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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.

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I read this paper with interest as the topic aligns closely with some of my own research. Now that we have decade-long satellite aerosol records it's nice that we can (try to) say something quantitative about how aerosols might be changing, and there's been a flurry of papers on this topic in recent years. I had a few comments on this study by Yoon et al which I hope that they and the reviewers might consider during this Discussion phase of the peer review.

The text on page 26003 (middle of the page), pointing out the sampling issues asso-

ciated with satellite aerosol optical depth retrievals, makes good points and it is worth articulating them. A few wording choices struck me as odd, however, such as describing these satellite instruments as 'experimental' when I'd consider them a proven technology (as imagers of this basic type have been flying for decades and being carried on with VIIRS; caveat being I'm not an engineer so maybe some subtlety escapes me). Perhaps the authors use the word 'experimental' as the sensors' primary purposes are mostly not aerosol remote sensing? If so, perhaps a reword to something like 'non-aerosol-focussed' would be better. The point about diurnal cycles is relevant and I thought I'd mention this recent paper, which provides an example of that (MODIS missing peak aerosol loading due to overpass times):

Kocha, C., P. Tulet, J.-P. Lafore, and C. Flamant (2013), The importance of the diurnal cycle of Aerosol Optical Depth in West Africa, Geophys. Res. Lett., 40, 785–790, doi:10.1002/grl.50143.

Another recent paper makes some points about diurnal cycles and the radiative effects of aerosol:

Arola, A., Eck, T. F., Huttunen, J., Lehtinen, K. E. J., Lindfors, A. V., Myhre, G., Smirnov, A., Tripathi, S. N., and Yu, H.: Influence of observed diurnal cycles of aerosol optical depth on aerosol direct radiative effect, Atmos. Chem. Phys., 13, 7895-7901, doi:10.5194/acp-13-7895-2013, 2013.

I don't know that the question of aerosol diurnal variability has been entirely solved. However, over the time range for morning and afternoon satellites, I was under the impression that in many cases it is not large. See e.g. this paper for more information:

Smirnov, A., B. N. Holben, T. F. Eck, I. Slutsker, B. Chatenet, and R. T. Pinker, Diurnal variability of aerosol optical depth observed at AERONET (Aerosol Robotic Network) sites, Geophys. Res. Lett., 29(23), 2115, doi:10.1029/2002GL016305, 2002.

Anyway, my point here is that there is a distinction between the aerosol diurnal cycle

overall (which may be large in some cases, and is in at least some, as Kocha et al present), and the specific part of the diurnal cycle which is sampled by your specific satellite sensors (here about 10:30 am to 1:30 pm) and is likely to (in many cases) be small. With that in mind, I'd found it odd that authors mentioned the overpass time difference as the first (and by inference most important, although perhaps that's just my reading) sampling problem for trend/change detection. I'd have put things like calibration stability or cloud coverage first. On the topic of calibration stability, for SeaWiFS this was very good and we also checked temporal stability of AERONET validation at long-term sites as a sanity check in our previous study (Hsu et al, ACP, 2012; some references about SeaWiFS radiometric performance are given in there). Also relevant are things like inconsistency of spectral bands and spatial resolution between different sensors, which mean that applying consistent retrieval algorithms to their measurements is difficult or impossible.

Going back to overpass time for a moment, the wide swath of MODIS/AVHRR/VIIRS means that the local time at opposite edges of the swath will be significantly different from the nominal equatorial crossing time at the centre of the swath. For trend analyses from these instruments, I suspect any diurnal variation across these would cancel out given sufficient sampling.

Page 26005-26006: why do you cite an IOCCG report as a reference for the MODIS, MISR, and SeaWiFS instruments? These seem out of place and not really relevant as general introductions to these sensors. I also couldn't find the reference in the bibliography. I'd suggest just removing it as the textual descriptions and other cited references are sufficient.

For MODIS, it seems as though the authors are not using the Deep Blue dataset (see e.g. the hole over the Sahara in presented maps). I'd encourage them to use Deep Blue to fill some of the Dark Target gaps: the dataset is stored in the same files as the authors must already have, and it'd provide a useful backup to the other sensors. Our own SeaWiFS-based analysis (Hsu et al, ACP, 2012, which is cited but results not

discussed) found some strong positive trends over the Arabian Peninsula, and it looks like the same is seen in this study with e.g. MISR. So by including MODIS Deep Blue you'd have an additional check there. In Figure 3 the authors note that the Middle East lacks MODIS data but this is a problem which would be easily remedied via inclusion of Deep Blue data into that record. I would be happy to advise the authors regarding Deep Blue data use.

Page 26013: the authors state that SeaWiFS orbital time drift is one potential reason for weaker trends observed by SeaWiFS than AERONET. Based on the previous papers I linked to on diurnal variability, I doubt that's a significant cause.

The regional results and discussion in the study are interesting. However many of these results were reported previously by Hsu et al (ACP, 2012) and de Meij et al (Atmos Env, 2012), which are cited parenthetically in the introduction to the study, but not discussed in the text. In fairness to the previous work, I would have liked to see the authors compare their estimates to the existing prior research on this subject, and discuss similarities/discrepancies. See, for example, Figure 9 of Hsu et al (2012) in comparison to Figure 8 of the present study. My eyeballing suggests that some of the results are quantitatively similar to those of Hsu et al (2012), which would be quite a nice achievement in my view (similar results with different algorithms). Perhaps the authors could even add the NASA SeaWiFS product to their own analysis (I'd be happy to provide assistance regarding data use) and show these things quantitatively side by side. de Meij et al (2012) also looked at emissions as possible reasons for aerosol trends, and again discussing their results would be relevant to the topic here.

In any case, this is a complicated topic, and no single study gives us the final word on aerosol trends, so I think it's important to present comparisons between these different analyses to figure out whether the different studies are reporting consistent results. If they are consistent, great (but we still have to be careful as many trend analyses are based on the same underling datasets so may not be truly independent analyses) and if they're not consistent, maybe we can figure out why. The lack of this comparative dis-

cussion of the trends is my main issue with the paper. It's important to place research in the proper context.

I hope the authors and reviewers find these comments useful, and please don't hesitate to get in touch if you'd like some assistance with NASA SeaWiFS or MODIS Deep Blue data products.

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