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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 of the reviewer#2:

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#1. - 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 reviewers are discussed below:

Referee #2:

General comments This paper reports on retrieval of volcanic ash optical depth and radius (from which mass loading is also obtained) for a specific test-case study, using thermal infrared data from SEVIRI, MODIS and IASI. A quick comparison between the results from these instruments is proposed, and uncertainties brought by altitude and refractive index (particle type) mischaracterization are mentioned. The general construction of the paper seems perfectly reasonable: first showing that there is exploitable information in the spectra, then explaining the method, showing and comparing results and finally mentioning uncertainties. However, comparisons are only made between

results from this work, not with previously published results on the same eruption, and the uncertainty discussion is very limited. The paper focuses on showing consistency between retrievals from the three instruments, while it would maybe be more interesting to discuss complementarities, especially considering the huge resolution difference between IASI and the two other instruments considered. This is briefly mentioned at the end of the paper but not at all exploited. Even though work has been clearly done (and is in the general scope of AMT), I find very difficult to extract from this paper new concepts, ideas, results or conclusions. Scientific concepts and results are presented as if they were new but it would seem that the authors did not keep their literature reading up to date (see specific comments). Indeed, the literature citation is very limited, especially in the introduction. Furthermore, many parts of the paper lack in precision (see specific comments). The extensive use of terms like “adequate”, “accurate”, “useful”, “satisfactory” should also be avoided because they are purely qualitative and their interpretation depends on the reader. Parts of the scientific discussion of results are not complete and even misleading (again see specific comments). In particular, I find extremely surprising the amount of information that the authors expect to retrieve from 2 channels: cloud discrimination, OD, particle size, and particle type from 6 possibilities! Given that, we could directly jump to the conclusion that they indeed reach: it’s not possible to retrieve all that with as little spectral data, even if adding a third channel. The whole discussion in section 4 is unclear and inconclusive (more details in the specific comments). A long list of specific comments is found here under. I would clearly not recommend the paper for publication in its current form. Major improvement is required, together with additional work to reach substantial new conclusions (the current conclusions do not bring new relevant information to the scientific community).

Specific comments : Page 2794 line 25: More recent references about impact on aviation would be nice. Surely there are lots of publications AFTER the Eyjafjöll eruption studied (not saying that the cited reference is not good, only that maybe an additional one, more recent, would be appropriate) Page 2795: - lines 1 and 6: Surely the Proceedings cited as only reference here is not the only work done on satellite retrievals

of ash plumes and on the Eyjafjöll eruption; same comment for the same citation page 2796 line 4: right after writing that this eruption is well documented in the literature one would expect more than a single reference to a Proceedings. - line 9: again why only this reference, there’s a lot of work done since 1989 on methodologies for volcanic plume characterization using passive sensors! - line 11: why “especially” the split window technique? Give a reference or show that this method is more used than other methods (or use another word)

The bibliography will be also completed and updated in the revised version.

- lines 17-18: “extinction efficiencies of particles vary with the wavelength in the infrared window” -> indeed, but with this formulation it would seem that the extinction efficiency in constant everywhere else, which is not the case at all

This point will be better expressed.

- last paragraph: what does “adequate spectral characteristics” mean more precisely? Which optical and physical properties would you expect to be able to retrieve (be more precise)? What does “accurate radiative transfer calculations” mean? Same last two comments for page 2797 lines 16-18. Page 2796: - line 16: “adequate spectral response”? which is? - SEVIRI: spectral resolution for TIR channels?

Each of these items will be detailed in the revised version.

Page 2797 - MODIS: spectral resolution for TIR channels? - IASI: “accurately calibrated” ? + explanation of the IFOV and pixels should be rewritten, the atmospheric cell of 50x50km2 is divided in 2*2 circular pixels which do not cover the whole cell, explaining the ground resolution of 12km2 at nadir (and lower resolution at the far sides) - line 13: “due to orbit characteristic of the satellites” -> give them somewhere (e.g. with the instruments descriptions)

These points 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.

