**Atmospheric
Measurement
Techniques
Discussions**

Interactive comment on "The Collection 6 MODIS aerosol products over land and ocean" by R. C. Levy et al.

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Recomendation: Major Revisions

Overview This is an overview paper on the much anticipated MODIS data collection 6 (Col 6) aerosol retrieval. The paper is long, outlining numerous changes to the algorithm, with additional appendices in the back outlining specifics. Given the importance of the algorithm and its documentation, I heartily endorse the publication of a paper like this in the open access peer reviewed literature. Too often such documentation has been buried in hard to find reports. However, for publication, I would recommend the addition of further algorithm information as supplemental materials.

Science wise, it appears that Col 6 has attempted to address a number of historical

trouble areas for the algorithm. First they are working on cirrus contamination over land and water. I am grateful that the MODIS team finally acknowledges that the band of high AOT around the high-mid latitude oceans is largely artifact, and that they have apparently done much to improve the algorithm. Lots of modelers erroneously tuned their sea salt emissions to match this artifact; I hope that more reasonable sea salt numbers will follow. A second major improvement updates the optical models so that, from broad global plots, it appears that some of the consistent biases in Central Africa and East Asia have been improved. Other important improvements include an a) update to the Rayleigh/gas absorption treatment; b) the extension of the latitude of retrieval; c) revised high AOT-cloud mask bias to allow retrievals over thick aerosol plumes. The recently developed 3 km MODIS product is also described, as is the joining of Col 6 dark target and deep blue algorithms for a "global" product.

An important consideration regarding this manuscript is that it does not address verification in any rigorous fashion. No doubt there will be several papers from within and outside the MODIS team that will appear in the next year after the product introduction. This paper merely describes the algorithms and demonstrates many large differences from previous versions. I don't have any direct objection to this course of action in particular, but that said, as in previous version updates, the algorithm has been updated based on the examination of relatively few data points. Within this paper, the authors demonstrate a monthly average for January and July, but omit the boreal spring and fall which tend to coincide with Asian dust and pollution outbreaks and monsoonal biomass burning seasons. So, based on this paper, we don't ultimately know how Col 6 is going to behave in key regions and time frames. Again, given the importance of the Col 6 algorithm to the earth science community, I would add additional plots for the spring and fall or, as we at NRL have done, break data down into the natural monsoonal aerosol periods of Dec-May, June-November.

One area of great interest is the changes to the calibration. It has been clear to us for some time that there have been instabilities in the MODIS radiance calibration. I know

that part of the delay in getting Col 6 out has been to give the calibration team enough time to sort out trends in the calibration drift. But, the paper punts on this issue.

Specific comments: 1) First sentence of introduction. I was surprised that the MODIS aerosol team chose not to use the accepted definition of "aerosol". Please reference the AMS glossary of meteorological terms and use it to correct the text in the introduction. I would also not say (next sentence) that "aerosols drive the hydrological cycle" as, personally, I think water vapor has something to do with it. But you can say that aerosol particles are an important component of the hydrological cycle. I make these points not to be snarky, but the paper does appear to incorporate a lot of grandstanding. Also keeping with past practices, the aerosol team shows an unfortunate propensity to cite their friends, to the exclusion of other more relevant work. Certainly, when outside work is cited it tends ot be in a modest manner. The authors and developers for Col 6 should leverage this extensive body of knowledge. Col 6 is a flagship product for NASA. They need to be balanced. The body of the NRL/UND work, which has included the most rigorous global verification analyses, has been largely unreferenced except in token ways, and it is unclear from this document whether the aerosol team has learned any lessons from us on bias correction.

2) Section 2: I very much appreciate the authors reiterating the MODIS history and "fun facts" on the nature of product levels, pixel size, retrievals etc... in a more conversational way. It seems that users often forget these issues, and it is good to repeatedly point them out. However, in this section there is reference to the previous ATBD. Is this work in lieu of a new ATBD or will a new ATBD be generated? I think it would be helpful if the authors could note what we should expect.

3) Page172 line1-10. This is not exactly our point of view. Bottom line was: yes in fact we can assimilate (which don't get me wrong is a huge achievement), but we do that by largely throwing out 40% of the data points and making, at times, 30% bias corrections. In the next paragraph, it is pointed out that perhaps not all sites perform equally, which comes off as a waffle. Instead, at this point the authors should simply state the facts.

That is, over 30-50% of the world, the MODIS algorithm performed quite well. It was certainly never "comparable to sunphotometers" as that would suggest parity, which no satellite product can achieve. But, the data was clearly within your pre-specified error bar. Another 30% of the data were ok if used with caution and with bias correction, but 20% of the data were utterly unusable. Recall, a bulk comparison against AERONET favors those sites where there are lots of sites and lots of data. From Shi's paper, we know those sites are where MODIS performs well. However, there are large tracks of land with few sites, and there MODIS tends to perform poorly.

