Atmos. Meas. Tech. Discuss.,
doi:10.5194/amt-2017-359-RC2, 2017

Atmospheric Measurement Techniques Discussions

Open Access

AMTD

Interactive comment

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

Anonymous Referee #3

Received and published: 22 November 2017

This paper provides a detailed description and validation of a new algorithm for the retrieval of aerosol optical depth (AOD) and Fine Mode Fraction (FMF) (plus surface reflectance in four MODIS bands, although these values aren't really discussed or evaluated by the authors). The new algorithm, refereed to as the Bayesian Dark Target (BDT) Algorithm, uses the same preprocessing, look-up tables and aerosol typing as the operational MODIS Dark Target (DT) algorithm, but otherwise takes a completely different approach to the retrieval problem:

- The spectral constraints on surface reflectance employed by DT are done away with.
- Spatial correlation in AOD and FMF are used to provide a priori constraint

Printer-friendly version

Discussion paper

- An entire granule of MODIS observations are analysed simultaneously using a Bayesian maximum a posteriori optimisation, which provides pixel level uncertainties based on prior assumptions of measurement and forward modelling uncertainty.

The algorithm is evaluated against both the DT and the Deep Blue algorithms (both of which are operational MODIS products), using AERONET as a reference, over the year 2015. The BDT algorithm shows a significant superiority to the two operational products across almost all regions, with the authors particularly emphasising improvements over urban areas.
I am happy to recommend this paper for publication, subject to the authors addressing the following questions and corrections. In particular, I am keen to see my first two general points addressed satisfactorily. The paper is generally clearly presented and concise, and the work represents a novel and valuable addition to the field of aerosol remote sensing from satellite.
I have divided my comments and questions into general points, followed by specific points denoted by page (P) and line number (L).

General points

I am somewhat concerned by the way the approximation error (§3.3 and Appendix B) is computed. In §3.2 the authors state that the mean of the measurement noise PDF ($\mathbb{E}_n$) is assumed to be zero. However, it appears that this constraint is not applied when computing the approximation error mean ($\mathbb{E}_u$) from comparing MODIS TOA reflectance to values simulated from AERONET AODs. Is this correct? If it is, then this approach is making an implicit bias correction to the MODIS L1B reflectances, based on AERONET aerosol measurements and the retrieval's own forward model (plus the MCD43C surface reflectance) - this is fine as it stands, although it's clearly a bit of fudge. However, the authors then use the same AERONET measurements as validation data. It is thus not a huge surprise that they see a significant improvement

Printer-friendly version

Discussion paper

in the bias against AERONET compared to the DT and Deep Blue products. Indeed, as the correction is computed separately for different regions (the same ones used in the validation?) and seasons, we might expect it to improve the correlation and RMS of global and yearly comparisons of AOD or FMF vs AERONET as well.
If I am correct in my reading of how the approximation error is calculated and applied, then I would like to see the authors provide a comparison against AERONET where $\mathbb{E}_u$ is assumed to be zero. Otherwise, a clarification of how the approximation error is calculated is needed.

The result resented in §5.2, that the retrieval performs best vs AERONET if the prior constraint of the spatial correlation of AOD and FMF are switched off, doesn't seem make sense without considering the above point, as you are then retrieving 6 parameters (four surface reflectances + AOD + FMF) from 4 measurements. Thus the results in tables 3 and 4 need further explanation.
I think that the fact that the best correlation, bias, fraction within EE_{DT} and RMS error all correspond to the configuration where spatial correlation is disabled, but approximation error is enabled, is simply due to the regional/seasonal bias correction against AERONET implicitly applied by the approximation error methodology. Again, there is nothing inherently wrong with doing such a correction, but you cannot then pretend that AERONET is an independent source of validation data.
Furthermore, tables 3 and 4 show that applying the correlation constraints don't actually improve the results vs AERONET, even if the approximation error is disabled. This would seem to imply that the correlation constraints aren't improving the retrievals at all. I would be surprised if it turned out that these constraints don't actually improve the retrieval in many cases, but I feel that a concrete example is needed, rather than the vague assurances given at the end of §5.2.
Also, am I right in thinking that if both the approximation and spatial correlation errors are switched off, the retrieval effectively assumes the prior surface reflectance values from the MCD43C product are correct (i.e. the retrieval doesn't move from the a priori

values)?

I agree with the point made in an earlier comment by Dr Sayer regarding the name of the algorithm. Clearly, given that the processing done by the authors shares the cloud-clearing, look-up tables (and thus aerosol models) etc as the DT algorithm, it would seem fair enough to call the resulting *product* as the Bayesian Dark Target product, but the retrieval algorithm itself could be described as opposite in approach to the NASA DT algorithm, as BDT does away with both the independent pixel and spectrally correlated surface reflectance assumptions which form the basis of the DT (and Deep Blue) approaches (while introducing its own assumptions about spatial correlations in aerosol properties). My feeling is that the authors are in danger of "under selling" the algorithm, as, without going through the details of the algorithm, it could appear to just be DT with pixel-by-pixel uncertainty estimates.

In a similar fashion, I'd like to see the authors move away from the (in my opinion misleading) use of the term fine-mode fraction to describe the fraction of the AOD due to the fine mode. Perhaps "fine mode AOD fraction" is a better term? At the very least, the definition of what is actually meant by FMF needs to be stated up-front.

Finally, though I appreciate that adding in an additional product into their validation would be a bit much, the authors don't seem to be aware of the MAIAC MODIS product, which (as Dr Sayer noted) in some ways has more in common with their approach than DT.

Specific points

P1L1: Suggest you reword to "... (BDT) algorithm for the retrieval of aerosol optical depth over land from MOderate Resolution..."

