

Interactive comment on “Global and regional trends in aerosol optical depth based on remote sensing products and pollutant emission estimates between 2000 and 2009” by A. de Meij et al.

Anonymous Referee #1

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This paper presents a global, decadal trend analysis of AOD retrieved from the MODIS and MISR sensors on board the Terra satellite, as well as from a selection of AERONET stations around the globe. The paper then relates the AOD trends to changes in emissions of various precursor species from a range of emission inventories. The paper is quite timely, in that it is only recently that global satellite aerosol datasets have begun to provide a sufficiently long time series to allow such trend analysis to be considered, and the attempt to tie the observed trends to changes in emissions is praiseworthy.

Unfortunately, the work itself is in need of extensive further work before it will be of an

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acceptable standard for publication in ACP. The paper is poorly organised, with at lot of repetition and waffle. It also has the feel of a rushed job, with many typographical and grammatical errors. The figures are not well designed and there are not enough of them (for instance, the authors discuss trends in single scatter albedo derived from MISR measurements repeatedly, but nowhere are these trends presented in a figure).

Furthermore, many of the results presented by the authors are of dubious relevance. Many of the trends, particularly for individual sites, are tiny. There is no indication of an uncertainty or confidence interval on these values, so they are essentially useless. This is especially so when the two or three independent measurements available appear to disagree.

Finally, the authors make no attempt to physically relate the changes in emissions (discussed at the end of the paper) to the observed trends in AOD. The paper's conclusions amount to little more than a statement that in regions where emissions of aerosol precursors have decreased over the past decade, a decreasing trend in AOD tends to be seen, and vice versa. Thus the authors fail to present anything new to the literature.

I therefore recommend that this paper is only considered for publication in ACP once the analysis has been greatly improved and the paper extensively revised, as detailed below.

General comments and suggestions

- Ensure that ALL acronyms are defined once, on their first use.
- The authors make extensive use of the terms level 1,2,3 when referring to satellite and AERONET data. It should be made clear in each instance, exactly what these terms mean. (i.e. level 1 is generally calibrated radiances on a grid determined by the instrument measurement system, when referring to satellite data, etc.)
- The methodology section is written essentially as a list of steps. This is fine, but

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it would be a lot clearer if presented as either a bulleted or numbered list.

- Note that there are two MODIS instruments, one on Terra and one on Aqua. Although the authors mention that MODIS is on Terra, they should explicitly state that the Aqua instrument is not used.
- The authors reference the global trend in oceanic AOD derived from AVHRR data (Mishchenko et al. 2007) – they may be interested in Thomas et al., *Atmos. Chem. Phys.*, 10, 4849-4866, 2010, which includes further analysis of this result.
- On a related point to the previous one, have the authors contacted the MODIS aerosol team regarding this work? If my memory serves me correctly, Lorraine Remer suggested that MODIS Terra might not have sufficiently good calibration stability for use in monitoring aerosol trends, at the 2010 EGU General Assembly (I suggest the authors check on this).
- AOD has a strong seasonal cycle in many regions, which the authors acknowledge several times. Care must be taken when attempting to fit a linear trend to a time series that is dominated by a cyclic variation, especially if the time series doesn't span an integer number of cycles. The authors don't provide details of how they have calculated their trends; have they accounted for the seasonal cycle in their calculations, or investigated it's potential impact?
- The authors need to be more quantitative in their analyses. Statements like "the trends are similar" are not sufficient. When comparing two measurements or two trends the authors need to provide a rigorous measure of whether they agree or not.
- Throughout the paper, figures are referred to as Fig. 2a, Fig. 5b etc. However, none of the figures are correspondingly labelled.

