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Atmospheric Measurement Techniques Discussions

Interactive comment on "Bayesian Dark Target Algorithm for MODIS AOD retrieval over land" *by* Antti Lipponen et al.

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We thank Dr. Sayer for positive and valuable comments. Below Mr. Sayer's comments and questions are shown in boldface font followed by our replies in normal font.

This is a very interesting and important study which provides what appears to be a better-performing way of retrieving AOD from MODIS measurements over land than the land Dark Target (DT) and Deep Blue (DB) algorithms. I have talked

with the authors a bit about their approach at recent meetings, and am glad to see a paper on the subject appear now. After a careful reading I had a few comments/questions which I was hoping the authors could expand upon.

The authors present their work as a Bayesian DT (BDT) approach, which essentially implies recasting the DT algorithm within a more formal error propagation system. As part of this statistical formalism, they also simultaneously retrieve all valid L2 pixels in a granule, which allows the use of spatial variability constraints, rather than using the independent pixel approximation, and transform much of the data into log space to avoid unphysical negative values. This is all good stuff. I think that the manuscript is written and presented well, the approach has technical merit, and the authors appreciate some nuances about DT that others often do not (e.g. the FMF is not "fraction of AOD from the fine mode" as it is in some other data sets, but "weight of the fine-mode dominated aerosol optical model").

Digging down, there are two other major changes: (1) the 550 nm band is also used in the retrieval (DT does not use this band) and (2) surface reflectance becomes a retrieved quantity (using the MODIS BRDF/albedo product as a prior constraint) rather than the spectral shape being an assumed quantity. These both have bigger implications, and are what I have questions about.

On (1), since the authors are adding this band, they must be generating new LUTs (since there is no pre-existing DT 550 nm land LUT). I may have missed it but did not see which radiative transfer code is used to generate the LUT? Is this the same as is used in the MODIS DT algorithm? And why was the 550 nm band additionally added; what happens if it is not used, is performance comparable? I know that MODIS DT and some other algorithms choose not to use this band

for retrievals over land, as assumed spectral/directional surface reflectance relationships don't always work so well for 550 nm as some other wavelengths.

We actually did use the pre-existing DT 550 nm land LUT. This LUT is not directly used in the operational DT retrieval but it is distributed with the stand-alone land code (https://darktarget.gsfc.nasa.gov/reference/code). The reason for adding this band to our approach was simple: we used all pre-existing LUTs that were easily available. Our algorithm is not restricted to the four selected bands and one topic for future research is to add more bands into the algorithm and see how the results are improved.

We carried out a test in which the 550 nm band was not taken into account in our retrievals. The retrieval accuracy of our algorithm was clearly worse than in the retrievals carried out with the 550 nm band. This indicates that the 550 nm band plays an important role in the retrievals and also suggests that adding additional bands into the retrieval may still clearly improve the retrieval results. As adding new bands is a good topic for future research, we will more carefully analyse the effect of including different bands into the retrieval in the future.

Point (2) is the bigger thing. For me, the defining characteristic of the DT algorithm is the assumption that the swIR region can be used to model reflectance in the blue and red bands, according to the relationships developed first by Kaufman and then expanded by Levy. All algorithms must make some simplifying constraining assumption about surface reflectance and this is the core of what DT is and what differentiates it from other approaches. For (almost) any sensor, when the AOD is low, the dominant over-land source of AOD retrieval error comes from surface model error (since most of the signal is surface reflection), so a retrieval's surface reflectance model is the first-order determinant of how the retrieval will behave and when it will and won't work well.

The BDT approach, on the other hand, retrieves surface reflectance simultaneously with AOD and FMF, using an aggregation of the MODIS BRDF product (which is itself a time-aggregated product based on atmospheric correction of MODIS imagery) and variability constraints to provide an a priori. In this sense these a priori constraints are the new surface model at the core of the algorithm and I expect the key to why it appears to work better than standard DT and DB. This is a bit more similar to e.g. the Deep Blue approach over deserts (to oversimplify, a climatology of surface reflectance obtained from the clearest 15% of scenes) or the MAIAC approach (where a time series of a number of days is built up and then surface and atmosphere are retrieved together) than it is to DT. The BDT algorithm has, unless I have misunderstood, entirely abandoned the swIR-to-visible surface model at the core of DT. All that appears to be in common are the aerosol optical models and cloud screening. This is not a criticism of the method, which appears sound. But it leads me to my main question: the BDT approach is clearly an approach which works well, but is it really correct to call it "Bayesian Dark Target", when the core feature of DT is the aspect which was discarded?

