

Interactive comment on “Characterization of atmospheric aerosol in the US Southeast from ground- and space-based measurements over the past decade” by E. J. Alston et al.

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Dear Referee #1,

We would like to thank you for your time in reviewing our manuscript. We appreciate your insights and questions. We believe in addressing your comments our paper will become stronger and have a better appeal to the audience of this journal. We have carefully and thoughtfully considered every point you made. We will address your comments/concerns in the order given.

First, we would like to briefly reiterate the scientific merit and timeliness of our research
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paper:

- We analyzed 10 years of daily satellite data from two different sensors (8 years for one sensor) and ground-based observations of aerosols in the Southeastern U. S., which has not been previously done for this specific region.
- We quantified the impact seasonality (e.g. spring and summer) has on aerosol observations.
- We related decreasing trends of satellite AOD to decreasing trends in aerosols measured at the surface for this region.

While we recognize that this type of analysis is not novel in its approach; it is novel in its application to this region and for this time period.

Major Comments:

Referee comment #1: The authors see substantially different patterns and trends to G09. Further justification and investigation is required. The authors suggest that the difference they see in spatial patterns of AOD compared to G09 is due to the use of Level 2 vs. Level 3 data, as well as different years – this could easily be verified by the authors. The discussion in the Conclusions section is unconvincing. The authors indicate that the temperature record in the region is flat, but need to provide details on whether the cited study was for the exact same region and time period. It is also unclear how what they have shown disputes the G09 results as the authors have shown here that there is a significant trend only at the surface and the total column AOD trend is non-existent or muted. Thus, it appears that aerosol aloft could behave different from at the surface (in agreement with G09), and in particular they cannot discount the possibility of a layer of organic aerosol aloft. And thus that surface $PM_{2.5}$ is responding to decreasing anthropogenic emissions, but that total column aerosol trends include additional factors.

Response: The cited temperature study (Menne et al., 2009) covers the contiguous U.S. for 1895 – 2007, and their analysis is based on actual surface temperature measurements instead of a derived global atmospheric data assimilation product cited by

Goldstein et al., 2009 (e.g., Kalnay et al., 1996). Ultimately our papers differ on their respective foci. We sought to understand the interannual and seasonal behavior of aerosols in the Southeastern U. S. over the past decade with analysis of this behavior over time. Whereas Goldstein et al. related increased AOD in the region to increased radiative cooling in the region with no discussion as to this behavior over time. Respectfully, we do not feel that our results completely dispute those of Goldstein et al., but rather put their work in more encompassing context. Our analysis never sought to credit/discredit whether there was a layer of organics aloft, but rather to show that is our belief that if there is a layer of organics aloft, it is not the primary driver of aerosol concentrations over time as other research has shown the effectiveness of air quality controls upon aerosol concentrations. We feel that is these policies that are main driver of aerosol behavior over time. Additionally, initial results from our forthcoming paper (see response to comment #4) using CALIPSO lidar data from 2009 does not appear to support the case for an elevated layer of aerosols aloft. A major finding of this research is that during the summer there appears to be only one aerosol layer that is approximately 2km thick from the surface. We have changed our conclusions to better communicate how our results complement and contrast with Goldstein et al.

The last area of difference between the studies is the AOD maps. The use of three additional years does indeed impact the seasonal averages; however, the addition of years does not take away from the impact of using a finer resolution product. The Level 3 product has a much smoother appearance and the metropolitan Atlanta area cannot be discerned at that scale. Please refer to Fig 1 on the Goldstein et al. paper, by using the Level 3 product implies that the entire Eastern region is experiencing uniformly elevated AOD, and our results using Level 2 data show much more variation than can be gleaned from the Level 3 product.

Referee comment #2: Page 7576, line 22: Given that only 1 of 3 satellite records show a decreasing trend, it is false to say that this analysis presents evidence of solar brightening.

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Response: We recalculated the trends to be on a per year basis, please see the updated Table 1 and Figure 9a–9c. Our statistical analysis incorporates testing of the slope and error terms as well. Thus we feel that our assertion that the region is experiencing solar brightening is warranted. We agree that given the shortness of our satellite data record, our trend has less statistical certainty than say the trend detected over global oceans by Mishenko 2007, yet our analysis agrees with that finding and other cited (Introduction) works that show solar brightening over the U.S. for the past 20 years or so.

Minor Comments:

Referee comment #1: Page 7560, line 19: unclear what $\alpha = 0.05$ refers to here, suggest remove “for $\alpha = 0.05$ ” or provide details of t-test.

Response: We removed the text from the abstract; however, we do provide details of the t-test in the results section.

Referee comment #2: Section 2.1: do the $PM_{2.5}$ measurements include water? Please give this information in the text.

Response: None of the $PM_{2.5}$ measurements included water. Please see Section 2.1 for the added text.

Referee comment #3: Page 7565, line 24: what are “fill values”? are these interpolated or duplicated?

Response: We recognize that the term “fill-in” raises questions about our methodology. The fill values referenced in text are place holders to ensure that the $PM_{2.5,FRM}$ dataset is the same length as the $PM_{2.5,TEOM}$ dataset. The fill values (e.g. NaN) do not factor into any calculations, and no spatial/linear interpolations or duplicates were done. We removed the line from the text to prevent any further confusion.

