

Interactive comment on “A Study of the Longer Term Variation of Aerosol Optical Thickness and Direct Shortwave Aerosol Radiative Effect Trends Using MODIS and CERES” by Ricardo Alfaro-Contreras et al.

Anonymous Referee #1

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General comments and recommendation

This study looks at trends in aerosol optical thickness (AOT) from MODIS and MISR, as well as trends in aerosol shortwave direct radiative effect (DRE) from CERES data. CALIOP data are also used. This is in part an update of earlier work by some of the authors, updated using newer versions of the MODIS data, and in part a new analysis. The study is within scope of ACP and the methodology is fairly standard and reasonable. The topic is of relevance and interest.

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I did however find it a bit hard to read. Some sections are quite verbose and hard to pick out the key take-away messages. This is however in part the authors being thorough in comparing this analysis to their previous MODIS analysis, as well as in noting some limitations of one of the CERES data products. So it's hard to give advice on how to remedy this while keeping the analysis thorough (which is an aspect I definitely like). As a result I recommend publication after minor revisions, listed below, mostly to address writing style. There is however also one important statistical error in terms of discontinuous trends in Figure 11 which needs to be addressed to make the manuscript technically correct.

Specific comments

Title: MISR should be added here. Maybe CALIOP too? Or the authors could remove the specific sensor names and say "various satellite products" or something.

Title: "Longer term variation" is a bit clunky and, to me at least, implies longer than single-sensor records (which isn't what is discussed in this study). I guess the authors chose this wording to make a contrast with their previous studies, which were decadal? Perhaps "21st century variations" would be better, since the data start in 2000 or later?

Lines 103-104: a reference for MISR should be added here. I'm not sure what the best one is. Perhaps Kahn et al (JGR, 2010), which I think is the main validation study for this version of the data?

Line 109: As a minor point, the MODIS product doesn't do "spectral AOT retrievals". It retrieves AOT at 550 nm and the weighting between fine and coarse aerosol modes, for various mode combinations. Spectral AOT is derived from these parameters. I suggest something like "provides spectral AOT at seven wavelengths" or even just removing the bit about wavelengths, since only 550 nm (the main data product) is used in this study anyway.

Line 110: "increased resolution" isn't quite right here, since the data are coarser at the

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edge of the swath. I think the authors either mean “increased pixel size” or “decreased resolution”.

Line 160: This line says only data with $CP > 95\%$ are used, while line 182 says $CP > 99\%$ are used. Is this inconsistent or am I misunderstanding something here? If these are for two different parts of the analysis, why the different thresholds?

Lines 167-169: I’m not sure why the first part of this sentence is needed. I think it’s fine just to say the arithmetic mean MODIS AOT is used.

Line 186: There have been a large number of studies into cirrus contamination of MODIS AOT data, not just Toth et al (2013), and many were well before that paper. I suggest rewording this to make it clearer that was not the first study, and maybe cite some of those other ones too.

Lines 200-207: This paragraph doesn’t really fit in this Section, which is otherwise describing the data sets used. I think it should be broken out into a new section summarising how trends are calculated and assessed (i.e. construction of time series of monthly deseasonalized AOT anomalies). It would be useful to add a bit of brief information about these two significance methods here as well. For example the Weatherhead approach attempts to account for autocorrelation, which is important in some areas for monthly AOT time series.

Section 3.1: I think I understand what was done here but from the discussion and tables it isn’t always clear what results apply to what bit. My understanding is the authors (1) compare C5 trends to C6 trends (for 2000-2009) and (2) compare C5 trends to the Zhang and Reid (2010) trends, which used a ‘data assimilation (DA) grade’ version of the MODIS products. So in this way they assess whether differences are more because of the C5/C6 change or the fact that Zhang and Reid (2010) used the DA-grade product and there isn’t a C6 equivalent (that I know of) DA-grade product. To help with this I suggest restructuring this section as follows:

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1. Move the bit about how trends are calculated to a new section earlier in the paper (see prior comment about lines 200-207). This will help streamline the text by putting the methodology in a methodology section.

2. Remove the text defining regions from the main body, since regions are already defined in Table 2, where they're easier to read.

3. Split out the analysis into two separate subsections, one to compare C5 vs. C6 trends for the 2000-2009 period, the other to compare C5 trends with and without the DA process. (Alternatively, since the conclusion seems to be that the differences are mostly minor, you could put in a few sentences that you looked at it but didn't find that things had changed much, and then just cut out the rest of the section.)

Line 273: I guess the authors use 1000 data counts because the MODIS level 3 aerosol products don't provide a count of number of days per month, despite various requests over the years. It would be good to indicate briefly the main areas where this removes data, and what the typical variations of data volume are in other grid cells (e.g. are the results sensitive to the threshold choice, or do most grid cells have many times more than 1000 retrievals?). From Figure 2 it appears that for MODIS it doesn't remove (m)any ocean grid cells in the studied latitude range. For MISR the gaps are roughly where I'd expect from e.g. cloud patterns in the tropics.

