

Interactive comment on “Introducing the 4.4 km Spatial Resolution MISR Aerosol Product” by Michael J. Garay et al.

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I am posting this review under my own name (Andrew Sayer) as I know several of the authors quite well (and work at the same institution as Kahn and Limbacher), and have recently collaborated with them. I feel I am able to provide an unbiased review of this study.

This is an overview paper describing the new version 23 (v23) MISR aerosol products (both level 2 and level 3, though mostly level 2). It integrates work from a half-dozen or so other papers published in recent years by the team describing individual updates to sensor calibration and over-land/water algorithms for v23. The main new contributions here are (a) putting all the pieces together; (b) providing additional technical details

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and acting as a user guide; and (c) providing some examples and analysis showing how the new v23 results differ from the previous version 22 (v22). This last point is important because a large number of papers have been published on the basis of v22 results over the past decade or so, and it's important to see how v23 is different. While a data set overview paper is not necessarily exciting, it is work that should be read by people wishing to use MISR aerosol data products in order to understand their structure, strengths, and limitations.

I feel that there is sufficient novelty to warrant publication; the article is within scope for AMT, and is well-written overall (both in quality of language and scientific soundness). I recommend publication following minor revisions, and would be happy to review the next version if the Editor finds that useful. I have the following comments. Here PX, LY means page X, line Y.

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.

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.

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.

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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.

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. ARC1). 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).

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(i,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?

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

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TASC climatology.

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 “2.57x10⁻²”) as the formatting differences cause unneeded mental gymnastics.

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.

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.

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).

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).

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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.

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

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.)

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.

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.

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(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?

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.

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.

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.

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