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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Analysis of linear long-term trend of aerosol optical thickness derived from SeaWiFS using BAER over Europe and South China" *by* J. Yoon et al.

Anonymous Referee #1

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Summary, general comments, and recommendation

This paper uses aerosol retrievals derived from SeaWiFS measurements from October 1997 to May 2008 to investigate trends in aerosol optical thickness (AOT) at 443 nm and 555 nm over Europe and southern China. AERONET data are also used to validate the aerosol retrievals, and compare the trends. This is an extension of a previous study (von Hoyningen-Huene et al., AMT, 2011), which described the BAER retrieval algorithm and presented an earlier version of the trend analysis. The main results of this work are that the retrieved AOT is comparable in quality to other satellite datasets; that there are decreasing trends in AOT found over Europe; and that there

are increasing trends in AOT found over the Pearl River delta.

The topic of AOT trends is an important one, and as high-quality satellite records become longer they become more useful for detection of trends. SeaWiFS provides (currently) the longest suitable single-sensor record, from 1997-2010. The paper is therefore of scientific interest, although the topic of trend analysis is a very complicated one, as the authors mention.

My opinion is that the paper presents some good results, although needs some clarifications and an extended analysis, which will strengthen the conclusions which are drawn. I suggest below using the full SeaWiFS time series, which will improve the ability to detect trends. Additionally, I feel the paper would benefit from a more thorough discussion of some aspects. I therefore recommend that the paper is only accepted following revisions, to address the comments as listed below. I think these revisions would strengthen the paper but should not take too much time to perform.

Specific comments

Abstract, line 15: I would say this is a relative difference, not relative error.

Page 20759: The Along-Track Scanning Radiometers (ATSRs) are not mentioned in your list, and have also been used to examine aerosol trends (your lines 16-18 mention AVHRR, TOMS/OMI, and MODIS only). See for example Thomas et al. (2010), which also has some commentary on the AVHRR trends. You also do not mention MISR in this sentence, which has been used for regional trend studies over land and ocean, e.g. Dey and Di Girolamo (2011).

Page 20760, line 24-26: some of these uncertainties are slightly incorrect. For example, the MODIS dark target algorithm has an uncertainty of 0.05 + 15 % per the references cited. While this approaches 15 % for a very high AOT, for a typical AOT around 0.2 or less, this is more like a 40 % or more error. Similarly, the MISR error estimate is the greater of 20 % or 0.05. I am not certain about TOMS/OMI on a global scale. I would rewrite this sentence to illustrate your point that aerosol retrieval is difficult.

Page 20761, line 15: Should be "These" centers of population.

Page 20763, line 9: While SeaWiFS indeed had very good calibration, as far as I understood it was not that good, with an absolute uncertainty of order 2 %-3 %. I do not know where the numbers from Li et al. (2009) come from. See, for example, Eplee et al (2007) or Franz et al (2007), and others.

Page 20763, line 10: I do not think Figure 2 is necessary, particularly since a similar image was shown in the von Honyningen-Huene et al (2011) reference. Also, which version of SeaDAS are you using to obtain the calibration coefficients? There are some changes between older and the current version (SeaDAS 6.2).

Page 20763, line 15: Why do you stop at May 2008? Since this is a paper about trends, and SeaWiFS gives us the longest time-series yet, it would make sense to use the whole dataset (i.e. end in December 2010). The extra two and a half years of data would help your trend significance calculations. I suggest the authors extend the analyses presented with these extra years of data. The SeaWiFS webpage (*http://oceancolor.gsfc.nasa.gov*) provides tools to search for and subset the SeaWiFS

data to only those scenes of interest, so it should not involve a considerable computational burden, and would strengthen the paper a lot.

Page 20764, line 5-7: I would delete this sentence as you essentially repeat it on page 20765, line 5-7.

Page 20764, lines 24-25: Please describe the colocation criteria used for the matchup between AERONET and BAER data.

Page 20764, line 25: You only show validation results for four European stations. Why is there no validation presented for the Asian region studied in the paper, and what about the other Eastern Europe sites (and Hong Kong) in Table 1 which you use to examine aerosol size distributions? There are many more sites available in and around the regions you study than those you use (see *http://aeronet.gsfc.nasa.gov*). Since it is known that certain regions are easier to retrieve aerosol for than others, the analysis would be much more convincing if the AERONET validation were performed for all available sites in your region of interest. For example, the optical properties of East Asian aerosols can be quite complicated and so make retrieval more difficult. You do not need to add lots of new figures, but I would suggest adding a table summarising the statistics of the comparison at each site, plus a figure for the Hong Kong site. Without an extended validation, the reliability of the trends is uncertain.

