption is close to its peak. This must affect the threshold detection of dust by SEVIRI, compared to the higher spectral resolution sounders, leading to the smaller (*$\simeq$ *20%) rates under low-ash loading.*

We have not compared the ash detection sensitivity of SEVIRI and higher spectral resolution sounders. The various ash types and dust have different refractive indices that may peak at different wavelengths. Hence, it may be difficult to come up with a single number for detection rates of SEVIRI compared with high spectral resolution sounders.

Presently many dust detection/volcanic ash detections and retrievals with hyperspectral sounders assume dust filling the FOV; this current paper goes some way to exposing the realistic problems of the dual presence of ash and clouds, though the hyperspectral sounders have more channels and hence are more effectively able to mitigate dust/ash and clouds.

The higher spectral information content in hyperspectral sounders in principle makes it easier to distinguish between ash, dust, and water and ice clouds. However, the footprint of the hyperspectral sounders is typically larger than for band instruments like SEVIRI and MODIS. The larger footprint increases the likelihood of pixels containing a mixture of ash and water/ice clouds.

As such, the paper can be shortened and a few figures removed. For example Figure 15 could be removed; in its place a table summarizing eg the detection efficiency could be inserted. In addition, the authors should remind the reader that their studies are equally applicable to atmospheric dust.

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The manuscript has been shortened in some places as response to specific comments from the other referees. We prefer to keep Fig. 15 as it is discussed in the manuscript, and the discussion is different from that pertaining to Fig. 10. As stated in the Conclusions, the manuscript discuss the situation during the Eyjafjallajökull 2010 and Grímsvötn 2011 eruptions. We hesitate to generalise these results to include dust, although we obviously are aware of the commonalities.

Comments

1. *Abstract, could be shortened.*

   The Abstract has been shortened considerably.

2. *Pg 11306, line 11 : Not sure what the authors mean by "experimental methods"; are they referring to having an ice/water cloud together with suspended ash particles, in a laboratory situation? Perhaps they should re-phrase this as "realistic atmospheric conditions"*

   We have re-phrased this sentence and it now reads: "Both experimental and model based investigations are possible, however experimental approaches are difficult due to the inherent problem in distinguishing cloudy and cloudless cases under realistic atmospheric conditions."

3. *Pg 11306, line 15 and Pg 11307, line 27 : there is not much scattering in the thermal IR. Can the authors explain why they needed to do a 3d radiative transfer simulation?*

   It has been shown by Kylling et al. (2013) that brigthness temperatures may be both over- and underestimated by 1-D radiative transfer models compared 3-D

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calculations. This is due to cloud shadow effects. A sentence explaining this has been added to Section 2 of the manuscript.

4. *Pg 11308, line 15-20 : Could the authors relate dust loading (eg 0.2 g/m2) to aerosol optical depth (eg 1 g/m2 = x OD at 0.55 um)*

   The aerosol optical depth depends on the aerosol chemical composition (refractive index), particle size distribution and particle shape in addition to the aerosol loading. We assume that the ash particles are made of andesite and that they are spherical. However, the aerosol size distribution from Flexpart varies from voxel to voxel. As such, no simple relationship is readily available between aerosol loading and optical depth.

5. *Pg 11309, line 12 : The volcanic events were in April/May so the WV profile should be average between subarctic summer and winter? Actually it is not obvious why could the authors not use the ECMWF WV model fields?*

   We would have preferred to use the ECMWF water vapour fields. However, as stated in lines 11-12 the current version of the MYSTIC radiative transfer model does not allow 3D-fields of trace gases. The effect of adopting a single water vapour profile is discussed in the Discussion section: "Since we are mostly interested in the difference in ash detection and retrieval between the cloudless and cloudy simulated scenes, which are similarly affected by the assumption of a constant water vapour profile, it is not anticipated that a constant water vapour profile will affect the results presented." We have re-written lines 11-12 slightly to make this clearer.

6. *Pg 11309, line 21 : Not sure I understand why there is such a small standard deviation of 0.25 K; looks like your simulations captured the dynamic range of 220 K (when clouds present) to 290 K (low dust/cloud loading). I believe you mean the std.dev of the BT10.8 - BT12.0 simulations?*

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The MYSTIC 3-D model uses the Monte Carlo method for radiative transfer. Thus, all simulated quantities (brightness temperatures for individual pixels) have a statistical uncertainty. It is the standard deviation of the simulated quantities that is quoted. The sentence has been re-written to clarify this.

7. *Pg 11311 : The OEM retrieval description is adequate; however it could be supplemented with a simple RT equation? Since the authors only parametrize BT10.8, BT12.0 in terms of surface temperature, ash/cloud amount and ash/cloud height, are they effectively assuming eg*

$$R = \varepsilon R_0 e^{-\tau/\sec(\theta)} + B(ash)(1 - e^{-\tau/\sec(\theta)}) \tag{1}$$

Else there are slight lower altitude emission effects, especially at 12 μm (since there is WV continuum absorption, though one could argue this is negligible at low wv amount of April/May in Europe); in addition there are competing cloud effects as well.

To solve the radiative transfer equation for the retrieval, the DISORT solver of Stamnes et al. (1988) was used. We have added the this information to the paragraph.

8. *Pg 11312, line 26 : in addition there are time-mismatch between the ECMWF model fields and SEVIRI observations.*

Phrase added as suggested.

9. *Pg 11315, Line 5 : diurnal variation due to surface effects?*

As explained lower down on page 11315 we attribute the diurnal changes to diurnal changes in the surface temperature.

10. *Pg 11315 : Possibly the broadband SEVIRI detectors mean only "higher" ash loading can be detected. Again, a conversion from ash loading → OD would be helpful.*

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Please see response to comment 4 above.

11. *Pg 11316, Line 1 : As mentioned earlier, using subarctic summer WV profile for April/May simulations maybe incorrect, though you do dis- cuss this on pg 11320, Lines 20-28. Perhaps rearrange this discussion so it comes earlier?*

    We have added a sentence informing the reader that this will be further discussed in the Discussions section.

Interactive comment on Atmos. Meas. Tech. Discuss., 7, 11303, 2014.