

Dear Andrew,

Thank you very much for agreeing to review our manuscript. Thank you for your many useful and insightful comments which helped us improve the paper. We hope that you find our answers, corrections, and additions satisfactory.

Kind regards,

Marcin Witek

P5 L25: While these are typically unstated assumptions, these bullet points might be expanded to include (1) the fact that 3D effects are neglected (i.e. 1D radiative transfer, independent retrieval regions), (2) plane-parallel or pseudospherical atmospheres (not sure which it is), and (3) scalar radiative transfer (which is still an important assumption, even though MISR is not polarimetrically sensitive, e.g. Levy et al. 2004 doi:10.1109/TGRS.2004.837336 ). The assumption of modified linear mixing between aerosol components in the 74 mixtures should also be stated, if it is still used, since that leads to some additional uncertainty.

Re: We added the following two points to the list:

3. RT calculations assume plane-parallel atmosphere and neglect three-dimensional effects; a scalar RT is employed.
4. Aerosol types are assumed to be externally mixed.

P7 L2: I think it would be useful to point out here that the max(0.03,10%) envelope is the same as the GCOS goal for an AOD climate data record, so there is broader specific interest in quantifying how often this specific threshold is achieved.

Re: We added the following sentence:

“As such, statistical assessments were performed in several studies comparing MISR and collocated AERONET sun photometer measurements using the percent of MISR observations falling within envelopes representing the larger of 0.05 or 20% of the AOD, and 0.03 or 10% of the AOD; where the AERONET values were taken as “ground truth”. The latter envelope, max(0.03,10%), represents the target requirement for AOD defined by the World Meteorological Organization’s Global Climate Observing System.”

P10: the paper defines the DW acronym for the Dark Water algorithm on page 6. Elsewhere in the paper, such as the first paragraph here, the full name is used several times instead of the acronym. These could be swapped out for DW. This happens later in the paper too.

Re: Corrected.

Figure 1e: It would be good to change the rainbow colour scale used here, and instead use e.g. the viridis palette used in Figure 1d. The rainbow is not printer-friendly, greyscale-friendly, or red-green colour-blind-friendly, and emphasises certain change points in the data while hiding others.

Re: Modified as recommended.

P15 L 6: from email discussions while I was learning how to use the v23 product prior to the submission of this paper, I understand that the Aerosol\_Optical\_Depth SDS is already filtered to remove retrievals identified as poor quality using the tests detailed in later sections (e.g. ARCI). So for many applications

users can just read this directly without additional filtering. I did not see this stated explicitly in the manuscript (perhaps I missed it), but I think it would be useful to state here (or if I am wrong then state what filtering is needed here).

**Re: We changed the wording in this sentence to incorporate your suggestion:**

“In the V23 product, the **primary quality-screened** AOD field is named “Aerosol\_Optical\_Depth” and it is reported at 550 nm, to make it compatible with MODIS (Levy et al., 2013).”

Equation 2: Is this typeset correctly? Parsing this from general practice and the text my impression is “sigma is either set to [5% of the signal], or [the noise floor], whichever is higher”. But what is written in the Equation is “sigma is 5% of [the signal or the noise floor, whichever is higher]”. I think the factor 0.05 should be moved right, after the max() argument, before rho\_MISR(l,j). You’re not multiplying the noise floor by 0.05, you’re either taking the noise floor or assuming a 5% uncertainty. Or have I misunderstood?

**Re: The equation is correct as written. In the MISR algorithm the absolute equivalent reflectance uncertainty is 5% of [the signal or the “noise floor”]. However, in V23 the “noise floor” has been effectively removed by setting it to a very small number (signal is always larger than that number). We include the original equation to describe how V22 and V23 differ from each other.**

P18L19-15: If I understand correctly, if certain cameras have enough of a glint signal to estimate wind speed then this is used in the retrieval. I am not sure I understand the following sentence: “When the result is ambiguous due to limited MISR sampling of the glitter pattern, the minimum reflectance between model and observation constrains the wind speed in nearly all cases.” Can this be clarified? Does this mean the reflectance difference between model and observation equals the wind speed? Or the smaller of the model and observation reflectances? Or there’s some transform between reflectance and wind speed? Or something else? How does it constrain it? It would be useful to add a map showing how often these three categories occur: (1) provided by glint (which I assume is in the tropics and sometimes high latitudes); (2) provided by “minimum reflectance between model and observation”; (3) provided by TASC climatology.