- lines 13-14: Why is it only possible to compare near-simultaneous retrievals from MODIS and SEVIRI? Is SEVIRI not measuring every 15 minutes day and night? Thus SEVIRI might certainly be near-simultaneous with IASI twice a day. Furthermore, there are two MODIS instruments, the authors here do not explicit which one they use, but following the time of measurement given later in the paper, I would think that they use MODIS/Aqua. Why not also MODIS/Terra, which is almost simultaneous with IASI? Thus it is actually possible to compare all three instruments for measurements around 9.45 UTC. Same comment for page 2805 lines 15-18.

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.

Page 2797 line 25 how has the RT code been “adapted” to the spectral response of IASI (which is?)? Is it possible to describe this adaptation in a few words instead of only providing a citation?

The radiative transfer code has been developed for the IIR instrument. The parameterizations used for gaseous absorption have been then modified in the code to take account of spectral characteristics of the instruments used in this study. These points will be detailed on the revised version.

Page 2798: - first paragraph: this work is not the first one showing that the TIR radiance allows to discriminate between clouds and ash, and those two from clear sky, so some citations could be given here

The bibliography will be also completed and updated in the revised version.

- lines 19-20: “interactions between absorption and scattering” -> what does it mean?

By the way, is it single or multiple scattering?

It is multiple scattering: this point will be mentioned.

- line 20: why are conversion tables from BT to radiance needed? It is a simple equation to apply, no need of tables that will introduce uncertainties from interpolation.

We have to take account of the spectral response of the instrument to calculate the BT with accuracy. LUT have been calculated using the real spectral response of each instrument with an increment step accurate enough to avoid uncertainties from interpolation. Note that this algorithm is not dedicating to operational applications and this approach is not limiting for the purpose of this study.

- line 22: “true spectral response of the sensors” -> which is?

We have used the spectral responses of the instruments available on the official website of the NASA or EUMETSAT. Perhaps the expression “true spectral response” is confusing and it will be rephrased in the revised version.

- line 23: “thermodynamic profile”: what exactly is required by the RT code and why is it thermoDYNAMIC? Same at page 2801 line 7

All the atmospheric parameters (Pressure, temperature, water vapour profile, ...) needed as an input of the radiative transfer code will be defined in the text. Moreover, the term “thermodynamic” can be used in science for all the phenomena that are related to or dependent on the temperature and its variations. This appellation is then suitable for instance for pressure, water vapour, etc.

- lines 24 to end: how is it possible to find channels without gaseous absorption while there is at least the water vapour continuum? Anyway as the authors here want to investigate consistency between the three instruments, IASI measurements for the whole spectral band of SEVIRI or MODIS should be used, convolved with the instrument function of SEVIRI or MODIS. If using only one channel from IASI it is obvious that it does not contain the same information, and even though comparisons may always be done,

this will not give information about “consistency” between instruments (as the retrievals start with totally different spectral information).

Column integrated water vapour is low for the considered atmospheres and the influence of the water vapour continuum remains weak between 8 and 15 mc, but we agree and we will perform simulations to quantify the influence on BT. In addition, 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.

Page 2799: - line 13: sigma if fixed to 2 “because it is in the range [. . .]”? This indeed states that 2 is not a wrong value, but why is it decided to fix it even for very big effective radii? Does it make sense or should sigma also vary with the mean (or effective) particle size? Or do you have proof that modifying sigma does not impact the optical properties of the aerosol? Still, the same effective radius may be obtained from different pairs of (mean radius, sigma).

This parameter is often fixed in numerous studies on volcanic plumes but, as mentioned above, a detailed uncertainty analysis will be added in the revised version. Especially, we will study the impact of this parameter variation in a realistic range.

- line 20: aerosol optical properties computed at 1cm-1 resolution then “integrated” along spectral response -> should specify that this is for SEVIRI and MODIS (for IASI, I guess that no integration is performed); furthermore, convolved would be a more correct term.

