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Atmospheric Measurement Techniques
Discussions

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 reviewer 1

Based on the comments of the reviewers, we have now included in the introduction explicit definition of our statistic metrics (e.g. median as a bias) and why we did so. We have also substantially altered the text of those sections that pertain to our methodology.

We thank the reviewer for his or her comments. The reviewer concludes that "This paper is well suited to AMT. It does show original results that may be of interest to the community. What is done is well described so that the objectives and results are clear."

The reviewer identifies a few issues with the paper: 1) the paper contains several (?%) that were not filled in before submission; 2) too many figures that dilute the paper's main message; 3) lack of a proper interpretation of some results.

1) The three missing values in Sect. 3.1 are now filled in. 2) We have removed two figures and rearranged 2 more. Captions have also been improved (we hope). 3) Text has been added in various places to provide more interpretation. At the same time, a proper interpretation of some results would require a study in itself and we feel it is outside the scope of this paper. We come back to this in our detailed response to the reviewer's comments.

Response to detailed comments by the reviewer

P3767-L7 known => suited

We believe that MODIS provides for an incredibly useful dataset of observations, irrespective of any errors that have been identified by ourselves or others. Whether they are the dataset best suited to aerosol studies depends very much on the application. We suggest to retain 'known'.

P3767-L9. Contrarily to what is written, Terra is NOT part of the A-Train

Agreed and corrected.

P3767-L21 "significant agreement". Too vague statement

Agreed and text modified

P3767-L26: "Systematically". Not true. It may tend to overestimate, but it is certainly not systematic

Agreed, we were thinking of bias but the random error component ensures that it is not systematic. Text modified.

P3768-L17: It should be mentioned that the big drawback of AE, is that it has no meaning as the optical depth tends to zero. It is very obvious that its noise gets large for small aod, which is not the case for the fine mode aod or the aod at another wavelength (or the aod difference between two wavelengths)

It's true that the error in AE becomes large for small AOT (see also our Eq. 3). So does the error in the fine mode fraction. The fine mode AOT is an amalgamation of abundance information (AOT) and size information (fine mode fraction or AE). Obviously, its error is small when AOT is small (at least the error will not be bigger than the AOT error itself), but that does not mean it is the better parameter to assess size at low AOT. Note that the error in fine mode AOT does not necessarily become small when fine mode AOT itself becomes small. Ultimately, these three combinations (AOT & AE), (AOT & fine mode AOT), (AOT & fine mode fraction) should contain similar information.

P3770 L3 : Provide the spatial resolution of the product

Done.

P3770 Eq1 : I believe this equation was not finished writing.

The equation was correct but alpha was never defined. Corrected.

P3771 section 3.1 : Several "?" indicate that the paper was not finished editing when submitted. This looks bad.

Apologies, this has been corrected.

P 3771-L26 : I do not understand the threshold at 1 times the typical value. Why remove cases that have typical values for the gradients. This may remove a large fraction of the data.

This criterium enforces a certain degree of homogeneity on our final sample and in this manner hopefully discards observations that have been contaminated by clouds or so. It is essentially a sanity check of the known fact that aerosol has correlations over long distances ($\sim$100km). The factor 1 is arbitrary, but Zhang & Reid and Shi et all

use a very similar criterium. We have added a bit more explanation to the paper. This criterium results in 14% loss of data.

P3772 Section 3.2. I fully disagree with this section and the example that is given to explain the process seems poorly chosen. In this paper, two fairly independent measurement of the same parameter (the AOD) are analysed. On the other hand, the example that is used uses non independent data of different parameters (ie the AOD at different times). If the biases are estimated for bins of the aeronet AOD, one should not expect a bias in the MODIS AOD. This is an important point in the paper that I would like the author to address carefully

In this section we try to argue 1) that co-location can 'create apparent' biases (since co-location is never perfect but always allows for some spatio-temporal separation); 2) that such biases are unimportant for our study. Although this appears to be a new insight (we have not found any discussion of this effect in earlier papers), it also is not relevant (because the effect seems small). We have removed this section. Note that in the submitted paper AERONET data were only used to assess the magnitude of this particular effect. No comparison against MODIS was made.

P3773-L16: Why 142 km ??? If the two MODIS pixels are less than 50 km from the aeronet site, it seems obvious that their distance is less than 100 km

Indeed, it is a mistake. Text has been modified.

P3773 L23-26: Not clear. Please reword.

Agreed. This section was substantially rewritten. In particular, we have added more interpretation of the results and made the link to the main issue of the paper clearer.

P3774, first paragraph. There are some very surprising (almost impossible) results described in this section, and the authors make no effort to interpret them. It seems impossible that a random sampling leads to a statistically different bias than that obtained with the full dataset. Besides, the authors make a discussion that involve the different biases between clear and partly cloudy scenes but fail to mention that the bias of both types of scenes are larger than for the “all cases”. Both of these results seem impossible and impose some fact checking and explanations.

