

Interactive comment on “Implications of satellite swath width on global aerosol optical thickness statistics” by P. R. Colarco et al.

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Overview: This is in principle a very straightforward paper; the authors wish to examine how long it takes the annual globally averaged AOD for different swath widths to converge to 0.01- a benchmark put forth by several prominent climate scientists. Their approach has been to use 8 years of MODIS Aqua MYD05 AOD as a proxy, isolating various portions of the swath to mimic the coverage of more limited sensors such as MISR or curtain sensors such as was destined for Glory. Because the 0.01 AOD metric does not leave much tolerance for any view angle dependent biases, the study begins with an assessment of the potential for systematic retrieval or diurnal biases, which over water they claim to be quite substantial- over 0.07 AOD difference between -20

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and 60 degrees over water. They report this bias is largely a natural zonal coverage issue. Over land, the angular viewing bias is much reduced with that which does exist (0.04 difference between -40 and 60 degrees) is reported to be associated with the retrieval. They correct for these characteristics and ultimately find that narrower swath instruments, despite potentially having higher information content, are likely to under sample the true environment to such a degree that they cannot directly monitor global AOD and its associated climate forcing.

Review: I think I understand where the authors are coming from. The 0.01 tolerance in globally averaged AOD has been batted around in climate circles for some time. Both climate and weather scientists alike must deal with the information versus coverage trade space. You can have good global coverage ala VIIRS with ok information content, or you can have claimed high information content but at diminished coverage. Assuming a perfect instrument and aerosol state variability, how long does it take for various swath configurations to converge? Can this convergence occur within a year? Within a season? This is a fair and pressing question. Let's call a spade a spade, shall we? With hard choices being made for the upcoming ACE mission, this is a politically charged topic with a lot of money on the line. While the point of the paper is very well taken, and even most likely correct, the presentation is a bit like bringing a pocket knife to a gun fight.

The question as posed however, despite the fact that current technology, sampling issues, or even simple definitions (is it an aerosol particle or cloud?) neglects the simple fact that this 0.01 number cannot currently determined to several integer factors greater than this (I not only to MODIS and MISR, but what I expect the capabilities of the ill-fated Glory mission would which in my opinion has been way overblown with little supporting science). Plus, a spatially average global AOD number does not even account for seasonal/zonal solar illumination or clear sky issues. Indeed, aerosol impacts and feedbacks on the earth system are likely more local and regional in nature than uniformly global. In my opinion the global 0.01 AOD benchmark is a false god.

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In regard to the specifics of the paper, I find myself largely in agreement with the previous reviewers. In fact, after the first reading a few days after the assignment I did suggest to the author that perhaps a withdrawal would be appropriate to give him time to address some shortcomings. It is therefore not expedient to repeat these comments. However, there are a few areas which I think require emphasis. I also have some suggestions which might improve impact of the work. Regardless, I do wish to encourage future work along these lines. We at NRL have done these calculations, but have not formalized it. I am glad someone is taking the time to do it.

First and foremost, in an effort such as this the criterion must be very well laid out. In my opinion the last sentence of the abstract is a non-sequitor. Indeed, as noted above, the standard which is set forth is ill defined and the specifics of the application of any data can go a long way of sequestering bias, despite the field-wide bravado about some benchmarks. Further, the authors should be clear in the definition of climatologies of extensive (e.g., AOD) and intensive (e.g., single scattering albedo) parameters. Anyway, let us continue specifically on the impact of sampling on AOD.

Given the issues surrounding the AOD metric, I would suggest tackling the aliasing problem head on and in a complete manner. One need only compare modis and misr at the monthly, seasonal and yearly level in NASA Giovanni to see the impact of narrowing swath on AOD. One could also incorporate CALIPSO if a mean bias correction were provided (One could simply use Aqua MODIS AOD along the CALIPSO track). This should be figure 1 to prove the point. As an aliasing issue, the following work should be treated with a formalism similar to any signal processing problem.

Third, I fully agree with the previous reviewer that this should be done in model space where the environment and sampling can be fully controlled. Given that GEOS 5 now has data assimilation capability, I would recommend following NRLs protocol: performing the sampling off of the 24 hour aerosol forecast fields with reanalyses meteorology (e.g., Zhang and Reid, 2009). Then use that next day's combines Terra/Aqua modis retrieval location's to get a global map of simulated retrieval locations.

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Forth, while I understand why the authors want to do this first attempt with MODIS data, the very act of aggregating to 2x2.5 degrees looses the benefit of using real data in the first place-hence my previous comment. If the authors wish to continue down this road with this paper, then I whole heartedly agree with previous reviewers then the corrections to the MODIS data should be done with great care, and a baseline unmodified product also be shown. Unless you can fully resolve the differences in the AERONET-scan angle relationship then it is inappropriate to continue with the experiment. Indeed, you should not perform a global correction by what you admit might simply be a simple latitudinal sampling dependence. As I corresponded earlier to the author, we at NRL do not see such a strong dependency, and that makes me fairly suspicious. We used over an order of magnitude more data in our error estimates than in the cited Remer and Levy papers. I would suggest the authors here re-read the Hyer, Zhang, and Shi papers listed below. Indeed, Hyer clearly showed that due to increasing pixel size and improved signal to noise, MODIS retrievals are better at the edge than at nadir. The fact that they have three curtain samples in the analysis does go a long way to sequencing this bias in their "thumbs up or thumbs down" hypothesis. But, this can be presented in a much clearer way

All of this previous point said, it probably does not change the underlying result if the data is presented the right way. Peter pointed out to me that he did not see a large difference between uncorrected and corrected. I think this is because they are applying a linear correction to the different swaths. As such, one could simply normalize it out. Perhaps in the plots, doing the simple subtract the mean and divide by the standard deviation trick should be performed to see if variances are matching in the same location-this would be a good indicator as to the total amount of the variability in the signal you are capturing. The fact that they are getting convergence in the global number over ocean is a good sign-so there is likely nothing terribly wrong. Zhang and Reid (2010) showed that at the global level over water, MODIS and MISR monthly anomalies were very similar once MODIS calibration issues were taken care of. If there was a large systematic differences across swath in the Colarco paper, there is no way they

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could get such convergence.

I do have a number of minor comments, but it is likely not worth going into too much detail here. Certainly the figures could be modified for impact. For example, for Figure 5 (c) I would also have a figure 5(d) where the differences are not booleans. I had a very hard time reading figure 6. Figure 7 should be broken down not only into a 9 year average, but also several 1 or year comparisons should also be made to help address the issue of trends.

Finally, I would recommend the authors taking a step back and thinking about how people can use the results of these efforts. Simply concluding that narrow swaths are generally unhelpful-I already know that. Rather, more comprehensive sampling curves should be generated. Given real world meteorology, how long does it take for sampling pattern 1 to converge to the larger sampling pattern 2? The issue of narrow swaths will be a recurring problem in the future, as all sensors outside of VIIRS will lack global coverage. European sensors in particular tend to be narrow swaths, and a general treatment of the problem will likely be appreciated. One could even use GEOS-5 to directly study the aliasing in MISR relative to MODIS and determine if MSP1 would have sufficient width.

Good luck.

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