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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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Response to Edward Hyer's comments,

Thank you for commenting on our paper and the subsequent email discussion we had. The suggestion to add more detail on statistical methods is very useful to us and we have done so. Please find below our responses to your questions.

Q: The figure 1 labeling indicates that the statistic being shown is a rank correlation. For a continuous quantitative variable like AOD, why is a rank correlation being used here? This is an example where a different choice of statistic complicates comparison with other recent studies (for instance, correlation length of AOD is also the subject of a recent ACP paper by Shinozuka and Redemann: http://www.atmos-chemphys.

net/11/8489/2011/acp-11-8489-2011.html)

A: Thanks for bringing this paper to our attention. I (Nick) was aware of the work by Shinozuka through a conference talk but have somehow missed the publication.

We agree with you that it is sound practice to use metrics that can be compared among papers. But it is also important to try out different approaches and see which works best before deciding on a common method. We chose the rank correlation (and the median and quantiles in general) because these tend to be less affected by outliers in the data. MODIS-AERONET AOT (Fig. 17, revised paper) has a near Gaussian distribution but broader wings due to these outlier. A normal mean, standard deviation and (Pearson's) correlation coefficient may be unduly affected by those outliers (see Fig 17 for the effect on standard deviation). Spearman's rank correlation is appropriate even when the variables are continuous. If the variables are linearly dependent but with Gaussian noise, Spearman's rank correlation and Pearson's correlation are expected to give similar (but not identical) results.

Turning to Shinozuka & Redemann 2011, their Fig. 3 is similar to our Fig 1 but for 1) different correlation coefficient; 2) different dataset. We use a more or less global dataset, while Shinozuka & Redemann use campaign data for two regions. We believe especially (2) makes it difficult to compare results. Nevertheless, our correlation coefficient seems to be similar to their curve for remote regions. There does not appear to be any conflict.

Q: "correlations in the full dataset will suppress the biases", and later "Cloud-free scenes allow more succesful retrievals (more co-located pixels) than cloudy scenes." The second is a more proximate explanation for the differences seen in Figure 3. Is having more retrievals under certain conditions an auto-correlation effect? I guess what I am saying is, the principle of correlations suppressing biases makes sense, but what you are actually observing seems to be a sampling effect. If differences in sampling alter the statistics, then it seems to me that systematic variation in uncertainty is present

AMTD
6, C1577–C1580, 2013

that needs to be diagnosed and hopefully corrected. In the case of clear and cloudy, you have taken this approach by including cloud fraction in your empirical correction. In your estimation, is this sampling difference a correlation effect?

A: There are two aspects to this. First, because of the spatial correlations in errors, it is not beneficial to use all MODIS observations that collocate with a single AERONET observations. Sure, you will increase your sample size but it will not contain independent information. Second, if you do use all observations you 'stack the deck' in favour of clear scenes that naturally allow more co-locations per AERONET observations.

This second aspect is indeed a sampling effect, while the first is a physical effect. The second aspect is not an auto-correlation effect (at least it has nothing to do with spatial correlations in the aerosol field).

Q: The sampling used to estimate uncertainty should match as closely as possible the sampling of the bulk dataset. You have developed corrections based on several observation characteristics that affect both AOD retrieval error and probability of retrieval success (sampling). If this correction is successful, one effect should be that the sampling-related differences in statistics shown in Figure 3 should diminish after application of corrections. Did you observe this?

A: This is strongly related to the previous question. I agree that you want similar ranges for the important variables (latitude, longitude, time, AOT, cloud fraction, wind speed etc) in any sub-sample. I also agree that if there are e.g. more clear AERONET observations than cloudy one, this should be reflected in any statistical analysis. Our method of sub-sampling does not violate these conditions. The only purpose of the sub-sampling is to create a dataset of independent (see answer above) MODIS-AERONET data pairs.

Q: Interesting results on matching the observed distributions to equations based on the interquantile distance. I am impressed by the quality of fit achieved, but I do not understand how these synthetic distributions are generated. Perhaps an equation might

AMTD
6, C1577–C1580, 2013

make this clearer.

A: We have modified the text that explains Fig 17 (revised paper) in the hope that it is more readily understood. The distributions are Gaussian with standard deviations that were estimated in two different ways. The first method uses the standard deviation of the error distribution. The second method uses an interquantile range (our random error).

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AMTD

6, C1577–C1580, 2013

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