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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Changes in atmospheric aerosol loading retrieved from space based measurements during the past decade" *by* J. Yoon et al.

Anonymous Referee #2

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The study by Yoon et al. is interesting because it documents changes in aerosol optical thickness (AOT) throughout the world on a timescale of 5 to 10 years as measured from space, and validated with independent ground-based measurements. To their credit, the authors try to take into account the temporal representativeness of various satellite datasets. However, they miss the opportunity to provide useful, quantitative statements on the effects of temporal sampling on AOT trend estimates, and for this and other reasons (below) the paper is not as useful as I would have hoped.

Furthermore, the paper does not fulfil the ambition to attribute the observed changes in AOT to their underlying causes. Meaningful statements about attribution of the AOT

changes cannot be expected from the study by Yoon et al. because the relation between particulate matter emissions, aerosol formation from secondary sources, and the ultimate aerosol loading is complex, and the relation with AOT even more complex. The paper simply does not address these relationships, but only provides some rough sketches on what may have caused the changes in AOT, mainly calling on papers in the literature. To my opinion, the problem with the study by Yoon et al. is that the authors use too strong language that suggests that actual attribution has been achieved by the authors, whereas the reality is that they only cite a suite of peer-reviewed papers to guide interpretation of their results. That clearly falls short of 'attribution'.

For instance I find the abstract suggesting that AOT trends have been successfully attributed ("increase . . . over East China is observed and attributed to both the increase in industrial output and the Asian desert dust") claiming too much. Related to this, the complete discussion of Fig. 8 (section 6) is long-winded and merely anecdotal:

1. In Europe the authors report a reduction in AOT and associate that with reducing emissions: "AOT from industry and traffic sources decreases significantly (Marmer et al., 2007; Karnieli et al., 2009). This is attributed to the success of environmental regulation in the EU countries (Streets et al., 2003; Yoon et al., 2011, 2012; Hilboll et al., 2013)" (P26013, L21-22). But what is the relative reduction in European AOT (%/yr) and how does this compare to reported reductions in direct aerosol emissions (%/yr) over Europe? And what is the role of secondary aerosol formation from precursors such as sulphur and nitrogen oxides? What about changes in the composition of the aerosol mixture over time? Such changes may also have led to changes in AOT because of the differences in effective optical properties, apart from loading! Neither of these important aspects is being discussed in sufficient detail to warrant the statement that "This is attributed to the success of environmental regulation". The authors should either come up with a thorough investigation of the full emissions-to-AOT chain, or simply refrain from claims about attribution, and just report on the observed changes.

2. In China, it is unclear to what extent the increase of desert dust with time can

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explain the trend in overall AOT. For Japan (P26015) reductions in precursor emissions have been reported from the Bremen-group, but we see here an increase in AOT over Japan. The authors should investigate and demonstrate what has driven the increase, or refrain from vague attribution statements drawing on literature.

3. For the eastern USA, only papers are cited that handle changes in AOT, yet the claim is that the reported reductions in AOT are due to reduced emissions (P26016, lines 3-7). The authors provide no evidence that aerosol (precursor) emissions have actually decreased in the eastern USA. The authors should include Figures or tables to support the claim that the reported reductions are actually due to reduced aerosol emissions, or rule out other possibilities.

Some other important concerns

4. What is the bias in satellite AOT? The retrievals surely not just suffer from random errors. Does the bias (and random errors) remain constant over time? Different trends near coastlines are particularly suspect (i.e. as in Fig. 3(a)-(b)), especially since aerosol sources in these areas are the same for aerosol over land and sea. The assumed albedo climatologies in the retrievals may provide information on such biases. Can the authors rule out that the jumps in trends are not due to albedo artefacts?

5. Why haven't the authors determined the local trends for the ground-based instruments for all data and for the ground-based coincident with satellite retrievals only (fewer samples)? Such a comparison would immediately put a number on the differences in the trend from differences in temporal sampling!

