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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.

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Dear anonymous referee,

Thanks for your suggestions and comments to improve this discussion paper. The purposes of the discussion paper are not only to investigate trends in aerosol optical thickness (AOT) at 443 nm and 555 nm over Europe and southern China, but also to show the uncertainty caused by cloud disturbance in the trend analysis of cloud-free aerosol.

Q1. Abstract, line 15: I would say this is a relative difference, not relative error.

A: We will modify it.

Q2. 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).

A: We will check and include them in the introduction part as additional references related to the AOT trend analysis using ATSRs and MISR.

Q3. 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.

A: We will rewrite the sentences related with aerosol retrieval the accuracy as you recommend.

Q4. Page 20761, line 15: Should be "These" centers of population.

A: We will modify it.

Q5. 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.

A: As you recommend, we will check the statements about the SeaWiFS calibration in Li et al. (2009), Eplee et al (2007), and Franz et al (2007).

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Q6. 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).

A: There is not calibration information for channel 5 and 6 in von Honyningen-Huene et al (2011). So, we would like to keep Figure 2 in this paper. By the way, with respect to your question about version of SeaDAS, we didn't use SeaDAS in this study, but used the latest calibration table, which had been provided by Dr. Eplee (NASA SeaWiFS science team).

Q7. 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.

A: 1. Even though some data observed by SeaWiFS are provided until 2010, there is a lot of missing observations from 2008 to 2010 as you can see in Q7-Figure 1 (from Patt (2010)). If we do analyze the AOT trend including this period (2008-2010), we expect a serious bias caused by missing data.

2. SeaWiFS has a shift in the measurement time (caused by orbital drift), which started in around 2002 (see the Q7-Figure 2). This could induce significant uncertainties in the trend analysis. So, we used the SeaWiFS data from October 1997 to May 2008 in order to minimize such bias because the time shift until 2007 remains moderate (around one hour).

Q8. Page 20764, line 5-7: I would delete this sentence as you essentially repeat it on

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page 20765, line 5-7.

A: We will skip it.

Q9. Page 20764, lines 24-25: Please describe the colocation criteria used for the matchup between AERONET and BAER data.

A: In order to make a comparison between AERONET and BAER data, we used the temporal averages of AERONET AOTs within $\pm$ 30 minutes compared with the Sea-WiFS over-passing time, and the spatial averages of 0.12°âĚź0.12° pixels of BAER AOTs compared with the location of AERONET station. We will also mention this in the paper.

Q10. 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.

A: As you mention, there are a lot of global AERONET stations (roughly over 850). However, we could not use all AERONET station within the defined regions in this paper.

1. Not all stations distribute sufficiently large temporal records, which are suitable for the validation of AOT and AOT trends. Specifically, it is difficult to make a validation at

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Hong_Kong_PolyU station in Pearl River Delta because the matchup points between AERONET and BAER AOTs are only two. Furthermore, long-term and continuous data are highly required in order to make a validation of AOT trends. As you see the Q10-Table 1, there are not many global AERONET stations, which could be used for the trend validation.

2. BAER has still limitations to retrieve AOT using SeaWiFS over some regions, which are affected by frequent cloud disturbance or/and high surface reflectance. Especially, at higher latitude in Eastern Mediterranean it is difficult to compare BAER with AERONET AOTs.

Therefore, the only four AERONET stations, which have been chosen in the paper, could be used for validation of AOT and AOT trend. Of course, you definitely have a question about the BAER's retrieval accuracy over South China. Related to this, please check the Q10-Figure 3 and 4, which were shown as validation results of the BAER AOTs against various AERONET stations (Lee et al., 2004; von Hoyningen-Huene et al., 2011).

Q11. 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):

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1. 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.

2. 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).

3. Deal only with monthly or seasonal trends because of the issues of a whole-year linear fit.

A: Linear trend model is not perfect to handle whole-year data. However, there are some reasons why it is difficult to apply your first and second suggestions. Following the third suggestion, we will discuss the trends in more detail.

1. As we have mentioned in the paper, some monthly AOT averages during cloudy seasons have problems with respect to the statistic quality (i.e., the mean value is calculated by a too small number of observations). Therefore, based on these data including the "inferior" monthly values, it is difficult to discuss the seasonal AOT cycle even though there is clear seasonality of AOT. As we have shown in Figure 8 in the paper, we found that some monthly averages in cloudy seasons are one of main uncertainties in AOT trend analysis. So, we think that it's difficult to apply the first and second suggestions.

