Atmos. Meas. Tech. Discuss.,
doi:10.5194/amt-2020-222-RC1, 2020

Atmospheric Measurement Techniques Discussions

Interactive comment on "Modls Dust AeroSol (MIDAS): A global fine resolution dust optical depth dataset" *by* Antonis Gkikas et al.

Anonymous Referee #1

Received and published: 6 July 2020

This study describes a new dust optical depth (DOD) data set, MIDAS, which is derived by taking MODIS (Aqua only) satellite-based aerosol optical depth (AOD) and the aerosol speciation from the MERRA2 reanalysis. CALIOP and AERONET are used for evaluation.

The manuscript is in scope for the journal, though would be a closer fit to the other Copernicus journal ESSD because it is mostly a data set description paper. The material is important because speciated AOD is one of the next frontiers for better climate and air quality applications of data sets. The quality of language and visuals is satisfactory overall, though some edits are needed, and figures 5, 7, 9 would benefit from labels being increased in font size (hard to read without zooming in). Some of the content in the Supplement should be in the main paper. Overall, I recommend major

Printer-friendly version

Discussion paper

revisions and would like to review the revision.

General comments:

The main technical weak point of this study is that all the observational data sets used are out of date: MODIS Collection 6 instead of 6.1; CALIOP version 3 instead of version 4; AERONET version 2 instead of version 3. So this affects the AOD source used (MODIS), the optical properties used for matching (AERONET), and the data sets used for evaluation (AERONET, CALIOP). Some of the differences between old and new versions are systematic. So it is not clear to me how different the derived data set, or the evaluation results, would be if the newest data versions were used. To my best knowledge all of these latest data versions have been available for 1.5 years or so (i.e. they are not that new), so it is unfortunate that outdated versions were used when this analysis was done. It sounds like the authors are using a post-processed CALIOP product from another group (LIVAS?) rather than the official NASA CALIOP data products, so maybe that can't be changed. But, if the authors intend for others to use MIDAS for scientific analyses, it would really be best to use the most up to date inputs. I know that this means more work downloading files and rerunning code but this should mostly be computer time if the analysis code has already been written. So my main recommendation is to do that. I guess it is up to the authors and editor to decide what is most reasonable here. The 2007-2016 time period could also possibly be extended, I see no reason why it couldn't cover more of the Aqua record. Longer time series are of course more beneficial for things like trend analyses.

Numbers are often given to too many significant digits. One example I'll mention again later includes referring to an offset as 4.264%. Including all these digits gives an unrealistic impression of the precision of these estimates: can you really say that the true population offset is 4.264% and not 4.265%? Is it important that it is 4.264% and not 4.265%? If the answer to either of these is no, this is an indication that there are too many significant digits being reported. The authors should consider all numbers presented in this manuscript. For this case, for example, I'd probably just say 4.3%. This

will also make the paper more readable.

I downloaded some MIDAS data from the link in the paper to have a look. The contents of those files seemed as described. I have four suggestions based on looking at these files:

1. I didn't see a MIDAS file version identifier, but the Readme file notes that some things are in testing or will be added in a future version. So it would be good to add a MIDAS version number somewhere in the filenames so the user can be sure which version of MIDAS they have (and which version of MIDAS technical documents such as this refer to). It may be unclear for the data user otherwise.

2. One issue with is that the files contain some negative AOD values, which are unphysical. This is a result of the Dark Target land AOD algorithm which allows small negative retrievals. However since this is unphysical I recommend that in the next version, the authors set these values to 0. This is one issue with the source data which is easily fixed.

3. It would also be useful to add an uncertainty estimate to each pixel. There is extensive discussion in the middle of the paper about uncertainty estimates, but these don't appear to have made it through to the data set itself, based on the files I looked at.

4. Finally, the files seem to contain some data fields inherited directly from the MODIS aerosol product, e.g. Angstrom exponents. As these are for total AOD and not dust AOD, I wonder if it would be better to remove these. Or, combine the Deep Blue land Angstrom exponent with one of the Dark Target ocean ones. There's also solar and sensor zenith angles, but I'm not sure what these are in there for. This would decrease the size of the archive to be downloaded somewhat.

My more specific comments are as follows:

Line 133: should the word "conclusions" be added before "are drawn"?

Lines 143-147: I suggest rewording this sentence. The Dark Target algorithms are really two different approaches as they have different bands used and completely different assumptions between them. Also, Deep Blue is over all snow-free land, not just bright deserts. So really it is one water algorithm (Dark Target ocean) and two land algorithms (Dark Target land, and Deep Blue). It is probably worth acknowledging that there are other MODIS aerosol algorithms too (e.g. MAIAC), they are just not included in those files.

Line 153: I think the authors mean either “increasing pixel size” or “decreasing pixel resolution” here. Not “increasing pixel resolution”, which is the opposite.

Line 206 and 212-215: note that the MODIS aerosol product is not assimilated. Rather, it is a neural network retrieval based on MODIS radiances that is assimilated. Not a neural network bias correction based on the MODIS retrieval. So the MODIS information going into MERRA2 is not the same as is being used as the main AOD data set here.

Line 333: note that Levy reference is only for Dark Target over land. For discussion of Deep Blue Angstrom exponent over land, see the Sayer et al (2013) paper that is cited later in the manuscript.

Line 357: I am not sure it makes to take the quadrature sum of DT and DB uncertainties when they are merged. This means the overall uncertainty is worse than either DT or DB. If you think the uncertainties on these algorithms are independent, then you are effectively averaging two observations which means the uncertainty in the sum should be divided by sqrt(2). Since the retrieval is the average of two algorithms then the uncertainty should represent the uncertainty on that average.