This point will be modified in the revised version.

Page 2800 and figure 2: what is wished here are 3 information (OD, size, discrimination) out of 2 BT values! In figure 2, OD and size are varied simultaneously, so this plot does not allow showing that the retrieval of those two parameters is independent. The values of OD used should be shown on the plot together with the particle size to allow proper interpretation, and each parameter should be varied separately. I would

expect that multiple pairs of (size, OD) lead to very similar patterns in the plot and that no unique solution exists (i.e. that the same (BT, BTD) pair may be obtained by different (size, OD) pairs). Did the authors look for such possibilities? (same comment arises page 2801 lines 11 to 16). From all that, it seems that figure 2 only shows that the plotted BTD to BT dependence is different for clouds and ash, but does not give relevant information regarding what can be retrieved for ash.

As mentioned above, we agree that it is not possible to retrieve more parameters than available. However, we agree that 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.

Page 2801: - lines 3 to 10: which OD and effective radius exactly (and which pairs) have been used for building the LUTs? Is this computation of BT for each channel of each instrument, for all aerosol characteristics including different particle composition, done for each spectrum? Wouldn’t there be a faster way to do that? How are the surface properties (temperature and emissivity) accounted for and where does the data come from? They matter highly for TIR radiance computation.

LUTs have been built with OD from 0 to 20 and effective radius from 0.2 to 30 mc for each type of particle and for each channel. The ocean surface is assumed to be a blackbody with a surface temperature coming from the ECMWF reanalyzed data. This information will be mentioned in the revised version.

- line 22: how is the geometrical thickness of the ash plume obtained?

The ash plume is fixed to a homogeneous layer defined by the atmospheric profile and generally of a few hundreds of meters thick

Page 2802 lines 4-5: “the atmospheric profiles and the state of the volcanic plume” -> be more specific about the information required by the RT code (e.g. which molecules

are included, and what information on the plume is fixed)

All the atmospheric parameters (Pressure, temperature, water vapour profiles, ...) needed as an input of the radiative transfer code will be defined in the text.

Page 2803 last paragraph: how is it possible to get the percentage of each particle type? Each spectrum leads to 6 different solutions (at least. . .) for the 6 particle types, but these solutions do not provide any quantitative information regarding proportions between them. Or maybe it is based on the relative number of solutions for each particle type for the whole plume? However from the previous explanation of the retrieval algorithm it seemed that a solution was obtained in each case for each particle type. Anyway all this should be clarified, and in case a solution is not always found, it should be explained how it is determined if a solution exists or not (because the method is based on linear interpolation from modeled BTs and I do not see how this could not give a solution every time). The final sentence is completely useless: obviously it is not possible to retrieve from 2 channels the OD, size, (cloud discrimination) and particle type from 6 possibilities!

We will clarify all of this part. For the last point, see above.

Page 2804: - first paragraph and figure 6: this is not necessary, it does not bring anything new or important or even used later on in the paper; if kept, the relevance of a uniform size distribution should be discussed

This figure will be deleted.

- lines 15-16: “simulations presented in figure 5” -> is it not retrieval results?

This sentence will be rephrased.

- figure 8 and its discussion: I do not quite understand what this brings to the discussion. This is obviously linked to the lack of explanation on when only one particle type is “retrieved” (and relevance of this as indicator of the presence of only this particle). Furthermore, 20% difference between the particle size from the 2 retrievals is quite

high, why was this value selected?

This value will be discussed in taking into account the error budget.

- final line: again, not very surprising that three spectral information are not enough for retrieving that many parameters. However, there’s no clear conclusion regarding the additional information brought by using this third channel. I wonder if the fact that a lot less results are obtained when using 3 channels than when using 2 channels is not simply the reflection of the multiple results possible in the 2 channel case (from which one is “arbitrary” selected), leading for the two pairs of BT to mutually inconsistent results.