We thank the reviewer for pointing out the poor explanation in this section. Nowhere in this section do we make the claims that the reviewer thinks we make. We have therefor substantially rewritten this Section and hope it has improved. In particular, the ’clear’ and ’cloudy’ cases do not together form the ’all cases’. Also, we agree that a mere random sampling leads to the same bias (within statistical noise) as the original dataset (this can be seen in Fig. 2 (revised paper) by comparing the results for ’all’ and ’random’ cases).

P3775-L8-9. Covariation of to and AE may be purely physical. The largest AOD over the oceans are mostly desert dust events, and are therefore associated with small AE. This is one of the case where more interpretation of the results would be expected.

We agree with the reviewer that AOT and AE can covary for purely physical reasons. But here we were talking about the covariation of AOT bias and AE. To the extent that this bias seems retrieval-driven (and not sampling-driven), this suggests limitations in MODIS retrieval algorithm that are caused by AE assumptions. Hence our conclusion. We have reworded this sentence.

P3776-L7: : : : for the scattering angle. What is the parameters that co-varies with the scattering angle?

We have rephrased this. We meant to say that solar zenith angle and viewing zenith angle co-vary to a certain extent with scattering angle (due to the orbit).

P3776-L17: why jump to Figure number 21 ?

This is a layout mistake and has been corrected.

P3777-L2: [: : :] outside the scope of this paper. Not clear why

We would have preferred to understand this better ourselves, We set out to describe MODIS errors and, when possible, develop correction formula. As it is, the paper is already substantial in size and we feel we have accomplished our main goal.

P3778-L18-20 : Low correlations for a few sites. All sites listed here are located in very clean regions and the AOD remain extremely small (<0.1) all year long. As a direct consequence, the random noise of 0.05 on the MODIS AOD leads to poor correlations, even when the RMS error remains small. This is a clear indication that the correlation is a poor indicator of the AOD retrieval performance. There is no good reason to discard these sites. I am rather surprised that the author have not identified this cause and they cannot claim that there is a “big discrepancy between those sites and aeronet”.

Contrary to expectation (initially, we shared the reviewer’s expectation that MODIS AOT would be low), those sites can show quite high AOT, up to 0.4. Nor are MODIS AOT > 0.1 unusual. We have shared the attached figures with the PIs for these sites but have not been able to find suitable explanations. Brent Holben suggested there may be issues with the MODIS ocean/land mask for island with extensive beaches.

The lack of correlation between MODIS and AERONET AOT for these sites implies that we cannot use AERONET to detect possible biases in MODIS AOT due to e.g. windspeed or cloud fraction. (Of course, we can detect an overall bias but this is much easier). We performed a sensitivity study where we changed the minimum acceptable correlation (from 0.5 to 0.75, this removes $\sim$ 20% of the data). This sensitivity study show the robustness of our results. In short, we believe that correlation is an appropriate metric for our task: finding sites that allow us to detect said biases. The section has been slightly rewritten.

P3779-L4: “As robust multiple (linear) regression is a field very much in development”. I am highly surprised by this statement. Multiple linear regression is mastered for many, many years, and I wonder why the authors have not chosen to use such method (using the proper parameters) rather than the somewhat complex method they chose.

The reviewer is of course correct when he/she states that multiple linear regression has been mastered for many years. The problem is the misleading '(linear)' adjective in our text, which should have been 'non-linear'. Multiple regression using non-linear functions on data that is not guaranteed to show the same non-linerarity and that moreover is non-Gaussian and suffers from outliers is not easy and no standard techniques exist.

P3779-L10-16: This section is not clear and must be reworded

We have rephrased this section.

P3782-L5: "due to surface reflection". This paper analyses observations over the oceans. For such cases, 860 nm is more favourable than 470 nm as for the surface reflection (realy black in the near IR, but not so in the blue part of the spectrum)

The reviewer is correct that ocean albedo at 860 is lower than at 470nm. But the difference is less than 2x. At the same time AOT at 860 is typically two times smaller than at 470nm. So we feel that AOT at 860nm is probably more sensitive to errors in assumed surface albedo (whitecaps and cloudiness have spectrally rather flat albedos). This seems to be borne out by our dataset as correlations are lower and relative errors are higher for MODIS AOT at 860nm than at 470nm. We have rephrased the sentence.

P3784-L2-5: The strong reduction in spatial variations of the AOD is certainly an interesting result that deserves discussions. What are the areas where significant changes in the AOD are observed, and what are the parameters that lead to this change. Is it mostly the cloud cover, or something else ? The change in the climatological distribution of AOD is certainly a key outcome in this paper and should be discussed in detail.

