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Interactive comment on "Assessment of diverse algorithms applied on MODIS Aqua and Terra data over land surfaces in Europe" *by* P. Glantz and M. Tesche

Anonymous Referee #1

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This paper is interesting in principle, but in its present form, is not useful. The MODIS science team collection 5 AOT product has been analysed thoroughly in many studies, some of which are cited in this paper. In that regard, the paper does not add significantly to the understanding of the collection 5 product. The other algorithm, Satellite AErosol Retrieval (SAER), is newer (although described in several other publications cited) and so it is useful to perform the validation and comparison with AERONET/the MODIS collection 5 product. However, as discussed in the specific comments, I do not think that the analysis has been done in an appropriate way. The discussion is not particularly comprehensive, and the volume of data not very extensive (three periods of one to two weeks each does not provide many matches at individual AERONET sites).

Additionally, there is the wider question: why should we be interested in SAER? Is the SAER dataset available somewhere for users to download (or will it be soon)? Does it have some advantage over existing datasets? This is not mentioned anywhere in the paper. The validation performed suggests that the collection 5 product is better in this region (Europe), so an advantage I could see in SAER is the finer spatial resolution, an aspect which is not discussed in the paper. If this dataset is not available to the community, or intended to be used for some purpose by someone, it is not clear what the utility of the analysis is. Unless this question is answered in the revised paper, then I do not see much scientific use in publishing it.

Finally, in a few cases statements made about cited references are in my opinion incorrect or incomplete.

For these reasons, I recommend major revisions of the manuscript, followed by further review. Some specific comments are listed below.

Specific comments

Title: The title should be improved. 'Diverse algorithms' is vague, and aerosol is not mentioned, so the casual viewer would not know what the paper is actually about. Additionally, although the title says 'over land', ocean data are also shown (Figure 3). It would be beneficial if the analysis were expanded to include data over ocean, as discussed later in the review, but I don't think 'over land' is absolutely necessary in the title either way. I suggest 'Assessment of two aerosol optical thickness retrieval algorithms applied to MODIS Aqua and Terra data in Europe' as a better title.

Abstract, final two sentences: periods 1, 2, and 3 are meaningless here without context. These sentences should be rewritten.

Page 2,366, line 13: I see what you are getting at here, but I don't agree that the recommendation of Kokhanovsky et al. (2010) is that more discrete aerosol models are needed. Rather it is twofold. First, because information content is limited, more

accurate aerosol models are needed, as well as better constraints on how to pick which one to use. Adding more aerosol models will not help if they do not represent aerosol scattering/absorption well for the particular situation. Second, we need sensors with improved measurement capabilities to decrease this source of uncertainty. However, since in this work SAER is (if I understand correctly) only using 1 model, it is true that additional models would be of use here.

Page 2,366, line 28: replace BAER for BEAR.

Page 2,368, lines 1-2: I don't think you need to say 'expected to differ', but rather 'will differ'!

Page 2,368, line 6: Although MODIS has a solar diffuser, as far as I am aware it is not used for absolute reflectance calculation in the level 1 data; rather, a set of vicarious calibration procedures are used by the MODIS Calibration Science Team. I suggest rephrasing this sentence.

Page 2,370, line 5: 0.2 at which wavelength?

Page 2,370, line 18: Levy et al. (2010) is the validation paper. The algorithm paper is Levy et al. (2007).

Page 2,370, line 21: I would say 'the 550 nm band cannot be used directly by this algorithm', as there are other AOT retrieval algorithms which do use green bands.

Page 2,371, equation 8: I suggest using the standard notation for absolute difference (i.e. |xi-xbar|) rather than abs().

Page 2,371, equation 9: I think this is wrong. Shouldn't the division by 2 appear outside the parentheses?

Page 2,372, section 2.2: Why only these sites, and not others? Additionally, since you are using Level 2 data, you should cite the AERONET cloud screening paper used to create these data, Smirnov et al. (2000).

Page 2,372, lines 19-21: You show data over water in this figure. However, this is not discussed anywhere in the paper. You should either discuss the retrieval algorithm and results over water, or remove these points from the figure.

Section 3.2. I have several comments on this section:

So in page 2,374, lines 20-21 you are comparing the SAER and MODIS science team data to AERONET in two different ways. This is not a good idea, as your spatial sampling will be different, so it is not an apples-to-apples comparison. See e.g. Anderson et al. (2003) about length scales for aerosol distributions. It would be much more meaningful to average the two datasets in the same way.

Your estimates of the area averaged are also incorrect: because of the MODIS scan pattern, pixels near the edge of the swath are larger than those near the middle. As the MODIS science team algorithm averages level 1 data on a pixel grid rather than an area grid, the AOT retrieval pixels near the edge of the swath are significantly coarser than 10 km x10 km.

Did you apply any quality screening to the MODIS science team data? This is not mentioned in the text. If not, you probably should do. I believe QA=3 is the recommendation for the highest quality retrievals (e.g. results in Levy et al., 2010, which you cite).