In my mind, it is not and it would be better to pick a different name as BDT could be misleading. The name DT conjures up the MODIS DT algorithm, and BDT likewise implies that. This is, for the reasons discussed above, something different.

Yes, you are correct. In our algorithm, the swIR-to-visible surface model has been abandoned. We also agree with you with the name issue and therefore decided to change the name of the algorithm. We decided to call our algorithm as Bayesian Aerosol Retrieval (BAR) algorithm and have updated the manuscript accordingly.

On an unrelated note, Equation 3 defines the posterior covariance matrix for the retrieved state. This can be used to provide pixel-level uncertainty estimates for retrieved AOD (and other quantities), a topic of much current interest. It would be interesting to compare these to the actual AOD retrieval errors against AERONET, in a statistical sense, to assess whether these are reasonable. For example, for the subset of matchups with an actual retrieval absolute error of X, is the distribution of estimated uncertainties consistent with an expectation of an error of X? (See section 3.3 of Popp et al. 2016, doi:10.3390/rs8050421 for some other example analyses looking at validating pixel-level uncertainties.) If yes, great. If not, when and where there is a mismatch between typical estimated uncertainties and typical actual errors can tell you something about which terms in your error budget are not quite right.

We agree that it is important to evaluate the quality of the uncertainty estimates and made a figure similar to that in doi:10.3390/rs8050421, see Figure 1 for this new figure. This figures shows that the uncertainty estimates are in general overestimated for most of the AOD retrievals (small AOD values). There is room for improvement in the estimates but we think we are at least with small AOD values on the safe side as the uncertainty estimates do not give overoptimistic uncertainty estimates. The Table 8 in the manuscript basically shows the same information and from the table we can see that for large AOD values the uncertainty estimates are in general underestimated. We leave the further analysis and improvement of the error estimates as topics for future research.

I also had a comment on the results shown in Figure 6. The high bias in Ångström exponent (AE) in both DT and BDT when the AERONET AE is low (i.e. likely cases dominated by dust) may well be because the 'coarse-dominated' aerosol model used in the retrievals assumes spherical particles, which do not model the scattering/absorption of nonspherical dust particles well. This means

that the phase function is simulated poorly at some angles, and the spectral dependence of absorption and extinction is incorrect. Positive AE biases are one characteristic signature of this problem. Some theoretical simulations of this are shown in Mischenko et al 1997, doi:10.1029/96JD02110; more recently, we gave (over ocean) a practical demonstration of the differences between spherical and spheroid assumptions in Lee et al 2017, doi:10.1002/2017JD027258.

Actually, the coarse aerosol model in Dark Target over land assumes spheroid-shaped particles (for details see for example doi:10.1029/2006JD007815). Regardless of this, it is still possible that the AE biases are due to aerosol models. We thank you for the nice references that allows us to study this more. As in this manuscript we mainly describe the algorithm, we will leave the more careful analysis on effect of aerosol models for AE retrievals for a topic for future research.

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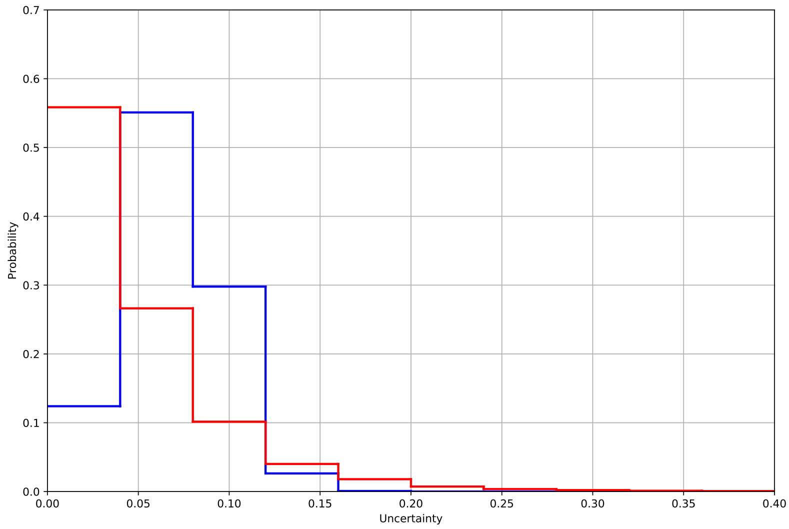

Fig. 1. Histograms of the the Bayesian Aerosol Retrieval (BAR) algorithm 1-standard deviation uncertainty estimates (red lines) compared to the difference between the AERONET and MODIS BAR AOD retrievals (blu