Line 386-389: If the output is at 0.1 degrees, then there should only be 1 retrieval in each (as the MODIS product is 10x10 km at nadir), so I don’t understand this part about decreasing uncertainties when you have multiple retrievals. Or is this about when there are overlapping retrievals at the edge of swath from consecutive orbits? If

so, that should be stated. If this is about averaging to a coarser space/time scale, then I don't think it makes sense to use the root n factor here because we know there is high spatial correlation in the errors because the errors are mostly not noise.

Section 4.1: I am not sure that it is useful to compare total MERRA2 and MODIS AOD in this way. Or at least, the framing of the purpose here is not right. If there is a systematic disagreement then that tells you that there might be an error in the derived MERRA2 dust fraction as well. Would it not be more meaningful for the present analysis to compare MERRA2 and MODIS dust AOD rather than total? Or to report summary results of the evaluation of MODIS AOD against AERONET (from DT/DB team studies)? As written, section 4.1 doesn't fit well with the rest of the paper.

Section 4.3: The authors here frame the differences as if MIDAS is in error. However, unlike the direct-Sun AERONET AOD data, the AERONET almucantar scan retrievals used here have non-negligible uncertainties (which are not necessarily random). So some of the discrepancies and biases might in fact come from uncertainties in the AERONET DOD estimates. This was not directly discussed beyond a mention that the AERONET DOD estimates made here neglect fine-mode dust, although I think with the AE filtering this is likely to be a negligible effect in most cases.

Lines 666, 667: here the authors say that MERRA2 has "biases" and "overestimates" compared to CALIOP. It would be better to refer to positive and negative "offsets" or "differences" instead, because "bias" and "oversestimate" imply a problem and that CALIOP is the truth. Really none of the data sets are the truth and we are only making comparisons and not diagnosing errors. So more neutral language like "offsets" should be used here (and throughout), and terms like "bias" and "overestimate" should be avoided unless it involves a comparison with something that can be considered a reference truth. I mentioned only these examples although there are others in this section and through the paper where these or similar terms are used (and there are places where the wording is ok as well).

Line 716: authors should check and clarify which of the data sets corresponds to which number here. For example the wording implies that 4.264% is more than 9.405% which is obviously backwards.

Sections 4.2 to 4.5 were honestly a little hard to read because it's a large amount of text which is basically describing several figures and providing references. This also comprises about 11 of 28 pages of body text in the paper. I wonder if this can be streamlined a bit. The authors write that there will be a follow up paper looking at this same material in more detail as well. So I wonder if here it is best to just show figures and highlight where the data sets do not agree well (and maybe try to figure out why), as these are areas to focus future study on. That type of approach (figure out where and why there are differences) would also make the paper fit better in AMT.

Table 1: This is a bit of a sea of numbers. It is difficult to easily pull out the main message here. What is the main message here? Or is this just for reference? Given it relies on regional acronyms, it would be better to present the map defining the regions in the main paper rather than in the supplement.

Figures 2,3: most of the world here is in the 0-0.2 range in Figure 2, which is very hard to distinguish visually because it is different tones of blue. Figure 3 solves this but as it's a separate figure, it is card to glance back and forward. Also, I am not sure how helpful it is to show an annual map here because dust seasonality is strong. So I suggest making seasonal maps instead of annual to replace these figures, this would give more insights. I am also not sure whether the maps of FB and FGE are needed for this panel. Maybe just replace this with an 8-panel figure: left column is seasonal MDF, right column is seasonal MDF minus CALIOP dust fraction (i.e. mean bias)? Then Figure 3 could be 4 panels showing seasonal correlation coefficients? I think they are the most crucial metrics to show here because they show the level of consistency in typical dust fractio and the variation captured by MERRA2, which are what inform the DOD uncertainty here. The other panels could maybe move to the Supplement if the authors think they are useful. I know there are a few seasonal maps in the Supplement

but think the maps discussed above should be in the main paper.

Figure 4 (and text discussion): I don't think the linear regression is appropriate here, so it should be removed. Since the uncertainty on DOD is proportional to total AOD, it is likely that the assumptions of regression are violated. Also I don't think a global regression is useful because it is likely there are regional differences in the errors, meaning that the global regression line is not informative. Same comments apply to Figure S5 in the Supplement.

Figure 5: the circles are all too small to see.

Figure 8 (iii): this is the mean of the DOD uncertainties, right? Or is it the uncertainty on the mean DOD? This needs to be stated more clearly.

Figure S4: this illustrates a problem I have with the validation methodology. A 4 hour averaging window is pretty huge! And the time variation of AERONET DOD in that window can be much bigger than the AERONET uncertainty. So some of the disagreement seen in Figure 4 is due to this time mismatch. For this example the range of DOD in this window is about 0.09, or 40% of the average. This makes it hard to assess the performance of MIDAS. This is something that shouldn't be buried in the Supplement; I didn't see the mention of a 4-hour window in the main paper (if it is there, it is not clear) so the reader may not realise how big it is. Probably a smaller window is needed, and some filter based on AERONET time homogeneity. I know this will decrease the data volume, maybe a lot, but with such a big time variation in DOD within the window it makes the AERONET comparison a lot less useful for MIDAS evaluation.

On a non-scientific note, I thought the use of MIDAS as an acronym was amusing and a good choice.

Interactive comment on Atmos. Meas. Tech. Discuss., doi:10.5194/amt-2020-222, 2020.