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Atmospheric Measurement Techniques Discussions

Interactive comment on "Retrieval of ash properties from IASI measurements" *by* Lucy J. Ventress et al.

Anonymous Referee #1

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General comments:

The authors present an interesting piece of work towards a better characterization of volcanic ash plumes from space. They use hyperspectral IASI measurements in the context of an optimal estimation scheme for assigning the most probable solution in terms of AOD, effective radius and ash layer altitude. Unfortunately the paper lacks a very important piece of information, which is essential to judge the quality and also the novelty of the presented approach. I was not able to find information about the methodology used by the authors to derive the optical properties of the ash, which are essential input to the radiative transfer calculations. The authors give hints that they assume spherical particles and mono-modal lognormal distributions. So I assume the optical properties have been calculated using Mie theory? With which code (there can be large differences!)? Which refractive indices have been used - and how have they

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Discussion paper

been spectrally interpolated (as there are not many refractive indices at the spectral resolution of IASI)? What parameters for the lognormal distributions have been assumed to end up with the presented effective radii? Without this information it is hard to understand what exactly are the capabilities of the method and which uncertainties may be hidden in the input assumptions. Consequently the manuscript requires major revisions before being considered for publication in AMT.

Specific comments:

p.1 l. 2: When the authors write "such as IASI", does that mean that the method as it is can be applied to AIRS or VIIRS? In that case not all IASI bands would be used (as AIRS and VIIRS lack significant parts of the specified range) and a description of the used IASI channels would be missing. Otherwise please make clear that the method has been developped for the use with IASI only.

p.1 l. 13: Please introduce abbreviations the first time they occur (RMS). Moreover, "RMS" is rather unspecific. What exactly is meant? Root Mean Square Difference (RMSD)? Root Mean Square Error (RMSE)?

p.1 l. 14: Please specify what exactly is meant by "low optical depth". AOD<0.1? AOD<0.5?

p.3 l.1: Typically the aerosol which can be detected by IASI is dust, not sand. "Sand" denotes particles >63μm, which are bound to the lowest part of the boundary layer due to the strong gravitational forces (unless in heavy sand storms, where turbulent forces can uplift even sand particles to larger altitudes - but they deposit rather fast after ceasing of the turbulent motions).

p.3 l.1: "IASI level 1c radiance data"

p. 4 l. 4: Is there any indication of the assumed orthoginality between ash signal and other signals in the spectra from theoretical considerations? If they are not orthogonal, the basic assumption behind the presented approach is at risk, so this orthogonality should be somehow derived or at least be motivated in a convincing way (this does not mean that I do not believe in that orthogonality!).

p. 4 l. 7: I would like to see a small discussion about the assumed distribution of the brightness temperatures and especially about the assumed distribution of their uncertainties. I guess the authors assume the uncertainties being distributed normally, otherwise one could raise severe concerns about the validity of eq. (3). Such an assumption should be clearly stated.

p. 4 l. 15: "With no volcanic ash signal" or "with no volcanic ash detection"? That is significantly different!

p. 6 l. 8: Do the authors think there is sufficient information about ash type (what exactly do the authors mean by that word?) from below the O3 attenuation? I have some doubts...

p. 6. l. 8: What about SO2? Are there any (important) SO2 absorption bands within the selected wavenumber range?

p. 7 l. 15: There are a lot of other parameters to be assumed in order to derive the mass loading. First of all the assumed particle sphericity is a strong and definitely wrong assumption. That could be overcome by assuming an asphericity factor, which would impact on the volume estimation from the effective radius. With that regard - for nonspherical particles it must be defined if the effective radius is cross-section equivalent or volume equivalent, which can be totally different numbers. Then, in order to get to an estimate for the mass loading, the extinction efficiency needs to be known (estimated or assumed). For volcanic ash particles in the presented size range that is definitely not 2.0 ...

p. 9 l. 3: "Degrees of Freedom for Signal (DFS)" - There also exist a lot of other definitions and concepts of degrees of freedom.

p. 9 l. 6: I would suggest to shortly explain the concept of DFS for the readers not familiar with it. Especially what we can learn from the numbers (by the way: why do the authors not show the DFS in Fig. 5?).

p. 9 l. 10: "Interestingly, and perhaps unexpectedly, the surface temperature uncertainty improves at the highest altitudes." To be honest, I do not understand this sentence. What exactly is at highest altitudes? The ash layer? I do not assume that surface temperature is at different altitudes? So please reformulate this sentence.

p. 9 l. 15: This is well known for quite a while now (for example S.A. Ackerman, 1997: Remote sensing aerosols using satellite infrared observations, J. Geophys. Res., 102, 17069-17079).

Section 5.1: Is the MODIS instrument described as input for ORAC? Then please make subsections 5.1.1-5.1.2 one subsection. Otherwise, if MODIS products are used, describe them (which algorithm, which collection, how they are aggregated).

p. 12 l. 5: How is 11μm AOD derived? Here again the description of the derivation of optical properties and basic assumptions is missing. Without that the reader is not able to understand how 11μm AOD and other ash layer parameters are derived.

p. 12 l. 20: It would be good to present the number of coincidences alongside.

p. 14 l. 4: Does that mean that for the aircraft data bimodal lognormal disrtibutions are assumed? It would be good to see the parameters for both modes along with the effective radii in table 1.

p. 15 l. 15: I have no hint from the manuscript where the authors derive this finding from. It would help to have the IASI derived effective radii averaged for the flight areas as well in table 1 (or at least mentioned in the text) or to have similar histograms as that in figure 7 from the aircraft data.

Figure 7: What is the bin size of the histogram? Is it really necessary to have such small bins (I assume the bin size is well below the assumed accuracy of the retrieval?)?

Figure 8: What exactly is colocated with what here? Are the black triangles IASI cloud top height? Does the CALIOP derived cloud top height include aerosol layers? More explanation is necessary.

p. 16 l. 20: How small is "small"? As before: it would be good to present numbers. Even if they are small: everone acknowledges that the conicidences are not widespread; providing these numbers does the manuscript no harm.

Figure 10 and 11: I would appreciate to have basic statistics (number of coincidences, correlation coefficient, bias, RMSD) together with the plots - either annotated to the plot or mentioned in the caption or the text.

p. 19 l. 10: I am not really convinced that this claim is true. What about the uncertainties of the ash optical properties? Where in the optimal estimation scheme are they reflected? Otherwise it is just not correct that all inaccuracies are accounted for.

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