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Atmospheric Measurement Techniques Discussions

Interactive comment on "Remote sensing of volcanic ash plumes from thermal infrared: a case study analysis from SEVIRI, MODIS and IASI instruments" *by* P. Dubuisson et al.

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Responses to general comments and recommendations of the reviewer#1:

We would like first to thank the reviewer for their helpful comments and suggestions. The paper will be then deeply modified following these recommendations. In particular, we will enhance the manuscript in the main following items. Note that these general comments are also valid for the reviewer#2. - The objectives of the study will be better defined: indeed, reviewer's comments have shown that the goals of our study are not clear enough. Especially, the objective of this study is neither to present a new retrieval

method, nor to define an operational algorithm. This point will be better explained in the revised version. - Reviewer's comments have also shown that some parts are not clear and they have to be better explained: some paragraphs needs then to be rewritten or improved, such as the inversion algorithm description, comparisons of our results with others studies and the use of three channels to better constrain the inversions. - Intercomparisons and analysis have to be more quantitative. Especially, an error budget will be added in the paper to better quantify the influence of atmospheric and surface parameters, as requested by the two reviewers. Comparisons or results will thus be discussed in relation with this uncertainty analysis. - We agree that the section concerning the use of a third channel to improve the retrievals is obviously confusing: the objective is to analyze if the use of a third channel can better constrain the retrievals and, finally, to obtain a more precise information on the volcanic plume. This section will be modified. - We still believe that it is interesting to analyse the retrievals of different instruments using the same algorithm. However, we agree that the approach used in our paper for this analysis is probably too much qualitative to be valuable. In the revised version, this analysis will be done in relation to the uncertainty analysis and the characteristics of the instruments. - The bibliography will be also completed and updated, as requested.

More specific comments of the reviewer are discussed below:

Referee #1: (25 April 2013)

General Comments and recommendation 1/ The paper does not contribute much to the current literature. The authors have implemented a well known algorithm for the retrieval of airborne volcanic ash from infrared sounders and discussed the results of a single scene of a volcanic plume. The fact that 3 different instruments are used does not mean there is something innovative in the presented paper. The comparison is in any case of little relevance for reasons outlined below. 2/ I have little confidence in the presented retrievals as several implementation choices are dubious at best (see below in the specific comments). 3/ The paper addresses several topics but does not treat

any of them in depth. (like section 5 and 6 are far too brief to be useful). Again, nothing new is presented.

So in my personal opinion the paper falls short both scientifically and technically and I can therefore not recommend it to be published in AMT in anywhere near its current form.

Specific Comments 1/ P2796, line 16: “narrow band sensors”. Do you refer here to MODIS and SEVIRI? If so, to my knowledge they are more commonly referred to as broadband sensors. Please give a reference or fix in the paper (the term narrow band appears several times).

This comment will be considered in the revised version.

2/ description of IASI (P2797): please explain or correct how a 50x50 km2 atmospheric cell corresponds with a ground resolution of 12km.

This point will be better explained in the revised version: The effective field of view (EFOV) is the useful field of view at each scan position. Each EFOV consists of a 2 x 2 matrix of so-called instantaneous fields of view (IFOV). Each IFOV has a diameter of 14.65 mrad, which corresponds to a ground resolution (footprints diameter) of 12 km at nadir.

3/ Use of IASI channels. The authors only use 3 IASI channels avoiding interference with gaseous absorption. This in my opinion needlessly complicates the comparison with SEVIRI and MODIS, and results in an apple-orange comparison. A far more logical approach is to integrate the IASI spectrum over the SEVIRI/MODIS bands and proceed in this way. In any case, what is the point of using a high resolution instrument if one only uses 3 channels?

The integration of IASI spectra will be done and analysed in the revised version, in order to quantify the influence of this spectral integration on the retrievals in comparison to the use of only 3 channels.

4/ Description of the retrieval algorithm. The split-window technique is mature and very well documented. The authors do not adequately acknowledge and refer to previous work. See “Prata, A. J. & Prata, A. T. Eyjafjallajökull volcanic ash concentrations determined using Spin Enhanced Visible and Infrared Imager measurements J. Geophys. Res., 2012, 117, D00U23. “ and all references therein. As far as I can tell nothing is new in the proposed retrieval algorithm of the authors, other than some minor implementation details.

We agree and it was mentioned in our paper that the objective of this paper was not to develop a new algorithm. This aspect will be better mentioned in the revised version. Additional references will also be added.

5/ The very important issue of underlying surface temperature/meteorological cloud is not addressed at all. Normally surface temperature as well as cloud top temperature would be part of the LUT. If this is not the case than any good match of the reported results with the literature can only be attributed to coincidence.

A detailed uncertainty analysis will be added in the revised version in order to quantify, for instance, the influence of clouds or surface temperature on retrievals.

6/ The third point of the retrieval method, namely how exactly the spectra are matched to the LUT is not well described. It appears this matching is done for each particle type; but then how is the particle type selected?

