Atmos. Chem. Phys. Discuss., 12, C4480–C4482, 2012
www.atmos-chem-phys-discuss.net/12/C4480/2012/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Global and regional trends of aerosol optical depth over land and ocean using SeaWiFS measurements from 1997 to 2010" *by* N. C. Hsu et al.

Anonymous Referee #3

Received and published: 10 July 2012

This paper investigates SeaWIFS-based retrievals of aerosol optical depth (AOD) over a 13 yr period to evaluate and determine trends in optical transmission, both globally and regionally. The results are qualified with respect to long-wave/period atmospheric circulatory phenomena, in order to segregate trend signal relative to background-induced variability. The authors report what they consider significant increasing AOD trends globally and over most regions. Instrument effects, both from SeaWIFS and AERONET, are considered negligible.

I want to preface this review by stating that this work represents the cutting edge of aerosol science available from present satellite monitoring and the long-term datasets

that have been collected. This is thus an important and highly-relevant report, and I believe that the work here should absolutely be published in the literature. However, there is a component of the scientific method here that I feel must be addressed before this paper should in fact be accepted. As such, and I will expand on my feelings below, I encourage the Editor to request a revision for method. Technically, the paper is sound. I have numerous minor editing/comments that I am attaching to this review for the authors to consider.

The binding paper of record on AOD trend analysis is that of Zhang and Reid (2010 - also ACP) from ten years of MODIS. I'm neither Zhang or Reid. But, they consider the work of Weatherhead et al. (1998) and Weatherhead (2003) in order to statistically demonstrate what a trend was, and how to determine whether or not the trend was significant. They do this in their Section 3, and throughout the text are deferent to Betsy's methodology; most notably in Table 1, which shows the ratio of trend vs. standard deviation and its relation to 2.0 being the threshold for 95% signficance. As such, their report and results are statistically and quantitatively robust with respect to the literature. The narrative is strong, convincing and consistent.

This paper and narrative do not presently demonstrate such rigid quantitative evaluation and consistency. Yes, there is discussion of statistical methods in Section 3. Yes, in Table 1, there are trend and std error (is that stddev?) reported. The authors do refer to thresholds for 90% and 95% significance at numerous spots. But, they never say how they actually derive this, or why its relevant. The reader is left to presume that it actually means something. Rather, in many spots, there is tangential discussion about regional and circulatory anomalies within the record, for which they cannot devolve, never convincingly correlate with respect to impact on their results (though Sec. 4.1 is a very interesting read), and ultimately feels more like simple hand waving. Error bars are not given with Figs. 7, 10 and 12. Black dots, corresponding with a 95% confidence level, are not legible in Figs. 6, 8 and 9, and its really not clear in either of those figures how the result relates to the statistical confidence threshold anyway.

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I'm really not trying to be super negative here. This is a very good paper. I'm trying to constructively point out to the authors that, whereas I'm not necessarily advocating that they follow Zhang and Reid (2010) step for step, I strongly believe that the Weatherhead methodology therein very much represents the core argument you're trying to make. Therefore, I encourage you guys to thus revise the narrative so as to strengthen the quantitative argument that her work allows, as its already mathematically consistent with the methods that I believe you're applying presently. Leave the tangential discussion, which you cannot quantitatively decouple anyway, out, and allow the math to guide the narrative toward your admirable conclusions.

One final point. I disagree with your statement at the end of Sect. 4.1. Trend determination is a function of signal versus noise. If you're saying that climatic forces impart uncertainty, then this is reflected in the standard deviation. Physically, climate forcing is not doing any such thing that you guys state here, and I believe this to be a very important sentence in the paper. The variability that you are describing acts to lower the significance of your analysis, in the absence of an overwhelmingly large sample. Make the statistical argument, not the qualitative/forcing argument here and elsewhere in the narrative.

Please also note the supplement to this comment:
http://www.atmos-chem-phys-discuss.net/12/C4480/2012/acpd-12-C4480-2012-supplement.pdf

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Interactive comment on Atmos. Chem. Phys. Discuss., 12, 8465, 2012.