As might be expected, the reduction is due to different factors depending on the location. We have added text to discuss this in more depth based on additional analysis of our data.

P3786-L23-25: Statement that there is no validation of fine mode AOT is plain wrong. Several papers, including some cited by the authors, have assessed the MODIS fine mode AOD.

Correct, we have found Anderson et al. 2005, Kleidman et al. 2005 and Breon et al. 2011 who all evaluate fine mode fraction. Ichoku et al 2002 and Remer et al 2002 evaluate effective particle size. This is now mentioned in the introduction and when appropriate.

P3787-L8: Can one state that the band of elevated aerosols in the southern ocean is due to cloud contamination ?

It seems to be the result of larger than usual wind speeds and possibly issues with the scattering models (e.g. there is a correction due to AE as well). See extended discussion in Section 6.

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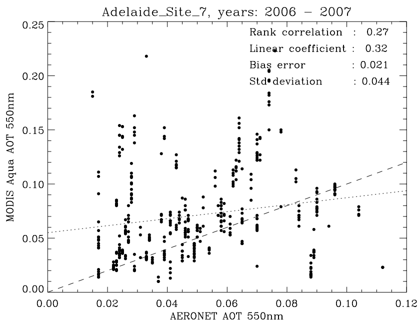

Adelaide_Site_7, years: 2006 – 2007
Rank correlation : 0.27
Linear coefficient : 0.32
Bias error : 0.021
Std deviation : 0.044
MODIS Aqua AOT 550nm
AERONET AOT 550nm
0.00
0.05
0.10
0.15
0.20
0.25
0.00
0.02
0.04
0.06
0.08
0.10
0.12

Fig. 1.

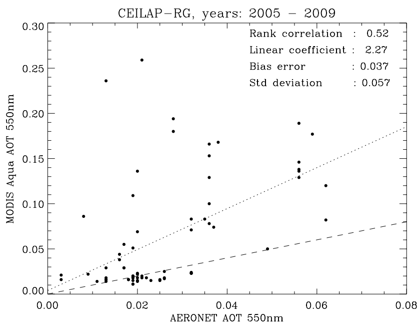
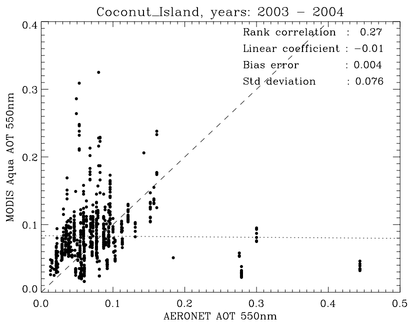
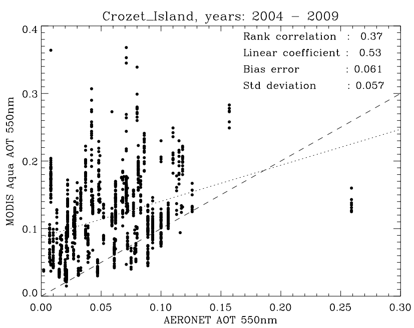

CEILAP–RG, years: 2005 – 2009
Rank correlation : 0.52
Linear coefficient : 2.27
Bias error : 0.037
Std deviation : 0.057
MODIS Aqua AOT 550nm
AERONET AOT 550nm
0.00
0.05
0.10
0.15
0.20
0.25
0.30
0.00
0.02
0.04
0.06
0.08

Fig. 2.



Coconut_Island, years: 2003 – 2004
Rank correlation : 0.27
Linear coefficient : −0.01
Bias error : 0.004
Std deviation : 0.076
MODIS Aqua AOT 550nm
AERONET AOT 550nm

Fig. 3.



Crozet_Island, years: 2004 – 2009
Rank correlation : 0.37
Linear coefficient : 0.53
Bias error : 0.061
Std deviation : 0.057
MODIS Aqua AOT 550nm
AERONET AOT 550nm

Fig. 4.

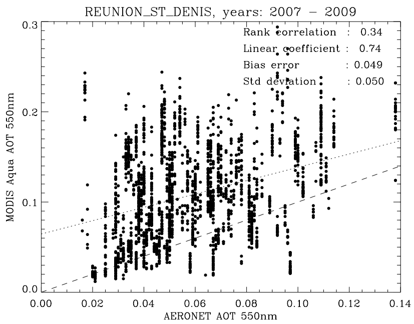

REUNION_ST_DENIS, years: 2007 – 2009
Rank correlation : 0.34
Linear coefficient : 0.74
Bias error : 0.049
Std deviation : 0.050
MODIS Aqua AOT 550nm
AERONET AOT 550nm
0.0
0.1
0.2
0.3
0.00
0.02
0.04
0.06
0.08
0.10
0.12
0.14

Fig. 5.