The wavelengths used in your analysis differ between MODIS and AERONET, and so, for the collection 5 product, there will be some difference in AOT because of this (less so for SAER where the wavelengths are more similar). This is another example of the comparison not really being of the same things. As the satellite wavelengths are longer than AERONET (469 nm vs. 440 nm, 555 nm vs. 500 nm), and the Angstrom parameter is positive, MODIS would naturally be expected to retrieve a lower AOT for these wavelength pairs. You could account for this by interpolating the AERONET data to the MODIS wavelengths using the Angstrom power law (your equation 7), and remove this source of bias from the comparison, which is often done in other studies. This would make your error analysis more meaningful, and is simple to do. For a typical

Angstrom parameter of 1-2, the wavelength difference will lead to about a 5%-15% difference in AOT, which is similar to the biases you are reporting in your least-squares regression fits. The discussion of the uncertainty estimates, RMS difference etc is also not appropriate because of this bias you are not accounting for (if you don't account for the wavelength shift, you can't consider AERONET an unbiased 'ground truth' to base the confidence envelope on).

More information about the AERONET comparisons should be presented in the revised manuscript. Perhaps a table with information about each site, and the statistics and number of matches obtained at each. Then we could see more clearly whether performance is the same over all of Europe.

Page 2,375, line 13, and 15-18: I would have thought Levy et al. (2010) was the more appropriate reference to cite here, as this is where the MODIS science team data are validated and this confidence envelope is tested. The Levy et al. (2010) analysis largely supersedes Remer et al. (2008) as it is more comprehensive in scope, with more MODIS and AERONET data becoming available in the intervening years.

Page 2,376, section 3.3: This section is quite lacking in detail and would be more useful if you e.g. separated the points according to perhaps surface or aerosol type. Additionally, you do not mention one obvious point: SAER assumes a perfectly scattering aerosol while the MODIS science team product does not. So that is one simple reason why SAER tends to retrieve lower AOT in high-AOT conditions.

Page 2,377, line 7: I would suggest you mean 'relative overestimation' here, since you don't have evidence that the collection 5 product is overestimating AOT. All you know is that it is higher than SAER. Comparing with AERONET, both look similar.

Page 2,377, lines 25-27: I would not say that the poor agreement between MODIS and AERONET Angstrom parameter is 'surprising'. These comparisons were also performed and discussed in the MODIS collection 5 aerosol validation paper (section 3.3 of Levy et al., 2010, which you cite). They suggest a typical uncertainty of 0.4,

although note that retrieved values tend to cluster in two populations.

Page 2,378, lines 16-18: You mention the MODIS collection 5 uncertainty envelope with respect to the SAER data. What reason do you have to expect that the same uncertainty envelope should apply to both algorithms? It would be more useful for future work with SAER to estimate an uncertainty estimate for SAER (which I'd suspect may be larger than the collection 5 uncertainty), and then frame your discussion based on that.

Discussion/conclusions: Your main new result from this study seems to be that there are problems in SAER relating to certain viewing geometries. Yet, the relevant figures and information on this aspect are not shown and only mentioned briefly. This is something which should really be discussed in more detail in the paper.

Table 1: the maximum possible number of satellite points is 9, but the mean plus one standard deviation is typically more than 9. This suggests that the distribution of the number of points is quite skewed. It would be interesting to see this distribution. (However note my previous comment that since the areas averaged for the two MODIS datasets differ, this is not a very fair method of comparison.)

Figure 1: I would delete the data measured at night from this, as it will be 12 hours or so removed from the MODIS overpasses, and aerosol can change a lot in that period.

Figures 4-10: In all of these plots, you quote relative differences and give units of % for them. I think this is probably incorrect, i.e. a label of 0.12% probably means 12% (i.e. a fraction of 0.12) rather than 0.12%!

Additionally, least-squares regression is not appropriate to calculate the linear fits presented in most of these plots. This is because the uncertainty is not constant over the range of the data (as you write, the error is proportional to AOT). Also, at low AOT, as there is a minimum value of AOT which can be measured/retrieved, which means that the errors cannot be Gaussian in this regime. This means that the linear fits and

their uncertainties presented are not meaningful, as the assumptions required for least-squares regression are violated. Figures 9 and 10 might be ok in this regard.

Additional references used in this review:

Anderson, T. L., R. J. Charlson, D. M. Winker, J. A. Ogren, and K. Holmén (2003), Mesoscale variations of tropospheric aerosols, J. Atmos. Sci., 60, 119–136.

Levy, R. C., L. A. Remer, S. Mattoo, E. F. Vermote, and Y. J. Kaufman (2007), Second-generation operational algorithm: Retrieval of aerosol properties over land from inversion of Moderate Resolution Imaging Spectroradiometer spectral reflectance, J. Geophys. Res., 112, D13211, doi:10.1029/2006JD007811.

Smirnov, A., B. N. Holben, T. F. Eck, O. Dubovik, and I. Slutsker (2000), Cloud-screening and quality control algorithms for the AERONET database, Remote Sens. Environ., 73(3), 337–349.

Interactive comment on Atmos. Meas. Tech. Discuss., 5, 2363, 2012.