The main reasons are (i) the particle model used for simulations which is not perfect (particle shape, spectral characteristics, etc.) and (ii) situations including a mixing state of aerosol particles that it is not taken into account in our retrievals. In this case, the addition of a spectral band makes more difficult the retrievals. This part will be better explained in the revised version.

Page 2805 section 5 and figure 9: why do the authors not use more measurements (IASI night data, MODIS from the second platform, ...)? + same comment as earlier regarding the possibility for near-simultaneous data from all instruments; Line 21: “slightly” different spectral configurations -> certainly not if comparing IASI and the other two We agree: as mentioned for the reviewer #1, the comparisons will be performed using similar collocation in time between the instruments.

In addition, we don’t use more measurements because the objective is to analyze a case study to evaluate consistency of retrievals using several instruments.

Page 2806: - line 1-2: “The mean retrieved sizes should be explained by the spectral characteristics of instruments” -> please rephrase, I do not quite understand what it means, and if it means that the difference in mean radius is due to the difference in spectral resolution/window, I would say that this is not the only possible explanation

(possible time and space differences as mentioned further in the paper) and that the authors should maybe think of a reason why MODIS results are closer to IASI results and more different from SEVIRI results.

As mentioned above, this section will be modified.

- line 8: the “time lag” may (indeed) explain part of the differences, BUT in the choice made for the retrievals there is no time lag between MODIS and SEVIRI while the major differences seem to be between those two retrievals.

There are some potential differences such as slight differences on spectral position of channels or footprints. Impacts of these differences will be evaluated with the uncertainty analysis. The conclusion will be more quantitative in the revised version using the result of the error budget.

- lines 10 and 12 say the same thing, it is like saying this is like this because it is like this; anyway, why should the mass loading decrease with the spectral resolution of the instrument?

It is mentioned in the text that the retrieved mass loading decreases with the spatial (not spectral) resolution of the instrument. Here, we show the influence of the instrument spatial resolution on retrievals of mass loading.

- line 16: which IASI IFOV do you mean? The 12km2 pixel? If yes, then it is the correct appellation, but it should be checked as the previous description of IASI IFOV was not fully clear.

See above.

Page 2807: - line 4: what is a “satisfactory level of agreement”? The whole discussion of the previous section was showing significant differences, so the authors should explain why when changing of section is becomes “satisfactory”.

We agree that the intercomparisons and analysis have to be more quantitative. Espe-

cially, an uncertainty analysis will be added in the paper to better quantify the influence of atmospheric and surface parameters. Comparisons or results will thus be discussed in relation with this uncertainty analysis.

- line 21: what does the different refractive index have to do with altitude sensitivity?

We don’t understand this comment: it is written that, for a given pixel, multiple solutions can be found as a function of the plume altitude and the aerosol type. This sentence will be rephrased.

Page 2808 - line 13 to 15: of course retrievals in the TIR are sensitive to refractive indices (it is that and the particle size distribution that condition the aerosol effect), the interesting part is to know how much different the results are with different refractive indices, to quantify the uncertainty, so I would remove this last sentence.

This sentence will be removed or rephrased.

- line 17: “consistent” retrievals? Again, why from section 5 where there were high differences mentioned we arrive here to consistent retrievals? And anyway consistent how?

We agree, as previously mentioned. Comparisons or results will thus be discussed in relation with this uncertainty analysis in a more quantitative manner.

- lines 21-22: the way IASI data is used does NOT ensure “consistent” comparisons: for that, IASI data should have been convolved with MODIS/SEVIRI instrument functions, as explained in a previous comment.

We agree: 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.

Figure 4: why does this stop at 14:00 while the next figure shows results at 19:00? It would be better for comparisons if the time frames overlap.

We agree, as mentioned for the reviewer #1, that the comparisons and simulations will

be performed using similar collocation in time between the instruments.

Figure 9: the scale for IASI OD seems slightly different from the scale for the other OD plots; time of each measurement should be written somewhere on the plots

Indeed, the colour scale in figure 9 seems to be not exactly the same: this will be modified in the revised version.

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