**Re: Yes, the initial description of the glint-based wind speed retrieval used in V23 was ambiguous. We corrected and clarified the description so that it’s easier to understand. The new section reads: “As described by Cox and Munk (1954), the peak surface reflectivity decreases, and the angular width of the glitter pattern increases systematically with wind speed. Given the range of view angles observed by the MISR instrument, it is possible to constrain the wind speed from the MISR data itself in some situations. For example, Fox et al. (2007) investigated in detail the degree to which the nine MISR view angles capture the wind-speed-related glitter pattern under a range of wind speeds and observing geometries. This work formed the basis of a simplified wind speed retrieval introduced in the new version of the aerosol product.**

In V23, the modeled TOA reflectance,  $\rho_m$ , used for  $\chi_{abs}^2$  (Eq. 1) is pre-calculated at 3 discrete wind speeds:  $ws = 2, 5, \text{ and } 7.5$  m/s. At locations where no MISR channels are within the  $40^\circ$  glitter angle range, the algorithm selects the wind speed nearest to the monthly wind speed climatology from the TASC dataset. Otherwise, where at least one MISR channel is within the  $40^\circ$  glitter angle range, the algorithm selects the wind speed with the minimum equivalent reflectance difference,  $\Delta(ws, \tau)$ , between the model ( $\rho_m$ ) and the observation ( $\rho_{MISR}$ ), expressed as

$$\Delta(ws, \tau) = \sum_{l,j}^{4,9} v(l,j) \cdot \left( \frac{\rho_m(ws,l,j)}{\rho_{MISR}(l,j)} - 1 \right)^2 \quad (4)$$

where  $v(l,j)$  is 1 for channels within  $40^\circ$  glitter angle range; and 0 elsewhere. Note that the wind speed selected may be different for each aerosol optical model. The best estimate of wind speed reported in the V23 product is the wind speed selected for the aerosol optical model with the best fit according to  $\chi_{abs}^2$  and  $\chi_{maxdev}^2$  metrics.”

P19L20: If I understand correctly, based on P6 L1-3, v22 assumed water-leaving reflectance was zero, and Kahn et al (2005) was a sensitivity study (which used similar numbers to those eventually implemented in v23). So the numbers quoted here are not the previous V22 values. Is that correct? If so, I suggest deleting these numbers in case a reader thinks this describing V22, since the relevant information is 13 pages further back in the paper. I had to read it a few times to understand, and it is potentially confusing to give the sensitivity study's numbers, if they appeared in no version of the official product. If not, then more explanation is needed, because describing these numbers as "negligible" doesn't make sense given those on P20L1 (used in V23) are smaller still. If the numbers are not deleted then those on P20L1 should be written in the same format (e.g. "0.03" and "0.0257" instead of "0.03" and "2.57x10<sup>-2</sup>") as the formatting differences cause unneeded mental gymnastics.

**Re: Good suggestion. We deleted the mention of coefficients that were used in the Kahn et al. (2005) sensitivity study and simplified the description.**

"To first order, the reflection of sunlight off the ocean surface can be accounted for by considering only the effects of sun glint and whitecaps. However, research initially performed by Kahn et al., (2005a) showed that ocean color (i.e., water-body reflectance, or **underlight**) impacts could be non-negligible for aerosol retrievals using MISR radiances. **These effects** were first assessed in Limbacher and Kahn (2014). In that work, accounting for..."

P20L7: How many AGP surface types are there and what are they? I did not see this stated, but it would be good to point out since this section is talking about using a LUT indexed by AGP surface type.

**Re: We added description of the AGP surfaces:**

"The albedo contribution at a given location is set according to a configurable LUT indexed by **7 distinct AGP surface types: deep ocean, deep inland water, shallow ocean, coastline, shallow inland water, ephemeral water, and land**. For surface types corresponding to land or near land (including shallow water and coastlines), the albedo contribution in V22 is set to a constant value of 0.015."

Figure 2: Here "retrieval with AOD=0" and "overflowed but no valid retrieval" are both shown in white. These should be made distinct as they are quite different things. Perhaps the same grey used to denote "not overflowed", or a different grey, should be used.

**Re: We modified the figure so that the areas with no retrievals are shown in grey.**

Figure 3: Is this area-weighted? If so, this should be stated; if not, area-weighted would be a more representative global mean as equal-angle is quite distorted (as the authors discuss later).

**Re: Yes, the data is area-weighted. We added this clarification in the figure caption.**

**"Figure 1 (a) Time series of **area-weighted** annual mean AODs from V22 (blue) and V23 (red) between 2001 and 2016."**

Figures 4, 5: See comment about Figure 2. This is a bit worse because "not overflowed" also seems to be shown in white, and because large areas (especially in Figure 4) seem to be low AOD. So it is hard to tell what is low AOD and what is missing data. Also, a small thing, but the formatting describing the date and orbit/path number is inconsistent between these Figures (Figure 4 doesn't match in style with 2 and 5).

**Re: We modified the figures so that the areas that have no retrievals, either due to being out of field of view or due to retrievals being not attempted or successful, are shaded in grey. We also unified the captions.**

P26L13-17: PM was defined on the previous page but the subscripted size modes talked about here (2.5, 10) were not. I know what they are, but they should still be defined, or the text reworded to refer to PM in the general sense.

Re: We modified this sentence to read:

“Prototype versions of the 4.4 km MISR product have already been used extensively over parts of southern and central California and the current V23 product has been used over Mongolia to estimate **PM with diameter less than 2.5 micrometers (PM<sub>2.5</sub>), less than 10 micrometers (PM<sub>10</sub>)**, and speciated PM<sub>2.5</sub> concentrations (Franklin et al., 2017, 2018b; Meng et al., 2018).”

