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Discussions

Interactive comment on "Joint retrieval of surface reflectance and aerosol properties with continuous variation of the state variables in the solution space: Part 2: Application to geostationary and polar orbiting satellite observations" *by* Marta Luffarelli and Yves Govaerts

Anonymous Referee #1

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I think Part II does a rather nice job in introducing the different sources of uncertainties for SEVIRI and PROBA-V (uncertainty assessment - a necessity - is too often neglected). As a general impression though, the writing seems more of a lab "living" log, i.e. notes accumulated as the work was being carried out. You'll notice how many of my highlighted comments aim at condensing the text as the reader might get lost in

details that are often redundantly expressed. Please try and be concise. Every time you start a sentence with "In other words,", ask yourself why you need to re-explain what you just said. As a reader, I had the impression of re-living the struggles to make sense of results, and learning a lot about the things that can go wrong while developing an algorithm, rather than walking confidently away with a message on original and reliable results. This is also reflected in grammatical hurdles. The manuscript should be proofread before submission; see countless instances of 1) "on" instead of "in"; 2) excessive use of "i.e.", "the former" or "the latter", "ones", "it can be seen", "it should be noted", or references to other sections when not really needed; 3) missing plurals; 4) missing articles; 5) "Section" and "Figure" instead of "Sec." and Fig." according to the journal's guidelines; 5) the term "miss-fit".

A pdf with detailed comments is attached. In the following are some other general comments:

Line 116: BRF needs be defined.

Section 2 concludes with "More effort would be needed to demonstrate that the forward RTM is unbiased". This is the kind of sentences disseminated all over Part I that shake confidence in the method. This particular sentence alone gives the impression that the whole method is systematically flawed. Unless the bias is quantified being negligibly small what should the reader take away from this message? As remarked above, the draft goes at quite a length in explaining different sources of uncertainties smaller than 1%; if this last bias is larger, it would cast quite a different light on the accuracy of the method.

Figure 4. This way to depict the subspace of solution is misleading. For example, the way you have things set up now, the magenta triangle does not include the peak of the distribution, with omega>0.98 and g~0.75. Lots of aerosol types are found in this region. How do you deal with this?

Regarding the graphs: - enlarge ALL font sizes. Things are barely legible. - please

take away the grid lines form all graphs. They're extremely confusing and provide no useful information. - many figures feature symbols in yellow green and violet, which are among the least differentiable colors (ask color-blinded folks). Try with more opposing colors like blue (or black) and red.

Fig 6. : merge the two panels into one, since you compare Carpentras with Zinder.

Line 280-281: this statement is simply not true and has to be reversed. While it is true that the diffraction peak is very sensitive to size, the backscattering contains tons of information (pretty much everything else). We wouldn't be doing space-based remote sensing otherwise!

Line 283-284: what was the retrieved optical depth for this day? AND AT WHAT WAVE-LENGTH? This is an essential piece of information. How would the figure change if the AOT is 0.05 or 0.8? A discussion on the linearity of the AOT Jacobians is due in the text.

The "Principle" in Sec. 5.1 needs to be explained better. Please re-elaborate lines 320-330. I simply couldn't get why the number of could-free pixels should be proportional to the quadratic sum of the mismatch between simulation and observation. Even in the rest of the section, I lost the logical thread. The QI/p tests part is very mysterious, I just did not get it. "QI" is not even defined, and there's no explanation of its range of values. Please review the whole text and try to make it more understandable. Also, "miss-fit" is not a correct terminology; change to "mismatch" or something else. Little to no guidance is offered for the comprehension of Fig. 11. WHEN IS A RETRIEVAL DEEMED SUCCESFULL?

In both manuscripts, it's never clear if CISAR can be applied to water and land indifferently. This should be made more clear throughout.

The approximation of a two-layer atmosphere is not discussed. In fact, it could be a reason for the algorithm failure in many cases.

Overlap graphs in Figs. 9 and 10 so as to make one figure only

Discussion following Eq. 10: it has to made clear if you're talking about "entropy" or "entropy difference" between pre and post retrieval.

Sec. 5.2 is "Theoretical Concept" and comes after Sec. 5.1, i.e., "Principle". I see no point in fragmenting the text this way. Please condense the sections.

Line 360: it remains a mystery why a cloud mask is not applied.

Line 456-459. This is one of my most important comments. After the manuscript goes to a great length in describing a very elaborate way to aid the retrievals with "tests", the results presented in Fig. 14 are clearly not satisfactory (a look at the correlation coefficients immediately tells that the algorithm is not retrieving appropriate AOTs). Then it is commented that at high AOTs the algorithm might fail (then why all the tests?), but that's not too worrisome since it is better if it performs accurately at low optical depths, which are more typical. I might agree with that, but then I have to ask 1) how do you deal with the fact that the 1:1 correlation is as poor at low optical depths; and 2) why the only AOT used for testing was 0.4 in part 1.

Line 477-480. I don't understand these comments about Fig. 17. CISAR/SEVIRI is in very good agreement? As CISAR/PROBA-V, it misses the peak of the distribution. Also, CISAR/PROBA-V is said to be underestimating the fraction but so does CISAR/SEVIRI. The significance of the ratio should also be discussed. What are typical ranges?

The relative magnitude of those"spikes" in Figs. 19 and 20 are worrisome. For the causes you attribute, shouldn't they confirm that your choice of the three vertices is inadequate?

Line 487: I take the chance here to expand on previous comments. "Coarse mode characterization" is very far-fetched. The algorithm is not so much retrieving surface and aerosol properties, as much as two aerosol radiative properties and a set of RPV

parameters white variability has not been ascertained. Even here, you've already got problems with unreliable retrievals of fine-to-coarse ratio, so much that you focus on the ratio being less or larger than 1. For these reasons, the title sounds a bit pretentious and should be adjusted accordingly. Omega and g are properties but based on the current title nowadays most readers would expect an extended set of microphysical and optical properties.

Sec. 6.3: how about Carpentras?

Line 545-547: This is either too obvious or a concept I don't get. You don't describe state variables, you retrieve them, so isn't just that the algorithm fails?

The manuscript should report complete statistics on the number of analyzed scenes, so that the retrievals can be put in context. I'm not sure this is what happens in Table 11.

Please also note the supplement to this comment:
https://www.atmos-meas-tech-discuss.net/amt-2018-265/amt-2018-265-RC2-supplement.pdf

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Interactive comment on Atmos. Meas. Tech. Discuss., doi:10.5194/amt-2018-265, 2018.