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- The authors have extensively used the absolute and percentage change in the AOD trend from 2000 to 2009 (in preference to the slope of their fitted trend line) to quantify the observed trends. This is fine in principle, but repeatedly referring to these values as a change in observed AOD (for example the first sentence of section 3.4.1, "In Fig. 5a, the change in MODIS Level 2 AOD between 2000 and 2009 is presented...") is incorrect. The difference between observed AOD between two years would be the difference between some average AOD value in those two years, not a trend calculated from a time series over the whole period.
- Section 3.1 is sloppy and requires a significant overhaul. Figure 2 essentially plots the same information in 3 slightly different ways, and the authors descriptions of the patterns seen in each of these plots is overly long and repetitive. Furthermore, the authors seem to be slightly confused as to what is plotted in the last two panels of Fig. 2, as they state that these panels show "the significance" of the trends, whereas the plots show the trends themselves. I suggest that the number of panels in this plot is reduced to four, showing the decade trends for MODIS and MISR followed by maps of the significance of these trends (or, alternatively, their uncertainty). A rewrite of section 3.1 based on this simplified figure should produce a more coherent and succinct description.
- I don't see the point of detailed analysis of MODIS and MISR AOD trends at specific AERONET sites when there is insufficient AERONET data to provide a comparison. Essentially, this is just rehashing the results presented in section 3.1, but for a small subset of single pixels scattered around the globe. I thus feel Section 3.2 is superfluous.
- I find Fig. 3 (and Fig. 5) virtually unreadable. If the authors want to show the trends seen by MODIS and MISR (and surely AERONET should be included as well) at these sites, I would suggest a miniature bar chart at the location of each station, with one bar each representing the MODIS, MISR and AERONET

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trends. Again, this should only be done where an AERONET trend is available. It is also very difficult to relate the plots to the names of the individual stations used elsewhere in the paper.

- In section 3.3 the authors are comparing the AOD trends seen by the satellites over specific AERONET stations with those derived from AERONET measurements themselves. This is a sensible thing to do, provided the AERONET measurements provide a complete enough time series (and the authors have filtered the data for this). Why do they then present the data as averages for each region in Fig. 4? What is the point this plot trying to make? By averaging over the AERONET stations in each region the details of the comparison are lost; but the sampling across the region doesn't appear sufficient for these plots to be considered representative of the regions overall.
- The comparisons presented in section 3.3 are not informative without uncertainties on the trends being compared. How is one to know if a changes in mean AOD of -30
- The authors also compute trends using both AERONET daily mean and monthly mean data in section 3.3. I approve of this, but the manuscript goes no further than saying the trends are mostly similar. How similar? Do they actually agree?
- In section 3.4, trends computed from MODIS level 2 and MODIS level 3 data are compared. Again, this is most certainly a worthwhile exercise, however I don't believe the approach taken by the authors is valid. Once again, the comparisons are done at the locations of AERONET stations. Although the trends from the AERONET stations themselves are mentioned, comparison is no more than stating whether the sign of the three data sets agree. A more quantitative analysis should be presented.
- A more fundamental problem is that the trends from level 2 and 3 MODIS data

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are not calculated for the same regions: for level 3 the closest 1x1 degree pixel to each AERONET station is used, while for level 2 it is data within a 15 km radius of the station. Thus it is not possible to determine whether the differences in the trends seen are due to the averaging used to generate level 3 data, or due to the differences in the area used in the trend analysis. The authors need to decide which of these potential sources of differences they want to investigate, and design their experiment accordingly.

- I am puzzled as to why the authors have included descriptions in the change in emissions inventories in section 3.4. The data in this section are samples from MODIS and AERONET taken at specific locations, whereas the discussions of the emissions relate to overall regions.
- The inclusion of chemical schemes detailing the conversion of gaseous precursors to aerosol constituents given in section 3.4.1 should appear in an introductory section, if at all. These reactions are well known and the references to Seinfeld and Pandis etc would be sufficient.
- On page 30757 the authors state that they don't know the sampling time of MODIS level 3 data. This statement is nonsensical; MODIS is in a sun synchronous orbit, and level 3 data is calculated from the same level 2 data the authors are using in their analysis.
- The final paragraph is, frankly, nonsense. The paper hasn't shown that consistent AOD trends have been derived from multiple datasets. Nor has it linked these trends to changes in emissions in any meaningful way. Finally, linking emissions to AOD using chemical transport models is a well established practice, not a "next step".