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Atmospheric Measurement Techniques Discussions

Interactive comment on "Retrieval of Aerosol Fine-mode Fraction over China from Satellite Multiangle Polarized Observations: Validation and Application" *by* Yang Zhang et al.

Anonymous Referee #1

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This is an interesting study looking at the fine mode fraction (FMF) of aerosol optical depth in China. The main focus is the comparison between results from a previously-published algorithm of the authors applied to POLDER data (referred to in the paper as "the algorithm"), the GRASP retrieval applied to POLDER, the MODIS Dark Target land retrieval, and the AERONET spectral deconvolution algorithm (SDA) and almucantar scan size distribution (SD) retrieval algorithm. The topic is relevant to the journal and the Special Issue. After total AOD, fine vs. coarse AOD is one of the next main frontiers of interest.

The quality of language needs some improvement. I appreciate the authors' first lan-

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Discussion paper

guage is not English, and they have done a good job explaining what was done in the analysis. But some copy-editing will be necessary to bring the article to publication standards as phrasing is strange in places (too many to go through as a reviewer). This might be able to be handled by the journal production office, but if the authors have access to a service or colleague who is able to give a proof-read that would be beneficial as well. Again, the authors have done a pretty good job with the writing overall.

My recommendation is for major revisions, and I would like to review the revised version.

Major comments:

1. It is not just the FMF which is of interest, but the overall fine and coarse AOD. It seems like a missed opportunity not to evaluate e.g. the fine mode and total AOD as well. Looking at only FMF we don't know if a bias in that is because the retrievals have errors in the total AOD or just the ratio between modes. This is briefly shown (Table 3) but only via summary metrics (would be good to see the data points) and only for the authors' approach (not MODIS or GRASP). I recommend the authors add this in the revised version. These could be also split by, for example, aerosol type or surface type (as these are some factors which often affect retrieval performance).

2. The authors' interpretation of MODIS Dark Target land FMF is, to my knowledge, not correct. The MODIS land fine weighting parameter eta is not a "fine mode fraction" but a "fine model fraction". The Dark Target land retrieval mixes between two bimodal size distributions, one which is mostly fine mode and one which is mostly coarse mode. See Levy 2007 (https://agupubs.onlinelibrary.wiley.com/doi/full/10.1029/2006JD007815), especially Figure 4 there. So in MODIS, FMF=0 does not mean no fine mode aerosol. It means that the proportion of the bimodal fine-dominated optical model is zero. There is still some fine mode aerosol from the coarse-dominated optical model. Similarly FMF=1 in MODIS still has some coarse aerosol present. This misinterpretation affects all the discussion of MODIS results.

3. The definition of FMF in other products is not be the same either, e.g. the AERONET SDA assumes a combination of fine and coarse modes but the AERONET sky-scan retrieval and (I think) GRASP look for a minimum in the size distribution and make the fine/coarse split there. The manuscript should be more detailed about the exact definition of fine mode fraction within the products, and make sure they are comparable. If not then the discrepancies will be partly due to definition differences rather than retrieval problems.

Minor comments:

1. Line 98: is the EOF method similar to the MISR land approach? That could be mentioned (and compared if different) as it is likely that the readership of this journal would have some familiarity with it.

2. Line 133: Angstrom should be written Ångström here.

3. Line 154: I don't know why it makes sense for EE to be +/-(0.1+10%). Why should FMF uncertainty depend on FMF? Is high FMF slightly harder to retrieve? More justification is needed here. If this was used in a previous study, we need to see the justification there, and if there wasn't one, then that's an issue. I do not see a physical reasoning why FMF uncertainty should be a function of FMF.

4. Line 154: Also, when calculating this metric, the uncertainty on AERONET FMF should be accounted for as well (this is dependent on AOD but can also be of order 0.1 to 0.15: this is discussed in some AERONET publications). Unlike AERONET total AOD, AERONET FMF cannot be considered a reference truth because there are non-negligible uncertainties in both the AERONET SDA and SD retrievals.

5. Lines 220-221: I believe the latest GRASP is version 2.1, not 2.0.6 as stated here. Also, which GRASP product? There are 3 separate GRASP POLDER data sets with different assumptions about aerosol size distribution form. See this paper by Chen et al for more information: https://essd.copernicus.org/preprints/essd-2020-224/ More

information should be added to the manuscript, and if possible the analysis should use the latest GRASP version. Including those results in this paper (rather than just citing an older evaluation study) would also help to compare GRASP and the authors' new approach. Right now it is still not clear to me which is better or what the relative benefits are.

6. Line 243: if possible, more information about the PM2.5 and PM10 surface measurements should be made here. For example is this BAM, filter, or something else?

7. Section 4.1: I do not see much value in showing these two case studies. It's just a couple of maps and text describing them. There isn't really enough context or external data sets brought in to make them interesting. I recommend removing section 4.1 (so section 4.2 would just be called section 4), unless the authors can provide additional material of scientific interest to make the reader care about these examples.

8. Section 4.2: It would be good to add AOD maps here as well, for additional context.

9. Figure 1: "Fuction" should say "Function".

10. Figure 2: I don't know why there is a color bar (FMF) on this figure, given it is only showing site locations on a blank background map.

11. Figure 3: These panels should have separate titles or similar (e.g. a, b) to separate them. Also, I would remove the regression lines. I don't think they add anything, and don't think they are appropriate. In some cases the relationships aren't linear (e.g. QOMS_CAS), and the technique is not valid because (1) it is not accounting for uncertainty in the AERONET reference data and (2) the data are constrained by the possible bounds of FMF (i.e. 0-1) meaning that errors cannot be Gaussian and unbiased. Both of these means that the assumptions required for validation are not satisfied.

12. Figure 4: It would be good to show a second histogram in addition, filtered for points where the AOD is above a certain value (e.g. 0.2?). We would expect that this

would be thinner because the sensitivity to FMF should be better when AOD is high. So this would be interesting to see how the width and midpoint of the distribution change.

13. Figure 5: same comment about regression line as for Figure 3. Also, both data sets have 143 points here: is this for the points where MODIS and POLDER are all matched together? This should be stated in the paper, this was not clear to me.

14. Figures 6-8: can we have more than 3 points labeled on the color scale? Also, it would be clearer to combine these together into one 1 figure, possibly with figure 9 as well.

15. Figure 9: is this FMF difference or normalized FMF difference? This was not clear to me.

16. Figure 10: same comment about regression line as for Figure 3.

17. Figures 15, 16: I don't see much value in these figures and suggest deleting them. The overall spatial distribution looks fairly similar year to year, and I think the seasonal maps in Figure 17 are more useful. Likewise, I am not convinced that the differences in Figure 16 are realistic. It looks like a FMF difference of +0.2 across many parts of China, even remote areas where the aerosol is mostly dust. So in my view it could easily be a calibration drift, as POLDER had no on-board calibration. There is only a paragraph devoted to Figure 16 anyway. If the authors wish to discuss trends, it would make more sense to show also total AOD and fine mode AOD (so we can see which is increasing) and bring in some additional satellite, model, or ground-based data to help verify and understand the mechanisms. If Figures 15 and 16 are removed, then Figure 17 could also be moved earlier in the manuscript, close to where the authors' retrieval method is introduced.

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