Line 275: Remer et al (2006) was before the MODIS Collection 5 release was complete, and you are using Collection 6 data. I don't know of a similar study to Remer et al (2006) using Collection 6 data, so it's probably still fine to cite that study here, but may be worth noting that was for an older data product version.

Lines 282-285: are these area weighted or simple mean? This should be stated. 1 degree grid cells at high latitudes are a lot smaller in real terms than those at the Equator. It may not affect the offset and trends shown in the figure too much, but may affect the baseline global-average AOT, since AOT tends to be higher in the Equatorial belt due to continental outflow.

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Lines 300-316: This is an interesting and I think pretty reasonable way of addressing/correcting for potential calibration drift, so that's good that the authors have done so. The basic idea is that if there's a trend in a region that's expected to be stable, one can subtract that trend from apparent trends elsewhere. However a caveat here is that assumes that the calibration degradation propagates linearly into AOT. That is probably fine for areas with AOT close to that of the remote region used as a baseline. But for example a 3% change in reflectance may cause a certain change in AOT when the true AOT=0.1 as compared to at e.g. AOT=0.5, since the radiative transfer isn't linear in AOT. The correction might therefore be an under/over-correction in those higher-AOT areas. Again, there's probably no simple better way of approaching this so the method is reasonable to use here. But since many readers of the article might not be familiar with the underlying radiative transfer and retrieval algorithms, I think this caveat should be mentioned.

Lines 339-340: the authors state that "the rates of increase of aerosol loading have slowed down over the last five years" because trend estimates over the period 2000-2015 are less positive than those for 2000-2009. That is certainly one possibility, but the statement is unsupported by the evidence. The trends for both periods may be statistically distinct from zero, but are they statistically different from each other? That is the relevant factor here. Only if so can one say that that the trend has slowed. The reader can't tell if this is the case, since uncertainty estimates for the trends are not shown. I suggest the authors look into this and either add text supporting it (if the trends are statistically distinguishable from each other) or remove this text (if they're not).

Sections 3.2, 4: As a general comment related to the above, it would be good if the estimates of trend precision could be given in the text when specific numbers are mentioned. For example on line 428 the SWARE trends in a region are given as 39.8 and 43.7 W/m²/AOT for Aqua and Terra. Without uncertainty estimates on those numbers, we don't know if the 4 W/m²/AOT difference between the two sensors is significant or

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within the uncertainty of the data sets used. This is just one example, the comment extends throughout the paper. It doesn't necessarily need to be given for every statistic in the paper but when it is a key result or comparison between two quantities, it makes sense to consider the uncertainty estimates. I realise that often both WH and MK methods are used to estimate significance in this study; it probably doesn't matter too much which method is used when you're quoting these uncertainties for the above points (as I'm guessing they will be similar).

Section 5: This section says it compares results to other trend studies, but really it only compares results to other trend studies published by the same authors. There are a number of other regional/global trend analyses using satellite aerosol data which could be considered. For example various Mischenko group papers for AVHRR over ocean, Thomas (ACP 2010) for ATSR over ocean, Hsu (ACP 2012) for SeaWiFS land and ocean, Yoon (ACP 2011) for SeaWiFS regionally, Yoon (AMT 2012) for AERONET, Dey and Girolamo (JGR 2011) for MISR in India, Babu (JGR 2013) for Indian surface observations. It would be good to include some of these more independent studies in the discussion here. The point is there's a lot of work which has been done and is relevant to the discussion here but isn't acknowledged. Maybe there isn't space to include anything but the authors only self-citing here is a bit of a let down.

Figures 6, 7: I couldn't find a mention of how the black lines in panels a, c here were calculated. This should be added. Also, it seems like results like this are the basis for quoting an aerosol forcing efficiency in units of $W/m^2/AOT$. From the shape of these curves it looks a bit more like a logarithmic fit with a kink around $AOT=0.15$. I know people like to think in units of $W/m^2/AOT$ but perhaps this paper is a good place to point out that the relationships aren't really that linear. This is something which could be highlighted again in the Conclusions (either in list items 5, 6, or a new item).

Figure 11 and associated text: This bit needs further work. It is fine to show trends split by periods, but the discontinuity at the breakpoint is not physical; it implies a sudden jump in the system. Having a breakpoint discontinuity is a sign that the derived

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values are not robust. There are methods to identify breakpoints in trends, and fit a piecewise continuous trend, rather than an unphysical broken trend. (I think the Weatherhead paper mentioned may discuss this? If not then some of her other work.) The authors should repeat this part of the analysis using a continuous piecewise fit. It's quite possible that this may affect the conclusions. Even if you get a similar answer, it will be on firmer theoretical ground, so it is necessary to do otherwise the manuscript contains methodological errors.

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