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Interactive comment on "Two decades of satellite observations of AOD over mainland China" *by* Gerrit de Leeuw et al.

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General comments and recommendation

I am posting this review under my own name (Andrew Sayer) as I've discussed the work with the authors at a recent workshop, and also am a developer of the MODIS aerosol products used as part of the analysis. The study looks at 20 years of satellite AOD over China, using the ADV algorithm applied to ATSR2/AATSR measurements, as well as the MODIS Terra Deep Blue/Dark Target (DBDT) merged product. The two are non-negligibly offset but have similar spatial patterns and interannual variability. CALIOP data are also used (principally to look at the vertical profile of aerosol loading), and AERONET is used as a validation data source. This represents part one of an analysis,

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[Discussion paper]

with a second part about seasonality and trends to follow. (Incidentally, I would be very keen to review the follow-up by Sogacheva et al, which I refer to hereafter as 'part 2', when this is submitted.)

On the whole this is an interesting paper but I think as currently presented it suffers from being split into two parts, and would feel more complete if the seasonal analysis and trend analysis were part of this study. In general I am not a fan of annual analyses because, especially in this study region, the aerosol loading, type, and satellite sampling completeness (due to e.g. clouds, snow) both show strong seasonal variability. This is accentuated by the ATSRs having a swath width of around 550 km as compared to MODIS' 2330 km, as well as ATSR-2 temporary failures during parts of 1995/1996 meaning that seasons were missed out. As a result I don't think that comparing an annual mean AOD is very meaningful. It's impossible to tell without going deeper how much of the offset is driven by sampling differences. So I imagine this aspect will be addressed more in part 2. I understand that changing to a seasonal focus here would make the combined paper too long and render the follow-up part 2 obsolete. In that case, this first paper could include more of a perspective on evaluating and thinking about how to merge data records. The ATSRs and Terra (MODIS and MISR) are among the few long-term morning aerosol records; the paper does a nice job of showing the offset (on a country-level scale and in spatial patterns) between them, but similarity in year-to-year variations, so perhaps the authors could go deeper here and identify for example when/where it is sensible to consider the records 'consistent' (and pave the way for a future attempt to merge records) based on a more comprehensive evaluation of the data sets vs. AERONET.

My recommendations are therefore for revision and re-review. I think this is an interesting paper but could have a better focus and clearer split between this and the follow-on part 2 paper. Indeed, after my initial read and talking to the authors, many of my suggestions were things they had planned for part 2. So I think this study would benefit from a clearer definition of scope, with this part 1 setting up the data, evaluating, and

giving a big picture, and part 2 focusing more directly on seasonality and time series. I have some specific comments and suggestions to this end below. While this is a long review, that should not be taken as a negative statement about the paper, but rather my feeling that this is an interesting and important topic which deserves careful discussion.

Specific comments

Title: from my reading of the manuscript and discussion with the authors, this paper does seem to be quite tightly linked with the forthcoming Sogacheva et al study. I therefore wonder if this could be reflected in the title? For example, add something like "Part 1: large-scale patterns and data set evaluation". That way the reader will immediately be alerted that there is a second related study they should look at too.

Abstract: this should be more concise (ideally one paragraph). I suggest a shortening; for example, the first two sentences could be deleted. The second paragraph could also be more concise, for example saying that ADV covers 1995-2012 and MODIS Terra 2000 onwards (and up to 2015 used in this paper). I'd delete the comment at the end about decreasing AOD since 2012 since trends/variations are not quantitatively discussed in the manuscript.

Page 3, lines 6-19: the authors state that the "focus" of the data is from the combined ATSR-2/AATSR ADV record, and it is "further extended to 2015" using MODIS Terra. I appreciate that this may in part be because the authors are developers of ADV and perhaps the analysis started that way. However as a reader it appears the analysis evolved and roughly equal emphasis and weight is given to each of ADV and MODIS, with CALIPSO being the add-on.

Page 4, first paragraph: some of the text here is repetitive with that on the previous page (talking about a 1995-2012 ADV record extended with MODIS) and can be shortened/deleted.

Page 4, line 13: this study uses the merged Deep Blue/Dark Target MODIS product.

The Levy et al (2013) paper cited here focuses on the Dark Target land and ocean algorithm changes, with a brief section about the DBDT merge. Sayer et al (2014), which is cited elsewhere, has an expanded description (and evaluation) of the DBDT merge (which was new to Collection 6). Hsu et al (2013), also cited elsewhere, is about Deep Blue updates. To better direct the interested reader to information about what changed in Collection 6 I'd suggest citing all 3 papers here, or else citing only the Sayer 2014 paper, rather than Levy 2013 alone.

Page 4, line 25: This Levy 2013 reference would also in my view be better off as Sayer 2014 as well/instead. (I'd argue that I am not biased in this since Rob Levy and I are authors on both papers. Levy 2013 is really mostly about Dark Target while Sayer 2014 is more explicitly about Deep Blue, Dark Target, and the merge all together.)

