

## ***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.***

**Anonymous Referee #3**

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### 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

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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.

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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)

- 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 is constant everywhere else, which is not the case at all

- 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?

Page 2797

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- MODIS: spectral resolution for TIR channels?

- IASI: "accurately calibrated" ? + explanation of the IFOV and pixels should be rewritten, the atmospheric cell of 50x50km<sup>2</sup> is divided in 2\*2 circular pixels which do not cover the whole cell, explaining the ground resolution of 12km<sup>2</sup> 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)

- 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.

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?

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

- lines 19-20: "interactions between absorption and scattering" -> what does it mean? By the way, is it single or multiple scattering?

- 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.

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- line 22: "true spectral response of the sensors" -> which is?
- line 23: "thermodynamic profile": what exactly is required by the RT code and why is it thermoDYNAMIC? Same at page 2801 line 7
- 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).

Page 2799:

- line 13: sigma is 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).
- 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.

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

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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.

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

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size, (cloud discrimination) and particle type from 6 possibilities!

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

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

- 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?

- 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.

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

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,

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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.

- 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.

- 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?

- line 16: which IASI IFOV do you mean? The 12km<sup>2</sup> 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.

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".

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

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.

- line 17: "consistent" retrievals? Again, why from section 5 where there were high

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differences mentioned we arrive here to consistent retrievals? And anyway consistent how?

- 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.

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.

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

#### Technical comments

- page 2795 line 23: volcano -> volcanic
- page 2802 line 18: “not so far from [THE] volcanic source”
- page 2802 line 20: “not AS thin as [THE] one observed”
- page 2806 line 11: “from MODIS compareD to SEVIRI”
- page 2806 line 20: “although [THERE ARE] differences in spatial resolution”
- page 2807 line 10: “In the same manner” should be removed

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