Section 3.2: I am not convinced that the linear trend model is appropriate for use, given the seasonality in AOT. If I have understood correctly, the effect of some of the noise will be removed by the noise autoregression. But you are still fitting a straight line to something which is fundamentally periodic, and the seasonal cycle is in many cases larger than the underlying trend in AOT. The trend calculation will also be influenced

by the facts that the start and end months are at different points in the annual cycle, and outliers or missing data in those months at the peaks and troughs of the seasonal cycle can easily also introduct artefacts into the trends. These issues are removed to an extent when trends are analysed on a seasonal basis, but I think that the whole-year trends reported are not so meaningful. I suggest the authors take an approach such as one of the following three here (at a minimum, some additional acknowledgement about the issues of a linear fit would be useful):

- Deseasonalise the data in some way (e.g. calculate the mean January, February, March etc.), subtract these values to create a time series of the monthly anomaly, and then fit linear trends to this anomaly.
- Fit a more complex function, such as a sum of a straight line and a cosine with a trend (this approach was taken in Thomas et al., 2010).
- Deal only with monthly or seasonal trends because of the issues of a whole-year linear fit.

Page 20767, lines 20-25: How are you calculating the data for the time series? Is it a simple average of all retrievals in the region in the month? Or do you average the data by day, and then average all days together? Is there any weighting applied? This should be stated in the text. Did you consider e.g. using medians rather than means?

Page 20767, lines 18-26: If I have understood correctly, if there were fewer than 5 BAER days in a month, or fewer than 10 AERONET, then you instead use the other dataset's value and the equations of best fit between the two datasets for that month. Is that correct? I think this paragraph could be clearer.

Page 20768, lines 20-25: I would just write the absolute trends rather than the percentage. You give percentages in the table already, and the absolute are more

meaningful, so just giving absolute trends in the text makes it more readable.

Page 20769: I think the main utility of a seasonal analysis is actually that the linear model becomes more appropriate. I am not convinced by your argument that positive trends are caused by clouds. I think this statement needs more explanation and evidence. Surely this would only be the case if there was a positive trend in cloud contamination? A positive trend in summer could e.g. be caused by a strengthening of the seasonality (high summer AOT getting even higher), for example. I do not think your statement that the Pearl River summer trend is definitively not real despite high significance is convincing. You are right to point out issues relating to seasonal sampling.

Figure 11 and associated discussion: I do not think that in the present state this section adds significantly to the content of the manuscript. I would suggest expanding the discussion. For example, how many points are going into these seasonal averages? Are they the simple means of the results? How do these aerosol properties compare to those you are assuming in the retrieval? If there are differences, are these related to the differences observed in the validation and trend analyses?

I have two further points which were not addressed in the manuscript, but should be mentioned:

- Many of these trends are on the order of a few percent in AOT. For typical surface brightnesses, what calibration change would be necessary to give a trend of this magnitude? I guess the question I am asking is, how confident can we be that these trends are not artefacts of uncharacterised sensor degradation?
- SeaWiFS drifted in orbit from an overpass around noon until about 2:30 pm.

There is therefore potential for errors in the BAER trend analysis relating to changes in temporal sampling, particularly where there is a strong diurnal cycle. As well as changes in aerosol, sampling could be affected by diurnal changes of cloud cover. See for example Meskhidze et al. (2009), who found differences in cloud properties between the two MODIS sensors. A glance at that paper suggests this might be more important for the Asian than European region studied. I would suggest examining whether there is a strong diurnal cycle observed in the AERONET data between these hours, to investigate whether the change in sampling time could be important for these regions.

Table 1: I would move the text about the purpose of AERONET data into the figure caption, rather than as a subscript.

Table 2: I would suggest writing significant trends in bold, or something like that, to make it easier to spot which are significant.

Figure 10: This would look better with bars in color.

Finally, I suggest adding a new figure, showing the seasonal average AOT from BAER for each of the two main regions. So, you could have one set of maps for the European regions and one for the Pearl River region. This would help illustrate the spatial distribution of aerosols retrieved by your algorithm.

Additional references used in this review

- Dey, S., and L. Di Girolamo (2011), A decade of change in aerosol properties over the Indian subcontinent, Geophys. Res. Lett., 38, L14811, doi:10.1029/2011GL048153.
- Eplee Jr., R. E., Patt, F. S., Barnes, R. A., and McClain, C. R. (2007). SeaWiFS long-term solar diffuser reflectance and sensor noise analyses. Applied Optics, 46: (5) 762-773.
- Franz, B. A., Bailey, S. W., Werdell, P. J., and McClain, C. R. (2007). Sensor-independent Approach to the Vicarious Calibration of Satellite Ocean Color Radiometry. Applied Optics, 46: (22) 5068-5082.
- Meskhidze, N., Remer, L. A., Platnick, S., Negrón Juárez, R., Lichtenberger, A. M., and Aiyyer, A. R (2009). Exploring the differences in cloud properties observed by the Terra and Aqua MODIS Sensors, Atmos. Chem. Phys., 9, 3461-3475, doi:10.5194/acp-9-3461-2009.
- Thomas, G. E., Poulsen, C. A., Siddans, R., Sayer, A. M., Carboni, E., Marsh, S. H., Dean, S. M., Grainger, R. G., and Lawrence, B. N. (2010) Validation of the GRAPE single view aerosol retrieval for ATSR-2 and insights into the long term global AOD trend over the ocean, Atmos. Chem. Phys., 10, 4849-4866, doi:10.5194/acp-10-4849-2010.

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