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Atmospheric Measurement Techniques Discussions

# *Interactive comment on* "Aerosol retrievals from the ACEPOL Campaign" *by* Guangliang Fu et al.

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This manuscript presents a straightforward, well-executed and well-written comparison of aerosol retrievals from three airborne multi-angle polarimeters, an airborne HSRL (lidar) and AERONET. The polarimeters and HSRL are simultaneously flying on the NASA ER-2 during ACEPOL, and all data shown are collocated in time and space, or are clearly described as being exceptions. The same algorithm is applied to the three polarimeters so that differences should be instrumental not algorithmic. I have nothing to criticize in this study or presentation, and offer only minor comments, as suggestions, below.

Before that, I will reveal myself. This is Lorraine Remer writing.

1. General comment. I understand that one of the purposes of this work is to determine expected uncertainty on the retrievals for the polarimeters. But AERONET and HSRL



should already have documented expected uncertainty. It certainly would be helpful to indicate on the figures what is the expected uncertainty of the known sensors. We see values for MAE, bias, etc., but do not know how to put these values into context. If we knew AERONET uncertainty for that parameter, for example, context could be established.

2. General comment. This is a corollary to (1). AERONET AOD has very small uncertainty, but AERONET retrieved products and these include the SDA products have larger error bars. The goal in comparing polarimeter retrievals to these other retrievals is comparison, not validation. This was not explicitly stated in the paper.

3. General comment. I see in the description of the different data sets mitigating strategies for inhomogeneity for registering the different angular views. Does this include topographical variation?

4. Page 9. Last paragraph that begins with "As with the extinction products", I'm a little unclear on what is being said here. "HSRL method" is when HSRL measures extinction. "assumed lidar ratio" is when it does not. The HSRL method is not available in many situations during ACEPOL, so the lidar information is going to come to us like an old-fashioned backscattering lidar with an assumed lidar ratio. It's not clear why the HSRL method is going to be unavailable. Then here it seems to imply that there is going to be a choice between the two methods, not that the HSRL method is unavailable, but that both are available. And then it says that the assumed lidar ratio method is actually BETTER than the HSRL method at low loading. This is because one measures its uncertainty in a relative sense and the other in an absolute sense. The fact that the assumed lidar ratio can be better than the HSRL method is very strange to me. Did I understand this paragraph correctly?

5. Section 3.5. AERONET section. Level 1.5 is cloud cleared, but not quality controlled. Also be aware that fine and coarse as defined by both the almucantar retrievals and the SDA methods are going to be different than defining fine and coarse by specific

modes as is done in the polarimeter retrieval (Table 1). This may introduce differences in your comparisons. It did with the MODIS Dark Target over ocean retrieval.

6. Page 11-12. Discussion of comparison of effective radius against AERONET. Perhaps AERONET is wrong here? This is retrieval vs. retrieval, not retrieval vs. truth. And the loading is extremely low. I would think that everybody is running on fumes here. This applies to fine mode, but especially to coarse mode. Nobody has SWIR to really nail coarse mode. And AERONET's definition of fine and coarse modes, and their respective effective radii, are defined differently than the five modes in Table 1.

7. Figure 3. If I'm interpreting these plots correctly... The MAP retrievals can be very different from AERONET. For example, RSP has differences of -0.04 where the (AERONET + RSP)/2 = 0.025. This means that RSP retrieved tau_c of 0.005 and AERONET 0.045. In absolute terms that's not a lot, but in terms of relative contributions of the coarse mode to the total AOD it is a lot. Is it within expected error of the AERONET retrieval? It would be very helpful to have some context for the magnitude of the differences.

8. Section 4.2.1 These comparisons are all with "assumed lidar ratio". Are these the only days with collocations? If there is a choice between assumed lidar ratio and HSRL method, how does the HSRL method compare?

9. Section 4.2.2. I grew up in Los Angeles and the Central Valley, so I know this territory well, but not everybody does. Maybe use "east" and "west" without place names, or annotate the image.

10. Final sentence of Section 4.2.2. " The differences from the direct comparison between SPEX and RSP are somewhat larger than those from individual comparisons with HSRL-2 of SPEX and RSP, respectively. This suggests that the differences with HSRL-2 are not caused by common assumptions in the SPEX and RSP retrievals, but are rather caused by errors that are specific to each MAP." I don't follow the logic.



11. Section 4.2.3. page 13. Lines 15-17. "It should be noted that the smoke plume exhibits large spatial variation so part of the MAP-lidar differences can be attributed to the fact that different instruments see a slightly different part of the smoke plume." What about different angles from the same instrument seeing different parts of the smoke, or what if the smoke changes between the fore and aft angles are measured? What happens to the retrieval? It would be really nice to have a quantitative sense of how variable that plume is. Could we see a spatial plot of the smoke retrievals or at least have stdev on the parameters shown in Table 2.

12. Page 13. Lines 25-26. "Our explanation for this, is that at high AOD the measured radiance and DoLP are less affected by the co-registration errors between viewing angles than for low AOD." How could this be? The evolving, hetereogeneous smoke plume has to be more difficult to co-register between angles than the unmoving ground.

13. Page 13. Lines 33-34. On the other hand, I think this is a really good explanation: "A possible explanation for the difference could be the simplified description of non-spherical particles in our retrieval approach. "

14. Figure 7d-f. Are lidar ratios here retrieved via HSRL method, or assumed? If assumed, does these figures make any sense. If retrieved, then why not use retrieved throughout the paper? Or show that they are worse than assumed. This whole retrieved vs. assumed lidar ratio choice never sat well with me throughout the manuscript.

15. Table 2. Maybe show stdev along with mean? Or show spatial distribution if any of these properties are varying downwind?

16. Page 14. Line 13. "the latter value is closer to the ALH derived from HSRL-2 (2.64 km)." Sure slightly closer, but still 1 km off. Not that much different from SPEX.

17. Page 14. Line 14. The explanation of ALH being difficult to retrieve without UV might be elaborated on a little here.

18. Finally... don't you want to state a conclusion? What is the overarching thing

you have learned? If this were my paper I would conclude that the 3 polarimeters are producing comparable results when forced through the same algorithm. The exception being aerosol layer height and perhaps some coarse mode parameters, which suffer from not having the bands that these parameters are sensitive to: shortwave (410 nm) and SWIR, respectively. So when there is no sensitivity, the retrieval becomes a random number generator. But for parameters that the instruments are sensitive to, there is little difference between instruments. It is still TBD whether algorithmic differences are going to matter. But it is not my paper. The authors can choose to write a conclusion of their choice. Or not.

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