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Interactive Comment

Interactive comment on "Validation and empirical correction of MODIS AOT and AE over ocean" *by* N. A. J. Schutgens et al.

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This paper is basically a reanalysis of the MODIS col 5 AOT verification studies conducted by the NRL/UND group. The primary difference, as they state is that they have made some effort to cope with the spatial correlation of data, and the angstrom exponent (whereas we have looked at fine mode fraction). Getting down to brass tacks, as a whole the NRL/UND groups welcomes follow-on analyses such as these. Certainly verification studies demand independent verification studies themselves, and there are numerous way to characterize error and bias-often related to particular purposes. In regard to this particular effort, I am not sure any key finding is in any way different than our previous works, but the statistical processing is very different. For a bottom line up front, with a little effort this paper could answer the most pressing question I had after giving it the first read: At the end of the day, did they find anything that was substantively different in the way that MODIS AOT error should be represented compared to what we did? As written, it is very hard to tell. I do have lots of small comments on the paper, which I can communicate directly with Nick as needed, but I expect this paper is going to require numerous revisions and recalculations in order to make it suitable for publication. Here are some major points that need to get considered.

1) I do take exception to Nicks statement's in the paper that compared to our papers, this present analysis is "more complete" and "better performed" (I do agree it is an extension). Ultimately, what we have is a difference in point of view as to how our error models should be constructed. What they have done is more complex, but after reading this paper I would not change the way we operate-namely we look at multiple retrievals against a single AERONET measurement. And I certainly would not agree that they performed a more robust analysis-although it does address a few interesting issues. We have personally explained our point of view to Nick that there is merit in this method in that we want to understand the regional variability around a single site. Has is shown in the paper, the correlation length of most aerosol features is well in excess of the 50 km rang ring that they and we use. Thus, variability within that range ring has meaning from a retrieval noise point of view. Error is error, and we will build up or stats any way we can. Now, the authors have a point that we need to be careful that a single site or good clear days do not bias the sample-we agree. But the way we have dealt with this is by adding dimensions to the error model. Over water, the dimensions are fine/coarse mode partition (which this paper does not address and hence cannot be called more compete see comment 4), plus wind, and cloud cover. Over land (contrary to what is stated in this paper), Hyer does in fact perform bias correction based on albedo, view angle and region.

2) The statistics presented (and in particular the plotting axis) are at times poorly defined and unclear. There are 23 plots and little synthesis. The presentation as a whole

in fact is unclear, with statements like "we can make a correction" and then we are referenced to the appendix. Our corrections are simple and on par with the real uncertainty of the system. I am curious how they came to their conclusions think these require more explanation. In their appendices they should define all variables. In many of the plots, "MODIS error" is listed. But really what I think they mean is mean bias. But is unclear what it is they are really presenting. The authors may want to go through these. Second, how they construct error estimates appears to be mean absolute difference, whereas most folks utilize root mean square error. Also in this paper, they look at the error distribution and the calculation of a median, which is fine if you want to map the distribution of error. But for our application, data assimilation, we need to use MAE and or the RMSE (or if the bias is known, the RMSD). Median is the most likely value, but the mean is the most representative. What is really mucked up here is that the standard deviation of their histograms is the RMSD, so why not just provide it? Perhaps the mean bias and an RMSD can be provided in a table across several dimiesions?

3) While diagnostic error is useful, what we really need is prognostic RMSE. This is an important point and one that should not be overlooked- everything we have done has been in the context of data assimilation. There is no way that we can take the error models presented in this paper and apply them to our data assimilation problem. What would be wonderful is if the authors could calculate RMSE as a function of MODIS AOT and compare that to the numbers in Yingxi Shi's paper. Here we have to use RMSE (over RMSD) because after bias correction, we don't know what the bias is! If we did, we would correct for it-hence in the face of the unknown it has to be RMSE. I really want to know if by their sampling if they get a different number. We can also provide the authors with the mean AOT data from our DA grade product, and they can demonstrate a difference.

4) Going through the paper, it strikes me there are some subtleties on remote sensing retrievals that might be missed by the authors. First and foremost, much of the

bias we found was microphysical, and that could be corrected by application of a correction term based on the retrievals own fine mode fraction. Thus, when you show global statistics, and have a mean bias near zero, much of this is a result of offsetting penalties from fine and coarse mode dominated aerosol airmasses (again, this analysis is not more complete than what we did). Second, choosing as an example AOTs for above 0.5 is not representative of the global marine atmosphere. The authors may think they are doing the retrieval a favor by using higher AOTs and thus better signal. But for AOTs>0.5 one enters a multiple scattering regime, hence errors multiply. For comparison against AERONET and why some sites "don't work out" it is both error on the MODIS side plus non-representativeness on the AERONET side. For example, sites like Coconut Island are impacted by upslope winds near the island of Oahu, and hence higher boundary layers and higher AOTs than just a few kilometers out to sea. It is for this reason in part that the site was later pulled. You mayy want to talk to the AERONET guys abotu yoru findings. Soem of yoru flaggged sites looked ok to us too as long as we dont use coastal retreivals.

Anyway, these are my major objections. Nick, feel free to contact me directly if you want to chat about this. Be well, Jeffrey Reid.

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