

Interactive comment on “Sensitivity study on polarized aerosol retrievals of PARASOL in Beijing and Kanpur” by X. F. Gu et al.

Anonymous Referee #4

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This paper compares retrievals of aerosol optical depth (AOD) from polarised radiance measurements by PARASOL with AERONET data at Beijing and Kanpur. It attempts to reconcile the relative biases in the satellite data with the partition between fine and coarse mode AOD as determined in the AERONET product, as the PARASOL dataset used is primarily sensitive to fine-mode particles.

I did not find the paper particularly informative, as the analysis was not very thorough, and the conclusions were not really new. These aspects of the PARASOL dataset used have been presented in previous studies, several of which are cited by the authors, and from my perspective the strengths and weaknesses of the Deuzé et al.

[2001] approach are quite well-known. This is, for example, discussed in the AMT paper by Tanré et al. submitted to the same special issue of AMT, in which they point out the weaknesses of this decade-old algorithm can be largely overcome by the new AERONET-like full inversion of the PARASOL measurements presented by Dubovik et al. [2011] (which the authors of this manuscript even reference). The original algorithm paper also mentions some of the limitations. Further, the title does not reflect the paper's contents very well, as it is largely a comment on the AERONET validation of PARASOL at two sites (which I am not convinced the authors have performed in the most meaningful way; see specific comments).

I do not favour publication of this manuscript, as I do not believe it contributes meaningfully to the literature on aerosol remote sensing. Some specific comments are below, in case the authors decide to extend the analysis and submit a revised manuscript here or elsewhere in the future.

Section 1

If you want to evaluate the performance of PARASOL for detecting the anthropogenic contribution of AOD, it is not obvious to me that Beijing and Kanpur are the best sites to do it. Although they are large cities with a significant pollutant contribution to the AOD, as the authors mention in the study, they also have significant seasonal input from e.g. transported mineral dust, and seasons of very high cloud cover. Depending on the objectives of the study, it could be better to consider additional or alternative sites. Looking at the AERONET websites, some urban/suburban sites without significant coarse-mode contributions, and with a long time series of measurements, include NASA GSFC, MD Science Center, Rome Tor Vergata, Fresno (as noted on the AERONET page for this site, *The Central Valley of California regularly experiences some of the worst air quality in North America*), Mexico City, or Lille. There may be more, and there could be issues limiting the applicability of some of

those sites listed. But I do not think the authors have justified their choices well enough.

Section 2.1

The AERONET QA process is thorough and designed to minimise the chances of poor retrievals. So if the authors found that using only the highest QA AERONET data (level 2.0) gave a small sample size, while allowing level 1.5 improved sampling, then they should certainly evaluate in some form the differences between the level 1.5 and level 2.0 data at these sites. If AERONET inversions were not raised to level 2.0 then there was some reason for it, and the authors should be aware of, and attempt to account for, the possible implications of this for their analysis.

If the objective was more to evaluate to what extent the Deuzé et al. [2001] algorithm retrieves fine-mode AOD, then a possibly more appropriate approach would be to use the AERONET spectral deconvolution algorithm (SDA) dataset, which uses the spectral behaviour of AOD to partition between fine and coarse modes, without a size-cut-off radius of the type used by the Dubovik and King [2000] inversion algorithm. I would argue that, if you are primarily interested in fine vs. coarse AOD, the SDA data are more meaningful for this purpose. Additionally, as SDA data are derived from the direct-Sun observations rather than almucantar scans, the dataset is significantly larger, and it may be that the authors would find level 2.0 provided sufficient sampling in this respect.

Section 3

It is not useful to state percentage over/underestimates in AOD, as these are only meaningful in the context of what the absolute AOD is, and how this compares with the stated and expected uncertainties of the data being compared. The authors do not provide this information. The stated correlations between AERONET and PARASOL data found by the authors are very high, which leads me to suspect that the offset between them is probably within the expected uncertainties, although without the authors providing this information it is not possible to say.

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Section 4

I do not find significant utility in this section. I suspect much of the discrepancy arises from the way the authors performed their comparison with AERONET. It is not clear to me how the information presented here could be useful for algorithm developers or dataset users. The improvement in correlation you get by tweaking the cutoff radius for the fine/coarse partition is just saying to me that the method you are using for your validation is not particularly robust (in these cases it is not clear what the *true* unknown fine-mode AOD is, or how the discrepancies relate to the uncertainties on the data, only that by changing some assumptions made you are able to improve agreement between two retrievals). If the authors have a recommendation to make here, it should be clearly stated and justified. Additionally, it is not clear exactly what is being simulated in this section (one solar and viewing angle, for presumably a range of relative azimuth angles, and the two bands used by Deuzé et al. [2001]—why not consider the range of geometries sampled at the AERONET sites, which would give much more useful/representative results)?

Table 1

This would be much more useful if some information about the absolute AODs and absolute uncertainties were included.

Table 2 (also text and Figure 2)

I am not convinced that seasonal means are useful here. I would imagine there would be a link between the AOD and the refractive index, as presumably elevated fine-mode AODs will be associated with more fine absorbing aerosol, and elevated coarse-mode AODs will be associated with more dust. So by taking a mean you may end up with a situation which does not correspond to a condition which you are seeing in reality. Additionally, as you are using level 1.5 AERONET inversions, and the sample size

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is small in some reasons, a single poor-quality inversion could affect your results dramatically. As mentioned before, it is important to examine the level 2.0 data as well, and quantify how your sampling decisions are affecting the conclusions.

Tables 5, 6

If you wanted to emphasise the differences when you alter the cutoff radius, it would be better to combine these tables in some way.

Table 7

It is not clear what the numbers in this table refer to, or how significant they are to the retrieval. For example, 107 % uncertainty at Beijing in winter for a surface of reflectance 0.005 still corresponds to a fairly small uncertainty in reflectance in absolute terms.

Figures 3-6

It is not clear to me what the particular information of interest to glean from these figures is. The flattening around $0.35 \mu\text{m}$? Also, what is the reason for the non-monotonic behaviour from $0.22\text{-}0.23 \mu\text{m}$? The authors should mention this (I don't think they do). They should also comment on how these changes in reflectance compare with the absolute and random radiometric uncertainty of the PARASOL measurements (if within the calibration uncertainty, it's not clear whether there is much we can do about it).

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