2. Now, we are testing a new method to consider the cloud disturbance in the AOT trend using AERONET data like Q11-Figure 5 (Yoon et al., 2011). That is a weighted least squares regression, which have a weighting factor defined as ratio of monthly observation number and monthly standard deviation. After testing this method successfully, we plan to apply it to the global SeaWiFS AOT results.

Q12. 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

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should be stated in the text. Did you consider e.g. using medians rather than means?

A: We calculated the simple monthly mean of all retrievals within the regions. There is no weighting for averaging.

Q13. 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.

A: Yes, you are right. If some monthly averages are calculated with very small number of observations (fewer than 5 BAER days in a month, or fewer than 10 AERONET), we remove them. Next, the missing values are recovered by the calculation from other dataset using the correlation plot from validation between AERONET and BAER AOTs. Through this approach, we receive a better agreement between BAER and AERONET AOT trends. This means that it is difficult to compare directly between the AOT trends from ground- and satellite-based observations and that the cloud disturbance has to be considered in the trend analysis of cloud-free aerosol. We will try to make it more clear in the paper.

Q14. 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.

A: We will delete the percentage trend in AOT from the sentence.

Q15. 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

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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.

A: Linear trend is more appropriate for seasonal analysis, and there have been many explanations about positive/negative AOT trends in previous studies. We will also mention them in detail. However in this paper, we highlighted the cloud disturbance, which was not discussed in previous papers. As we have shown in Figure 8 in the paper, we found that some trend in cloudy seasons induces big problems. However, we did not deduce that some trends are caused by clouds, but rather suggested that the cloudy season trend is easily contaminated by cloud disturbance (not only overestimated AOT in AOT retrieval, but also poorly representative due to less observations), so that it might be ignored to receive a more reliable total trend.

Q16. 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?

A: We will discuss it in more detail in the paper.

Q17. I have two further points which were not addressed in the manuscript, but should be mentioned:

1. 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?

2. SeaWiFS drifted in orbit from an overpass around noon until about 2:30 pm.

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A: The retrieval accuracy is very important factor, which have been already discussed many times in the previous studies. Especially, related to the observation time shift which is one significant uncertainty factor in the AOT trend analysis, answered in Q7. We will discuss them more detail in the paper.

Q18. 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.

A: Absolutely, there could be significant influence in the AOT trend if the AOT in the region is following a strong diurnal cycle. The trends can be quite different due to the strong diurnal cycle and cloud disturbance. Q18-Figure 6 shows two examples of AOT diurnal cycles from AERONET (Beijing and Ispra). Ispra follows a weak, but Beijing a strong cycle. Unfortunately, there are not many AERONET stations, which can be used for AOT diurnal cycle investigations (please, see the answer for Q10). So, we will discuss it using Smirnov et al. (2002), which investigated the diurnal variability of AOT observed at AERONET sites.

Q19. Table 1: I would move the text about the purpose of AERONET data into the figure caption, rather than as a subscript.

A: We will move the text into the figure caption.

Q20. Table 2: I would suggest writing significant trends in bold, or something like that, to make it easier to spot which are significant.

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A: We will make bold character for showing significant. In addition, we also show the results in cloudy season as italic or other.

Q21. Figure 10: This would look better with bars in color.

A: We will change the bar chart in color like Q21-Figure 7.

Q22. 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.

A: We will show the seasonal AOT (440nm) distribution over the specific regions like Q22-Figure 8.

Reference

1. Lee, K. H., Kim, Y. J., and von Hoyningen-Huene, W.: Estimation of aerosol optical thickness over northeast Asia from Sea-Viewing Wide Field-of-View Sensor (SeaWiFS) data during the 2001 ACE-Asia intensive observation period, J. Geophys. Res., 109, D19S16, doi:10.1029/2003JD004126, 2004.

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4. Yoon, J., von Hoyningen-Huene, W., Kokhanovsky, A. A., Vountas, M., and Burrows, J. P.: Trend analysis of the Aerosol Optical Thickness and Ångström Exponent derived from the global AERONET spectral observations, Atmospheric Measurement Techniques Discussions, 4, 5325–5388, doi:10.5194/amtd-4-5325-2011, 2011.

5. von Hoyningen-Huene, W., Yoon, J., Vountas, M., Istomina, L. G., Rohen, G.,

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Please also note the supplement to this comment:
http://www.atmos-chem-phys-discuss.net/11/C9411/2011/acpd-11-C9411-2011-supplement.pdf

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 20757, 2011.