Figure 6a: Same comment about colour scale as for Figure 1e.

Re: **Corrected.**

P29L26: I noticed the Sayer et al (2019) paper cited in the text here is missing from the bibliography. Sayer, A. M., Govaerts, Y., Kolmonen, P., Lipponen, A., Luffarelli, M., Mielonen, T., Patadia, F., Popp, T., Povey, A. C., Stebel, K., and Witek, M. L.: A review and framework for the evaluation of pixel-level uncertainty estimates in satellite aerosol remote sensing, *Atmos. Meas. Tech. Discuss.*, <https://doi.org/10.5194/amt-2019-318>, in review, 2019. (To the Editor: this is my main current collaboration with the authors, outlined at the start of this review.)

Re: **Indeed, it was missing because at the time of submission it was not yet available for referencing. We added a reference to the current discussion paper.**

P32L9-11: I'm not sure I am convinced by this explanation. It is true that the retrieved AOD is lower in v23. But it is the true AOD which is generating the signal observed by MISR, and that is of course the same between data processing versions. So I would not have expected the true AE sensitivity to be different in this version. If I understand correctly, the AOD at 860 nm is determined from the propagation of each mixture's 550 nm AOD to 860 nm (based on its optical properties), and then a PDF of AOD at 860 nm is created using Equation 3 (i.e. the same as it is for 550 nm). So even if the AODs shift I don't know that the lower AOD would mean a depressed range of AE compared to v22. A decreasing AOD in that case might broaden the AOD PDF for an individual retrieval (as the cost function would be shallower) but should not shift the peak – and the ratio of the peaks is essentially what is determining AE. So I wonder if this might in fact be a result of the methodological difference in AE calculation between v22 and v23 (i.e. going from a fit to best estimate from several mixtures, to a ratio of peaks of PDFs). Perhaps I am wrong about this.

Re: **Yes, we agree that differences in methodology are likely the main cause of the AE differences between V22 and V23. We removed the last overly speculative sentence in this paragraph.**

P33L9: In contrast to the above, I do agree that lower retrieved AOD over ocean in v23 could mean noisier SSA, because the SSA calculation method is the same as in v23.  
(No response is needed for this comment.)

Figure 10c: over the open ocean, it looks like SSA is higher in v23 in the northern tropics and midlatitudes, and lower in v23 in the southern tropics and midlatitudes. (Excluding the polar regions and dust outflow.) Figures 7 and 9 show that AOD and AE changes don't have this hemispherical difference. I therefore wonder what could be besides the hemispherical difference in SSA? Perhaps this is the

interaction of calibration differences between the data versions, scattering angle differences between the hemispheres, and (real) aerosol type differences between the hemispheres?

**Re:** It's an interesting observation. We noticed it initially but decided not to explore it due to the generally weak SSA signal over oceans. Notice that the hemispheric contrast is mostly apparent over the Pacific Ocean, not so much over the Atlantic. Furthermore, this contrast seems to originate from the V22 results, which show that the northern hemisphere Pacific has lower SSAs than the southern hemispheric Pacific. In contrast, the V23 results don't show this hemispheric difference. The northern hemisphere mid-latitude SSA signal in V22 possibly reflects long-range pollution transport from Asia over the Pacific towards North America, and from North America over the Atlantic towards Europe. In V23 this signal is not evident. However, V22 also shows decreased SSAs in the subtropical north-east Pacific, which are more difficult to justify. Overall, interpretation of SSA values over oceans where AOD values are relatively low is challenging. We therefore decided to refrain from overly speculative explanations.

P35L11: Yes, I agree again that this could be the reason for nonspherical fraction. It's just for AE where I am not convinced that is the case. (No response is needed for this comment.)

P41L9: While it is kind of the authors to cite me here, the rationale for interpolating to 550 nm that way came from Eck et al (1999, <https://doi.org/10.1029/1999JD900923>) and Schuster et al (2006, <https://doi.org/10.1029/2005JD006328>). It is correct to point out that I did it that way, but I think it would better to cite the earlier Eck/Schuster papers here so the reader knows why it is better to do it that way.

**Re:** We changed the reference to Eck/Schuster as suggested.

P44L14: the point about uncertainty in AE being high when AOD is low, for surface observations as well as satellite retrievals, is important. I suggest adding a citation to Wagner and Silva (2008, <https://doi.org/10.5194/acp-8-481-2008>) here who provide numerical experiments into AE uncertainty from an AERONET-like system. It helps make this point clearer to the reader.

**Re:** We added the reference as suggested. The revised sentence reads:

“This suggests increased uncertainty in derived AE values when AOD levels are low, which applies to both surface-based observations (Wagner and Silva, 2008) and MISR retrievals alike.”

P45L17: I suggest replacing the word “unprecedented” with a term like “a much finer”. I agree that going from 17.6 km to 4.4 km pixel size is a great thing and MISR's information content means that it offers more than some other sensors can. But there is definitely precedent for retrievals at that scale or finer already; for example, the MODIS Dark Target 3 km retrieval, the MODIS MAIAC product at 1 km.

**Re:** Replaced as suggested.