We thank the anonymous reviewer for their helpful comments – we have made changes to the manuscript in response to their suggestions outlined in red below.

This paper presents an observation-model integrated analysis of aerosol sources and seasonal variations in Southeast U.S. The high-resolution GEOS-Chem modeling is used as a platform to interpret a variety of aerosol observations from ground, aircraft, and satellite during the SEAC4RS campaign. Short-term trend of aerosol in the last decade is also discussed to some extent. The results from this study are a useful contribution to an improved understanding of aerosol sources and variability/trend in Southeast U.S. The paper is generally well written. I recommend the paper be published after authors adequately address following concerns.

My major concern is about their 40% downward correction of mixing layer (ML) height. First, what is definition of the ML? I thought it is defined as daytime maximum PBL height. However later they try to distinguish ML from PBL (p. 17668, line 26). Or do they define the mixing height just like that based on lidar profiling of aerosol? In any case they need define the ML in the first place and use it consistently throughout the paper.

Additional clarification has been added to the text. GEOS-Chem does not directly derive ML height, it is provided from the parent meteorological fields (GEOS-FP) generated by the NASA Global Modeling and Assimilation Office. The ML height from GEOS-FP is derived from the potential temperature gradient. The ML is not defined as the daytime maximum PBL height – here we define the PBL as the ML + CCL.

Second, it is argued that several studies (Scarino et al., 2015 – which is however not listed in the reference; Millet et al., 2015) have found GEOS-Chem simulated ML is too deep (e.g., 30-50% positive bias) across Southeast U.S. So they reduced the ML height by 40%. However it is not clear to me how they implemented this in the model. Did they adjust some tuning parameters to get the computed ML matching the observation? If they did this way, then they should document those tuning parameters so that other studies may take advantage of the outcome from this study. If not, what did they do exactly? Nevertheless it is important to document how they corrected the ML in the model.

Scarino et al. 2014ab have been added to the reference list. Additional information on how the ML was adjusted has been added to the text (a simple scaling of the variable read in from the offline GEOS-FP meteorological files during daytime hours).

Third, did they compare the GEOS-Chem ML height with some observations? For example, Seidel et al. (JGR, 117, D17106, doi:10.1029/2012JD018143, 2012) derived a climatology of PBL height over U.S. and Europe by using radiosonde observations. Is this PBL climatology useful for their study?

Evaluation of GEOS-FP ML heights against ceilometer measurements during SOAS is presented in Millet et al. (2015). Zhu et al. (in prep.) will present a more thorough comparison to lidar and climatological observations.

Fourth, the 40% downward correction was applied to the GEOS-Chem ML height throughout a year (Figure 13b). Here an inherent assumption is that the modeled ML has

a positive bias of ~40% regardless of season. Is this assumption justified? How does this assumption affect their interpretation of the difference in the amplitude of the seasonal cycle between PM2.5 and AOD?

This is an excellent question. We make the assumption that the GEOS-FP ML bias applies year round because we lack highly resolved data such as from the lidar during SEAC^{4}RS. However, we do not expect this to change the interpretation of the difference in the seasonal cycle amplitude observed between $PM_{2.5}$ and AOD. As an endpoint scenario where there is a 40% reduction in ML height during summer and no reduction in ML height during winter, there is still a 40-50% enhancement (based on Figure 13b) in the ML height during the summer that leads to a difference in magnitude in the seasonal cycles.

Minor changes:

p.17653, l.10-11: "GEOS-Chem simulation of sulfate requires a missing oxidant, ..." I don't quite understand this sentence.

We deleted that sentence from the abstract.

p. 17655, l.1: Figure 1 shows both summertime and wintertime aerosol trend. But the winter trend is not discussed in main text at all.

We don't have much to say about the winter trend because it isn't the focus of our simulation.

p.17657, l.26: what does "FP" in GEOS-FP stand for?

The FP in GEOS-FP stands for "forward-processing." Added to the text.

p.17662, l.1-5: The description of AOD calculation is not right.

We don't understand the reviewer's concern. The description is correct. This is how AOD is operationally calculated in GEOS-Chem. Please see: http://www.atmos.colostate.edu/~heald/docs/GEOS_Chem_optics_description.pdf for more details.

p.17671, l.20: Why use Aqua/MODIS, but not Terra/MODIS?

In our previous work, we found little difference between MODIS AOD on Aqua and Terra and so we chose to focus on the afternoon MODIS data for this analysis.