Comments on Alston et al. "Characterization of atmospheric aerosol in the US Southeast from ground- and space-based measurements over the past decade"

This paper continues an analysis begun in a paper by the lead author last year (Alston et al., 2011). The main points of the paper are that correlation exists between aerosol optical depth (AOD) measured from three satellites and PM2.5 measured by continuous and 24 hour average FRM monitors on the ground. The authors infer that the concentrations arising from Atlanta, Georgia, are a significant input to the State's aerosol load, affecting seasonal and annual trends. The authors also do not concur with the assessment of Goldstein et al. (2009) who contend that secondary organic aerosol (SOA) and biogenic volatile organics are a dominant factor in southeastern haze and aerosol loading.

The paper is thorough although not particularly well written. Reviews of prior literature are given but are given generally cursory treatment. Gupta and Christopher (2008) treated the same subject for much of the same period, but no discussion of the conclusions of that work are given. This topic has been widely published over the last 5-10 years and many of the papers have given little or no insight to the aerosol processes which correlate haze and mass. This paper often makes comments about the possible reasons for some observed correlation or behavior, but doesn't provide any assessment in depth to resolve the behavior. This is likely to weaken the impact of the work and place it in the larger panoply of suggestive relationships without any conclusions.

This reviewer is particularly perplexed by the observed trend in surface PM2.5 in Georgia from the surface samplers and no statistical trend in the AOD data. This must have a reason. Conjecture on the dominance of humidity in the AOD retrievals is one reasonable explanation in that the high haze events are dominated by high humidity and mask decreases in mass (or particle number), but other reasons (increases in burning and elevated smoke, for example) are possible but are not fleshed out. Could the authors not assess the number of fires over the decade from Fire Radiative Power (FRP) measurements?

Trends are often masked by the treatment of the statistics. For example, the authors know that 2007 was an anomalous year with a major fire burning for much of the summer. Why not remove that anomaly in the trend analysis? Clearly, much of the AOD in any year in the southeast is fire related and you need to keep the average behavior, but a single event (or perhaps even 2σ events) could be removed to make the trends more robust. There is a technique known as "Winsorizing" which has long been used in trend analysis to remove outliers which dominate the slope of a trend. The authors might consider that.

I have another concern about bias. The PM data at the surface is highly focused on Atlanta, with almost half of the samplers in that region. Yet, the AOD from the satellite should not have that bias. What would the authors have seen with more coastal surface monitors. Their results show elevated AOD near the coast. Is that real or an artifact of the satellite retrieval. Without concomitant PM measurements, it is impossible to determine whether there is bias here.

Overall, I think this paper should be published and the decision to include in AMT is the editor's decision. I don't find the paper particularly novel in the use of any new techniques but rather extends prior work with a significant level of analysis on a specific region.

I have some concerns about grammar and typography that the authors should attend to:

Pg 7560 line 14: there is "good agreement". R=0.64 is hardly "good agreement" since only about 40% of the variance is explained. Thus my concern with qualitative statements of "good" or "bad". There is correlation.

Pg 7561 line 7 "Samoa"

Pg 7561 line 23 "each sites trend slope" is very poor grammar. Next line "each sites respective region" as well. You need to use apostrophes to help us out figuring out what you mean.

Pg 7562 line 4 "geographic region that has been studied" grammar

Line 13: showing a decrease… of what? 19% as well? How much of a decrease?

Line 14: "distinctly characterized". Really? No other part of the US has this aerosol mixture?

Pg 7563 line 19: "Our earlier work, used probabilities of AOD" Why the comma? And what are probabilities of AOD? Probability AOD exists or doesn't exist?

Pg 7564 line 14: Sect. should be spelled out.

Line 18: ten years record>> the ten year record

Line 24: there are 18 sites, you used 12… seven are in Atlanta. What is wrong with the other six sites?

Page 7565 line 17: Atlanta's

Line 19: "We have three subsets for each PM2.5" You mean that you subdivided the data into three groupings: all of Georgia, Atlanta and the remainder.

Line 23: Given the repeat … not taken. You need to describe this process in more detail. How did you fill in data on a 6-day repeat cycle? Did you linearly interpolate between times? How do you justify this in terms of aerosol spatial and temporal scales in Georgia?