Section 2: Another obvious choice for inclusion in this study would be the MISR aerosol product, which is also on Terra (i.e. same local time and time series length) but has a different measurement and retrieval type from MODIS, and a swath more similar to ATSR. Or SeaWiFS, which covered late 1997 to 2010 but had an early afternoon crossing time. At present later on the authors compare ATSR and MODIS and note some similarities and some differences in spatial patterns and AOD magnitude. The reasons for using ATSR and MODIS are a bit arbitrary so adding an additional data set (I would probably suggest MISR) would help strengthen the understanding of where the satellite products agree and where they do not (for either algorithmic or sampling reasons). I realise this is not a negligible amount of work but do think that adding a third perspective will be useful.

Page 6 line 1: the new calibration update described in Levy et al (2013, 2015) and Lyapustin et al (2014) was not applied in Collection 6 Dark Target data, so this sentence is incorrect and should be deleted. Deep Blue does include polarisation, gain, and response-vs-scan corrections and detrending for Terra (which are described in the Sayer et al 2015 reference cited in the next sentence). But Dark Target does not, and the Levy/Lyapustin references are not related to what is done in Deep Blue. I suggest

rewording to reflect this.

Section 2.1.3: it is not clear to me from reading this which specific CALIPSO product and data version are used here. It says the 'L2 product', but there are several of those (aerosol profile, aerosol layer, combined cloud/aerosol). Also, is the latest version 4? If not, it should ideally be updated, due to some improvements in the version 4 product to do with calibration, layer detection/extent, lidar ratios, and type classification.

Page 7 line 5: the AERONET team like use of the level 1.5/2.0 products to have a reference to their cloud screening and QA paper, Smirnov et al (2000, doi:10.1016/S0034-4257(00)00109-7).

Page 9 line 5: MODIS Collection 6.1 data have recently been released; at the time of writing, Terra data from 2000-2008 are freely available and the reprocessing up to the end of the authors' 2015 study period should be complete by mid-December. The C6/C6.1 changes in the Deep Blue and Dark Target land algorithms are not negligible. For example Deep Blue made some improvements over mountains and biomass burning aerosols, and Dark Target made some improvements over cities. All of these feature in the study region. Note that the merging logic in C6.1 is unchanged. C6.1 is expected to remain the current standard version for the coming several years. In an ideal world this analysis would therefore be performed with C6.1 data; I do realise though that repeating the analysis takes time. To start with I have attached a figure (as a Supplement) showing the multiannual monthly mean C6.1 Terra merged AOD and C6.1-C6 AOD difference for the period available today (2000-2008), for the authors' study region. As you can see there are some pretty significant systematic differences, e.g. over the Tibetan Plateau (TP) and mountains to the east of that in particular, but elsewhere as well (systematic decrease over the North China Plain in some months). Since the TP is one of the areas highlighted for regional analysis and where ATSR and MODIS (C6) climatologies were different, this significant change over the TP in C6.1 (decrease by more than 0.25 in some areas) is relevant to the analysis and discussion. Note in the attached figure the colour scales are truncated, i.e. some AODs are

above 1 and some differences larger than 0.25. As a result I do think that changing to the latest C6.1 would be beneficial. Otherwise the authors' analysis will shortly seem outdated; using C6.1 would instead make it really at the forefront. We don't yet have publications about the C6.1 changes, but user guides illustrating some changes are online at https://modis-atmosphere.gsfc.nasa.gov/documentation/collection-61 .

Section 3: As noted earlier, I do not think that annual/multiannual averages are particularly meaningful because of strong seasonal variation in AOD, aerosol loading, and sampling. However I do understand that the authors want to keep most of the seasonal analysis for the follow-up paper, and they do give caution about sampling etc in the discussion here. But to give a hint of where this may be important, Figures 2 and 3 could perhaps be expanded to include a little more information. For example, panels showing the number/fraction of months corresponding to the aggregates, the variability within a year (i.e. the mean over all years of the standard deviation of AOD from Jan-Dec), the interannual variability (i.e. the standard deviation of annual aggregates), etc. I think this would provide better context for the discussion of spatial features between ATSR and MODIS data here. As mentioned in my comment about section 2, suggesting inclusion of MISR data, if you're trying to assess whether the aggregates are giving the same picture or not then the more (relatively) independent data sets you can look at, and the more you can see where sampling is good and where it isn't, the better.

Page 11 lines 8-15: the authors created a merged MODIS AOD product based on Collection 5.1 data, to compare with that provided in Collection 6 (as no official merge was provided in Collection 5.1). This is a nice thing to see as we as an algorithm team have not directly published C5.1/C6 difference maps from the merged product. However at present this part is not well integrated into the paper. The text says that "These differences will be further discussed after evaluation of the satellite AOD products versus available AERONET reference AOD data.", but I could not find the further discussion in the paper. Was this removed in a draft, perhaps? If the authors want to show C5.1/C6 differences then my suggestion would be to add more discussion about how these

changes may affect previously-published studies and their conclusions. Otherwise it can probably be deleted since C5.1 is considered obsolete now.

