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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Sources, seasonality, and trends of Southeast US aerosol: an integrated analysis of surface, aircraft, and satellite observations with the GEOS-Chem chemical transport model" *by* P. S. Kim et al.

Anonymous Referee #3

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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. 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. 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? 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 $\sim$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?

Minor changes:

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

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.

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

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

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

Interactive comment on Atmos. Chem. Phys. Discuss., 15, 17651, 2015.

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