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

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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

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Discussion paper

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.

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 firstorder 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.

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.

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.

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