Section 3.1.3: this section feels a bit disjointed from the rest. For example, why do we only have the thin strip of CALIPSO data from 35-45 N in Figure 5 rather than the whole study area? I think that better use could be made of CALIPSO's strengths here as well. For example it's good to see the seasonal vertical profile of extinction in Figure 6 across this strip, but showing something like depolarisation ratio or colour ratio or even aerosol type frequency would allow us to see the vertical structure and changing seasonal nature of aerosol composition, which we can't see from the extinction profile alone. I might also consider moving this part to fit in with the seasonal discussion near Figure 11.

Section 4: I like the validation shown, and the comparison between MODIS and ADV when both are collocated with AERONET in Figure 10. It would be good to add some statistics to Figure 10, similar to the other panels. Since the AERONET sites cover a range of aerosol and surface conditions, I think it would also be instructive to further separate the points in these figures to different regimes (e.g. compare dust/pollution dominated scenes, classified using AERONET AOD and Ångström exponent, maybe split sites by surface type). There is clearly variation in surface cover and aerosol type, and we know that algorithm errors are dependent on those factors, so it makes sense to look at the data that way. For example, in Figures 7 and 8 there are some high-AOD points with high biases and low biases. It may be that high are associated with one aerosol type and low another, or it may be that the data are unbiased but noisy. With the analysis as presented, we don't know. Understanding if there are systematic surface/aerosol type dependent errors in the ATSR and MODIS data here would also help understand the differences in seasonal and multiannual aggregates presented in the paper and then aid in the interpretation of the follow on part 2 paper. (I realise, and the authors state, that most of the AERONET sites are in low-lying veg-etated/suburban/urban areas, rather than deserts, so splitting by surface type may not

be feasible. But doing a simple cut based on AERONET AOD and Ångström exponent to separate into background, fine-mode dominated, and coarse-mode dominated aerosols should be possible.)

Section 5: although most of the seasonal analysis is to come in the follow up part 2 paper, some of it is included here. Figure 11 is very interesting (I saw also the online comment containing the correct seasonal panel ordering) as it reveals the large seasonality, going back to my earlier comment about multiannual aggregates alone not being useful. I think this figure though is also illustrative of my earlier point about needing to understand differences rather than just state them. Clearly both sensors see similar patterns and seasonality but there are offsets (and again the differences in C6.1 are relevant here). Looking at type-dependence of errors vs. AERONET (suggested above) helps answer this question. Adding different measurement types also helps. Right now we have ATSR (dual-view narrow swath) and MODIS (single-view broad swath). I think the authors should move the CALIPSO seasonal maps (currently Figure 5) to be an extra column of this Figure, and expand them to cover the whole spatial domain. That provides another different measurement technique (active curtain profiling by lidar). Adding MISR as a fourth would also help: it is multi-view like AATSR, but has more angles (9 vs. 2), and a different spectral range, which provides different information content. Put them all together and you have a nice ensemble of 4 different seasonal climatologies from different sensors and retrieval techniques, and you will really begin to see where the data are consistent and where they are not. That tells data users where to be cautious and data producers where to focus effort.

Figure 11: there are some strange artefacts near the northern end of the MODIS DJF panel (appears as SON in the Discussion paper). Are these really areas of zero AOD, or are these areas without valid retrievals, which should then appear white for missing data? I think it is more likely this is missing data; this should be checked. There's a similar smaller artefact along the Himalayas in the MODIS MAM/JJA panels (appear as DJF/MAM in the Discussion papers), near 35 N 80 E.

Section 6: I have mixed feelings about this section. I understand that temporal variations are one of the main topics of the follow on part 2 study by Sogacheva et al. Here the authors show that MODIS and ATSR time series are offset but have similar inter-annual variations. However these are annual aggregates across the whole of China so again the question is raised of how meaningful these numbers are (they are not representative of any one place in China or season so it is not clear what I would want to use these numbers to assess or calculate). In my view including it here opens a bit of a can of worms, while it is interesting to see. My suggestion is to remove Section 6 and save all discussion of time series and trends for the follow on paper. It is already fairly well signposted that seasonality and trends are the topics for the follow on, so I don't know that it is necessary to introduce that topic here. The end of the conclusion can be cut down to do this signposting as well.

Section 8: I think Sections 7 and 8 can be condensed into a single discussion and conclusion section. I don't see what is gained by having the separate section 8 since it is short, and most of what is in section 7 is fitting for a conclusion. The two are even somewhat repetitive, e.g. last sections mentioning time series will be discussed in Sogacheva et al. Condensing this down, as well as saving Section 6 for the follow-on, will help prevent the paper from getting too long if the analysis is expanded to cover some of my suggestions.

Please also note the supplement to this comment:
https://www.atmos-chem-phys-discuss.net/acp-2017-838/acp-2017-838-RC1-supplement.pdf

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Interactive comment on Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2017-838, 2017.