Referee comment #4: Section 2.2: did you cloud filter the data at all? This relates to the larger issue of how cloud cover can impact your long-term trends and seasonality. It would be helpful to tell the reader how the amount of data in each season is impacted

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by cloud cover. You should also verify whether there is any trend in cloud cover that may impact the satellite data coverage and hence the AOD trend.

Response: The “Optical Depth Land and Ocean” Level 2 aerosol product is cloud screened. The aerosol algorithm is not applied to any pixel that is considered cloudy. Your point about determining a trend in cloud cover is addressed in a subsequent publication Alston, E. J. and I. N. Sokolik, “A First-Order Radiative Forcing Assessment Based on Decadal Satellite Data in the U.S. Southeast” in prep. Our analysis suggests that there is no discernable trend in cloud coverage as observed from the MODIS sensor.

Referee comment #5: Section 2.2: in Section 2.3 you give information from the literature on comparisons of MISR and AERONET, to be consistent similar information should be provided for MODIS vs. AERONET.

Response: We added the appropriate text and citation to this sub-section.

Referee comment #6: Page 7567, lines 10–18: I am a little unclear here on your matching process. Can you confirm that you in fact did not use only co-located data? This may be fine for presenting independent maps and trends, but for scatter plots you should include EXACT comparisons (Figure 4). Thus, you should grid the MODIS and MISR data to the same grid and only include grid boxes where both were observed to create averages.

Response: The scatterplots in Figure 4 are co-located for the lat/lon box described in the text. The AOD values that fall within that box are spatially averaged over the domain to create one value for each day, and it is these daily averaged values, which are used for a majority of the analysis in Figures 1–4, 6–9. The only time where the satellite AOD values are averaged on a grid are for the creation of the maps shown in Figure 5.

Referee comment #7: Figure 3: you should discuss at least briefly why 2007 was high when you first discuss this figure on page 7568 and not leave it to page 7571.

Response: The text describing the wildfire was moved earlier in the text as suggested.

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Referee comment #8: Figure 4: I don't understand what data is shown on this figure, so perhaps the caption could be expanded a bit? I understand that the data is seasonal means, but there are more than 10 points, so it is not the average value on the 5x5 grid for each year. Does each symbol represent a value over each PM_{2.5} site? Co-located grid-boxes in the region?

Response: The scatterplots contain the monthly averaged AOD for each 3 month period per season. For example, the Winter (Dec.–Feb.) would have 3 data points, one for each month. We have changed the caption text to make this clearer.

Referee comment #9: Page 7569, line 21: grammar: “AOD values near clouds can double due to . . .”

Response: We changed the text as suggested.

Referee comment #10: Figure 6: why is winter on a different color bar than summer? It makes the figure confusing to compare.

Response: We chose to use a different color scale for the winter season to highlight the small values associated with this season. If we used the same colors as we used in summer when the values are threefold higher then the contrast between the seasons is diminished.

Referee comment #11: Page 7571, line 18: grammar “product produces smoother. . .”

Response: We changed the text as suggested.

Referee comment #12: Page 7573, lines 1–5: You should be clear that the correlation trends here are driven by the seasonality in aerosols in the SE (not the long-term trend). This correspondence in seasonality was previously shown in Figures 1 and 2 so it seems it would be more informative to look at the correlation in the de-seasonalized trends.

Response: We went back and performed that analysis and added those results to the paper in that section and the Conclusions and Discussion section.

Referee comment #13: Page 7573, lines 6–7: It is incorrect to say that trends are “read-

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ily apparent" in all datasets, when there is no apparent trend in the satellite datasets.

Response: We changed the sentence to "Most datasets have a decreasing trend with time, with the PM_{2.5} datasets having a more pronounced trend."

Referee comment #14: Page 7573, line 8–11: Could you tell us more about the 2007 fire? Where was it located?

Response: We added additional information about the 2007 wildfire and included a link to more information about the wildfire.

Referee comment #15: Page 7574, line 10: typo, I believe you should refer to Figure 9 not 8 here

Response: Indeed, that was a typo. It has been corrected.

Referee comment #16: Page 7573/7574: I'm confused about how the distinction between metro Atlanta and outside this region discussed in the text matches with Fig. 9. This figure appears to show very similar trends for both in the TEOM data.

Response: We need further clarification. We make no distinction between the geographic sub-regions (e.g., Atlanta, Outside Atlanta) in the supporting text for Figure 9. The fact that the behavior for both subsets is similar implies that at least from the TEOM perspective that the entire region has similar aerosol loading. We created these subsets to better understand the metropolitan area's impact upon the region, not necessarily because there was hugely different behavior.

Referee comment #17: Page 7574, line 17: You need to be specific about what is correlated. Temporal correlation of the regional means?

Response: We added clarification to the statement "The regression variables based upon AOD/PM_{2.5} vs. time are calculated on a per year basis."

Referee comment #18: Page 7576, line 3–4: there is no negative trend in AOD, do you mean PM_{2.5}?

Response: No, we do not. The Goldstein et al. (2009) paper is really focused on satellite AOD and not directly on PM_{2.5}, which is why we focus on AOD. Though we

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acknowledge that BVOC and SOA drive PM_{2.5} concentrations that AOD captures.

We believe we have addressed your issues and concerns; however, if further clarification is needed, we will be happy to provide it.

Interactive comment on Atmos. Meas. Tech. Discuss., 4, 7559, 2011.

C3005