

Interactive comment on “Sensitivity of PARASOL multi-angle photo-polarimetric aerosol retrievals to cloud contamination” by F. A. Stap et al.

Anonymous Referee #2

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This paper presents a novel cloud screening method for multi-angle, polarization sensitive passive instruments devoted to aerosol remote sensing. Rather than rely on high spatial resolution imagery to provide cloud masks, this method proposes using the retrieval goodness-of-fit metric as a means to identify pixels that have cloud ‘contamination’. This work represents an important contribution, particularly in the conclusion that using this technique would render the need for cloud imagers in future multi-angle polarimeters unnecessary.

A drawback of this method is related to the fact that any observations that do not correspond to a clear sky radiative transfer simulation are removed in screening. While most of the screened scenes have a poor goodness-of-fit metric because of clouds, I would have liked to see a deeper examination of alternate ways in which a poor goodness-

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of-fit metric is found. Ultimately, this method does not provide a means to differentiate between the causes due to liquid clouds, ice clouds, or simulation/optimization inadequacies. Furthermore, the authors found the need to provide additional screens to remove ice (cirrus) clouds, based on retrieved coarse mode refractive index and additional information from the MODIS cirrus mask (that uses a 1.38 micron wavelength channel). For that reason, I think would have been possible to differentiate better between the various causes of poor goodness-of-fit, since a goodness-of-fit criteria based solely on the viewing geometries associated with the liquid cloud bow (~140 degrees scattering angle) could be used to identify liquid clouds. This, combined with the ice cloud screens, would allow for differentiation between sources of poor goodness-of-fit, since a poor overall goodness-of-fit (but lack of other screens) would indicate simulation/optimization inadequacies.

In any case, I think this is a good paper that should be published with revisions related to a fuller description of the limitations described above. Specific comments follow.

1. The screening techniques are tested entirely on synthetic/real observations for ocean scenes. In that sense, this technique is only currently appropriate for the oceans, and this must be made clear in the abstract. As an aside, I would think that multi-angle polarization would facilitate even better cloud screening over land, typically subject to confusion with bright surfaces/snow for non-polarimetric single view angle observations. Is this true?

2. Page 2, Line 92: grammar, change to – “screening is not applied strictLY enough”

3. Page 3, last paragraph on left hand side – This is probably a good place to refer to and describe the 2010 simulation study by O. Hasekamp (Capability of multi-viewing-angle photo-polarimetric measurements for the simultaneous retrieval of aerosol and cloud properties).
4. Section 4.1 – While I generally appreciate simulation studies with realistic AOT, in this particular case I would have liked to see simulation of large AOT such as would be encountered in a smoke or dust plume. Those scenes are often

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flagged as cloudy. I assume your technique would not flag them that way, so it would strengthen your case to show this with simulations.

5. Section 4.1 – The angular sampling of PARASOL was too coarse to reliably sample the liquid cloud bow in all observations. In other words, in some pixels the cloud bow was centered at the viewing angle of an observation, in other pixels less so. I assume the identification of liquid clouds would be much more clear in the former than the latter case. How is this expressed in the simulations? Were they performed for a fixed set of geometries? If so, how representative are those geometries compared to the actual dataset?

6. Section 4.1 – The simulation involved the addition of Gaussian noise of 0.005 to the degree of linear polarization. What is the basis for this amount of noise? From what I understand, PARASOL polarimetric uncertainty is at least twice that (0.01 to 0.02). If you are planning to add noise that is much lower than stated uncertainty an explanation (and reference, if possible) is needed.

7. Section 4.1 – I know it is mentioned, but it needs to be more clear (in abstract, section headings) that you are only simulating liquid phase clouds, not ice/cirrus clouds.

8. Section 4.2, page 4, 1st paragraph: Its probably a good idea to cite this paper here: Di Noia, A., Hasekamp, O. P., van Harten, G., Rietjens, J. H. H., Smit, J. M., Snik, F., Henzing, J. S., de Boer, J., Keller, C. U., and Volten, H.: Use of neural networks in ground-based aerosol retrievals from multi-angle spectropolarimetric observations, *Atmos. Meas. Tech. Discuss.*, 7, 9047-9094, doi:10.5194/amtd-7-9047-2014, 2014.

9. Section 5, first sentence: The threshold on time difference between AERONET and PARASOL observations should be mentioned here.

10. Section 5, page 5, 1st paragraph: Again, I'm surprised why such low uncertainties are assumed for PARASOL. What are the 'false polarization' (moving filter wheel) impacts on scenes such as this that have big differences between bright clouds and dark

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oceans?

11. Section 6, line 683-6: I think this is a very important conclusion of this work! Great!

11. Table 2: Are your simulated coarse mode particles big enough? It would have been nice to push the boundaries of large particles that may (or may not) begin to have rainbow-like features.

12. Figure 6: Do I understand correctly that your interpretation of the medium resolution figure shows now clouds? It looks like there is the beginnings of a rainbow at ~148 degrees.

13. Figure 7: It would be nice for the casual reader to provide more details about the cirrus mask (from MODIS using NIR/SWIR channels) in the caption.

14. Figure 11: for consistency with Fig 9 and 10, list the number of measurements in each plot.

15. Figure 12 and 13: I'm not real fond of these figures, since the temporal trends in the data occur at frequencies higher than your matchup sampling rate. But Hasekamp has used them in papers previously, so I can see the reason to keep them in for consistency's sake. I do prefer the scatterplots like you have used previously. Also, is Fig 12 for the MR data?

Interactive comment on *Atmos. Meas. Tech. Discuss.*, 7, 10673, 2014.

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