Pg 7566 line 7: "5-15 min after" is not correct for Terra.

Line 14: Nominal resolution? The resolution of MOD04 is 10x10km. The resolution of Terra is as small as 250 m. You need to be careful about which product you are describing. But you don't use data at that resolution. On Page 7567 you say you aggregate to 0.25º (about 30 kilometers) for MODIS and less for MISR. Your box is not Georgia. It is 30-35N x 80-85W which contains half of South Carolina and much of Alabama. Yet you only use Georgia ground monitors. This significant mismatch draws the work into question.

Pg 7568 , line 5-6: MISR has higher AOD than AQUA MODIS. Not in Figure 2A.

Line 12: an increase of 10 to 18 ug m^-3 is "modest". Seems like a factor of two in my calculations (approximately). Line 13-14 …. TEOMs were operated only for 7 years. Did this enter into the removal of 8 monitors (see above).

Line 15: differences in measurement techniques…. Do you have any evidence that the TEOM data is biased relative to the FRM method? If so, why has EPA not removed this data stream. If you are going to make a conjecture like that, you need to have evidence to defend it. What is the PM2.5 correlation between TEOM and FRM from nearby stations?

Line 24: 2007 had anomalously high AOD. You know why. State it here instead of spinning this out as a mystery.

Pg 7569 line 19: "Remer et al. (2008)..1% of the time over land". What Remer actually said is that 80% cloud cover in a pixel with an aerosol retrieval only happened 1% of the time. This is different than what you suggest. Reread.

Pg 7570 line 5 "we do not force our linear regressions through zero"…. This is an obvious reason why you do not have a zero intercept but means little.

Line 6: Kahn's 2010 paper suggests no such underestimation of AOD by MISR. Reread.

Line 12-13: This argument of a difference between sources within the Eastern US and your study needs further argumentation. Is the speciation different in your region? Are there aerosol types not seen in the mid-Atlantic or Northeast? I find this to be a specious argument.

Line 13-14: this is not a sentence.

Pg 7571 line 4: Move this argument up to line 24 pg 7568.

Pg 7571 line 18: Your argumentation that Goldstein's data is biased by using Giovanni or another Level 3 product falls short since you aggregate your own data to a 0.25x0.25 degree grid. It is more likely that the monthly aggregation includes bad data. Giovanni was working on tools to remove outliers in that product.

Pg 7573, line 9: "does not have this peak". It appears to in Figure 7B. What are you referring to?

Pg 7573 line 12: determination of trend in highly seasonal data requires care. You have implemented a "Pearson" type solution which aggregates months together and looks at the anomaly from the mean. Not a bad first approach. You might want to consider more sophisticated treatment (See Sirois and Barrie Arctic lower tropospheric aerosol trends and composition at Alert, Canada: 1980–1995; JOURNAL OF GEOPHYSICAL RESEARCH, VOL. 104, NO. D9, PP. 11,599-11,618, 1999 doi:10.1029/1999JD900077 1999) for pulling trends out of highly seasonal data.

Pg 7574 line 7-10: determining a trend in AOD has been elusive. But you might consider scaling the results to the mean of the anomaly, i.e express the trend in % per year rather than absolute AOD units. It would be more instructive as to whether it is in any way proportional to the percentage decrease in PM.

Conclusion: There is discussion included in the Conclusion that should be moved up to the body of the paper. Line 2-6 on page 7575 discusses humidity and since this is a major confounding factor between PM and AOD, it should be moved up and discussed in greater detail in the body of the paper.

Pg 7583 Figure 1: Figure is unnecessarily complicated. Plot on a map, not google earth. I cannot see the labels and they will not reproduce well. Why include inactive monitors?

Pg 7585: text says that the Terra AOD has higher variability than MISR. This is not apparent from this figure.

Pg 7590: Figure 6: No labels on plots to identify A-F. Why change colorbar from 0.4 to 0.3 for difference? The Summer Mean MODIS AOD shows a hot spot right on the Summer 2007 fires. This seems to dominate the entire summer 10 year average. MISR misses those fires entirely.