6. Exclusive attribution of differences between ground-based and satellite retrievals to temporal sampling differences has not been sufficiently justified. The paper suggests a number of times that the AOT trend differences between MODIS Aqua and Terra are due to their differences in overpass time. But this is never actually shown. The authors should find out whether the AOT is structurally different at noon (Aqua) than in the morning (Terra).

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7. Why is the positive trend in Fig. 3(a)-(b) for high northern latitudes not explained? It is highly significant according to Fig. 6(a). Why not put a number on the ocean (section) trend by averaging over a larger area?

8. The conclusion section states (P26017, line 26) that "The positive impact of legislation in reducing AOT and improving air quality is unambiguously documented." This is not a conclusion following from the work done in this paper, but rather expresses an opinion based on the literature. Since very little has been done by the authors to make the link between reducing emissions and aerosol loading, I don't think such a sentence should be presented in the Conclusions section. The same holds for all of the text on page 26018, which reads as a personal opinion on the need to study climate change and develop new space-borne sensors, but really does not follow from the research described in the paper. I think the authors should revise this part thoroughly, cut the general statements, and focus what we have actually learned from their research.

Specific concerns

P26008, L11-14: the non-perfect temporal correlation between AERONET and satellite measurements and the differences between the retrieved AOTs is fully attributed to the 'limited sampling times'. I don't think the authors have made clear that ground-based AOT retrievals and satellite retrievals have no differences in 'spatial resolution'. AOT retrieved from the ground depends on the relative azimuth angle, so that also differences in spatial representativeness may have contributed to the AERONET-satellite differences. The authors should explain why they rule out differences in spatial resolution in causing trend differences.

P26009, L1-2: the authors should explain here how cloud screening leads to a reduction in the number of observations. What exact quantitative cloud filter criterion was used to screen the cloudy measurements?

P26009, L13: explain what the 'climatology' means here. Is it the climatological monthly mean derived from all instruments, or is it a per-instrument value?

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P26011, L12-13: it is unclear how representative Figure 4 is as an example of the outlier test. For which region and month does the Figure hold? How many measurements are –on average- retained after the outlier test?

P26011, L21: 5000 resampling iterations out of how many ($\pm$) samples in the total ensemble?

P26012, L9-19: a proper comparison of the capabilities of both the AERONET and satellite-based analyses would ensure consistent sampling (in time and space) of AERONET and satellite retrievals. It is unclear if this has been attempted. From the text, it appears as if AERONET daytime mean AOT has been used vs. satellite cloud-free AOT at particular overpass times. The authors should make clear what is exactly understood as an AERONET "actual trend". If AERONET is sampled in a consistent manner with the satellites, differences in temporal sampling can be largely ruled out, and remaining differences can be quantified and attributed to differences caused by errors in AERONET and satellite retrievals, and to differences in spatial representativity. The differences now shown in Fig. 5 may well be due to these issues, but also due to differences in temporal representativity.

P26012, L21-23: please substantiate why and how the smaller MISR swath might lead to a worse correlation between the MISR and the AERONET trends.

P26013, L3: please specify how the orbital drift would contribute to a low correlation between the AERONET and SEAWIFS/BAER-derived trends? To my opinion, such a cause could easily be resolved by consistent co-sampling of AERONET and SEAWIFS in time.

P26034, Fig. 5: the Figure could be improved if the some more information would be included, e.g. by colour-coding desert-trends vs. biomass burning region trends. Such a characterization could shed more light on why trends are sometimes positive in the satellite record, and negative in AERONET.

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P26013, L18: it is not clear from Figure 8 whether the trend for different instruments hold for the same periods. The time interval should be indicated in the text, and also in the caption of Fig. 8.

P26040, Figure 11: what do he individual data points represent? (Winter) monthly means in the 2003, 2004, 2005, 2006, 2007, 2008? There are 12 data points so they cannot be seasonal (3-month) means.

Interactive comment on Atmos. Chem. Phys. Discuss., 13, 26001, 2013.