4) Section 3: As mentioned in 1) it seems as if the aerosol team largely did not learn from the NRL/UND over-land bias correction work. If we can do an outside empirical correction to help alleviate such issues as microphysical bias and lower boundary condition, (which even we admit is a gross hack), why can't they improve the algorithm? I understand that they want to have a single scene retrieval, but if using external surface reflectances (like Hyer uses) improves the retrieval, why not do it? The argument of a "physical retrieval" does not hold, as the authors admit that there is no value in the over-land size information. The authors cite 'data volume issues' preventing making adequate corrections, but Moore's law has been in our favor. If the PEATE can reprocess a year of MODIS data in a week, why can't GSFC, with all its computational resources, mine the data? We also noticed that snow bias is never mentioned, although we have seen this to be a problem. With the increase in the latitude of retrieval, the authors must respond to the question "what is your assessment of snow bias working its way into the climatology?"

5) Section 3.2.4 There has been much evidence that the QA flags are not really QA flags, but reflect "goodness of fit", which does not actually correlate that well with data quality against AERONET. This should be discussed.

6) Section 3.2.6: Two months of data are insufficient to illustrate the differences in the old and new algorithms. Period. I fully acknowledge that the paper is not a validation paper, but if they want to demonstrate differences, presenting in this way is clearly

non-representative and possibly misleading. As it stands, I think there is considerable framing bias in the bulk statistics (although I very much appreciate the global difference plots). If the authors want to cut the number of plots and tables, then choose 1 year and verify on the more meaningful Dec-May and June-Nov time frames. If the biggest AOTs are in the spring and late summer/early fall, why are these periods not presented? January and July are pretty tame AOT wise (not many large events). In this light, Figure 7 has little meaning; while certainly the number of points within compliance is functionally the same, now the algorithm exchanges low bias for a high bias. I would prefer the authors present RMSE and RMSD as a function of AOT for the two algorithms, as this is much more robust and less subject to misinterpretation than presenting a correlation coefficient (I see they used r and r^2 for obvious reasons). Also, the authors need to show data for major global regions, (most of the data currently presented appear to be from US and European sites where the algorithm works well), and plots normalizing RMSE by region would have great value in displaying the changes from Col 5 to Col 6.

7) 3.3.1 Sentence 1: I am glad the wind speed correction was included. I appreciate the plot of 1 day in July, but please add the monthly average of this particular correction to give context with the other data presented. It would be clearer if there was one large figure showing the baselines, and then an associated cage for each correction that is made (surface, microphysics, cloud etc.) with a final total correction at the bottom. Minor comment, it is Zhang and Reid, 2006, not 2011, that noted the surface wind error. Similarly, the cloud bias issue listed in section 3.3.2 is also from the 2006 work. For collection 5, the work is dealt with in detail by Shi et al., 2011.

8) Section 3.3.3, I do not wish to be petty, but I want to point this out. When it comes to verification work you tend to cite your fields when in fact, collectively, our group has been reiterating to you for over 6 years that the QA flag does not correlate with verification. We have also been the ones pointing out (through our prognostic error modeling), the asymmetry in error, and why, if MODIS reports a clean AOT, it probably really is clean. What you fail to recognize is that these issues will lead to a representativeness

bias when you construct a climatology. We have prognostic error models for "good" and "very good" data, and we use both categories of data. However, generally the MODIS team recommends that people only use the "very good data". Through your formulation (and your weighting mechanism in section 4), you have probably biased your climatology.

9) Section 3.6. I am not sure doing an "average" of DB and DT is the best approach here. I would think based on internal machinery doing one or the other based on an error model would be the way to go.

10) For the level 3 data, I suggest the algorithm team include the median and geometric standard deviation. In a 1 degree box on a single day this is unlikely to be useful, but in the monthly+ products, this is a much more robust way of describing the AOT. Also, while the authors correctly note that pixel weighting can bias towards a sensor, they should recognize they have the very same issue in verification against AERONET.

11) Section 5-not much to say here. The paper sort of punts the topic to (potential) later documents. But that, I think, is okay as the 3 km product in some ways is a different beast.

12) The lack of long term trend analyses and descriptive statistics in this paper on the calibration issues, which from a climate trend point of view is extremely important, is a significant shortfall of this paper. In order for Zhang and Reid (2010) to determine the size of the MODIS calibration bias and remove it, it took nearly the entire data record. I do not think that testing on a few months of data is going to characterize the calibration change over time. There may be a baseline change, but for trends, you will have do as we did, and engage in more rigorous long term analyses.

Hope this review helps. I think I can speak for many and say that the MODIS AOT retrieval ids one of the greatest achievements in the field. But, with a little more effort, its impact can be even greater. Jeffrey S. Reid NRL Monterey

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