This point will be better described, as requested by the two reviewers.

7/ The end of section 3, beginning of section 4 is confusing. They both talk about 6 May, as if it were two different events. The two should obviously be discussed in a coherent way.

In order to avoid confusion, the comparisons will be presented in a more coherent way (especially by using a better collocation in time for selected data).

8/ On the retrieval of particle type. P2803. It is not serious to retrieve 3 independent

parameters (type, radius, optical depth) using only two parameters. How can you report the particle type distribution? It is neither interesting nor surprising that no good results come from this. It is not explained in the paper how this is done. If you do the split-window algorithm properly with 5 different refractive indices, you will obtain 5 different answers. It is not possible to tell which one is the right or the best one.

We know perfectly well that is not reasonable to expect to retrieve more parameters than available. However, we agree this section is confusing: the objective of the latter is to analyze if adding a third channel can better constrain the retrievals and, finally, to obtain a more precise information and with more confidence in results. This section will be deeply modified.

9/ The retrieval using three channels is not logical. The authors seem to have chosen an approach consisting of performing the retrieval twice, using two times two different pairs. Then these two retrievals are combined when a match is found between the retrieved of the effective radius. This of course yields very poor results. To fully utilize three channels, the logical thing to do would be to match the 3 channels with a 3D lookuptable.

See the previous comment.

10/ As for the intercomparison, it seems MODIS Aqua was used. This is an odd choice given that MODIS Terra has an overpass time very similar to IASI's one. A collocation in time of MODIS-SEVIRI-IASI is thus possible and I would recommend the authors to redo their analysis using MODIS Terra.

We agree: the comparisons will be performed using MODIS Terra and similar collocation in time between the instruments. The comparisons will be presented by using a better collocation in time for selected data.

11/ The abstract underlines that the overall motivation of this study is "to evaluate the consistency of retrievals from different thermal infrared instruments". A comparison like

this is usually done when dealing with different retrieval algorithms. This is not the case here. Assuming instruments are well calibrated, the only reason why retrievals from different thermal infrared instruments would be different, is major differences in instrumental characteristics. However this is not the case: - Spectral resolution/coverage. Here the difference between SEVIRI and MODIS is minimal. For IASI, a logical approach would have been to integrate the spectrum over the SEVIRI/MODIS band, so that also there the influence of the instrument would be minimal. (but see comment 3) - Footprint: Retrieval of ash has a large dependency on underlying surface/clouds and the presence of semi-transparent overlying clouds. The footprint will have an influence here. The smaller the footprint, the more chance of having 100% clear pixels. Also, a small footprint will allow to catch local concentration peaks. - Overpass time. Different instruments have a different overpass time. But since SEVIRI has a high revisit time, it is easy to collocate these measurements with the other two, which are around 9.30-10.30 local time (for IASI and MODIS Terra). So the only real reason why retrievals could be inconsistent is the due to a difference in footprint size. But these differences would naturally average out when looking at a large plume. So the overall conclusion that "the results are in good agreement" is hardly surprising. Given its limited relevance, this should not be the main point of the paper, and it should not be advertised as such in the abstract.

We are convinced that is very relevant to analyse the retrievals of different instruments using the same algorithm. However, we agree that the approach used in our paper for this analysis is probably too much qualitative to be valuable. In the revised version, this analysis will be done in relation to the uncertainty analysis and the characteristics (spectral and spatial) of the instruments.

12/ Why is there such a large difference between the retrievals of MODIS and SEVIRI?

This point will be commented in the revised version, following the results of the uncertainty analysis.

13/ Section 6 on retrieval uncertainties is incomplete, both in the number error terms and in the depth of the discussion. See eg. Pavolonis, M. J.; Feltz, W. F.; Heidinger, A. K. & Gallina, G. M. A Daytime Complement to the Reverse Absorption Technique for Improved Automated Detection of Volcanic Ash J. Atmos. Oceanic Technol., 2006, 23 (11), 1422-1444. and Wen, S. & Rose, W. Retrieval of sizes and total masses of particles in volcanic clouds using AVHRR bands 4 and 5 J. Geophys. Res., 1994, 99, 5421-5431. and references therein. The main sources of errors are (of which only 2 are discussed in the paper): 1. plume height, 2. surface temperature/underlying cloud temperature 3. aerosol refractive index 4. instrumental noise 5. Size distribution 6. interfering trace gases (H2O, SO2) under, in and above the plume 7. overlaying meteo clouds.

As mentioned above, a detailed error budget will be added in the revised version in order to quantify the influence of atmospheric and surface parameters, as requested by the two reviewers. As far as possible, the previously proposed parameters will be taken into account.

14/ Overall, apart from in the introduction, the paper does not discuss the methods and the presented results enough in the context of other relevant studies.

The bibliography will be also completed and updated.

Interactive comment on Atmos. Meas. Tech. Discuss., 6, 2793, 2013.