P1L18: Reword to "...particles may be hazardous to human health when inhaled..."
P1L21: Add a comma after "predictions".
P2L4: I'd describe DT as the primary operational algorithm used to retrieval aerosol, not just "an algorithm".
P2L7: Reword to "...DT algorithm is the brightening effect, whereby an increased amount of aerosol over dark..."
P2L19: Remove "for example".
P2L24-L27: The sentences describing the Deep Blue algorithm don't scan well at all.
P2L34: Please provide a reference/justification for the statement that taking advantage of spatial correlations in aerosol properties can improve retrieval results. I note that your own results (Section 5.2, Tables 3 and 4) don't actual support this statement (although I'd be surprised if it wasn't true!)
P4L8: Why did you choose the listed bands in particular? Was just because these were the ones for which look-up tables were available?
P4L21: As with the other referee, Dr Povey, I am also confused by the $\tau = \log(\tilde{\tau} + 1)$. I understand retrieving AOD in log space (not only do you avoid the problem of negative AODs, but the observed distribution of AODs is much closer to log-normal than normal), but where does the "+1" come from? Also, as Dr Povey notes, $\log(\tau + 1)$ is defined for $\tau > -1$, and thus doesn't prevent negative optical-depth values as you state in the text.
P4L22: The retrieval using log reflectance is also new to me. Can you elaborate on why this was chosen? In particular, doesn't this complicate the use of the estimates of the measurement noise of the MODIS reflectances? And, again the "+1" needs explaining.
P4L25: Please explicitly define the symbols used for expected value vectors and covariance matrices here.
P5L6: Eq (2) needs reformatting so that it fits within the margins.
P6 (§3.1.1 and 3.1.2): I don't agree with your approach in presenting the Prior models and their covariance matrices in particular. For a start, the values for σ^2_{nugget} make up

a small fraction of the total variance (3rd sig. fig. for AOD and 2nd sig. fig. for FMF) and thus are largely irrelevant. Furthermore, I think it be clearer if you were define a correlation matrix and variance matrix separately, which are then combined to give you your covariance. The off-diagonal elements of a covariance matrix are combination of the variance of the variables concerned (where an increased value corresponds to a less tight constraint) and correlation between them (where a increased value implies a tighter constraint).

P6L16-18: The authors need to acknowledge that an inherent weakness of their scheme, and the use of a spatial correlation constraint in particular, is that it will always result in the smoothing of sharp features in the aerosol field (such as smoke plumes). There is no way you will be able to "correctly" retrieve the AOD of a near-source aerosol plume when assuming a correlation in aerosol AOD and FMF over 50 km.

P7L14: Please provide some reasoning behind the choice of the blue sky albedo, and the 0.5 weighting coefficient in particular.

P7L29: I know Eq (5) is fairly ubiquitous in aerosol and cloud remote sensing, but a reference for its derivation would be nice.

P8L11: How are the look-up tables corrected for surface elevation? Is this an additional parameter in the table?

P9L8-9: Change to "We model the approximation error u as spatially, but not spectrally, uncorrelated, meaning the correlations between MODIS bands are taken into account".

P9L10: Delete the "the" before "spatial and season variations", Also, place a comma after "Similarly".

P9L19: Remove "the" before "± 30".

P9L25: Change to "In order to evaluate the near real time. . ."

P10L14: Remove "the" before "DB retrievals".

P11L13: The use of "oversmoothed" here irks me somewhat. For a start, how does one define what is "oversmoothed"? I feel the authors should be up-front and acknowledge that there is some evidence in Fig. 3 that their algorithm is smoothing the AOD field to a certain degree - in particular, I am looking at the thin smoke plume slightly above the smoother, wider area of smoke in the middle of the image. There is a clear linear feature in both the true-colour and DT images, which largely absent from the BDT results.

P11L14-17: The authors present some circumstantial arguments to suggest that the BDT FMF in Fig. 3 is superior to that provided by DT, and I agree that some of the features present in the DT results are almost certainly artefacts. However, due to the spatial constraints applied to the BDT algorithm, it is always going to produce a more pleasing looking, smoothly varying field than an independent pixel approach like DT. This doesn't mean that we can conclude that, for the scene in question, we can say which product is quantitatively more accurately. Again, some acknowledgement of the fact that some real features might be smoothed-out in the BDT product should be included.

P12L2: Insert a comma after "algorithms".

P12L4: Insert a comma after "Figure 4".

P12L8: Change to "...was carried out, based on the DT algorithm QA flag, which is designed to discard..."

P13L6: Change to "Visual inspection shows the BDT retrievals..."

P21L28: I have never heard of anyone trying to use spatial correlation constraints on the surface land reflectance. This doesn't seem to make sense as the land surface is generally pretty anisotropic at all spatial scales. Do the authors mean spectral correlation instead?

P22L1: I don't believe the claim that the signal-to-noise ratio of MODIS reflectances is too low to allow accurate aerosol retrievals. The reasons that aerosol products have usually been done on a courser spatial scale than native instrument resolution are all to do with dealing with residual cloud contamination and surface anisotropy (and reducing product size and computational cost).

P22L4: Due to the inclusion of the AERONET based approximation error term, the scheme already has a kind of data-fusion with AERONET!

P21/P22: I have an additional thought for future development of the algorithm: Is there

any reason the approach couldn't be applied over the ocean?
P22L21: Remove one of the instances of "statistical".
P22L25: Consistency of symbol for TOA reflectance.
P23L4-5: Insert commas after "unknown" and "ρ^{TOA} is fixed".
P24L2: Reformat equation to fit.

Interactive comment on Atmos. Meas. Tech. Discuss., doi:10.5194/amt-2017-359, 2017.