

## ***Interactive comment on*** **“Temperature-dependence of aerosol optical depth over the southeastern US” by Tero Mielonen et al.**

### **Anonymous Referee #1**

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The manuscript presents an investigation of the effects leading to the enhanced summertime AOD detected over the southeastern USA by satellite instruments. The authors show that the temperature dependence of AOD is an indication of biogenic aerosol formation by comparing observed trends (of AOD vs. land surface temperature) with modeled data in which biogenic aerosol formation is switched on or off. I have several major issues with this manuscript, as I will motivate below. My recommendation to the editor is to reject the manuscript in its current form.

My main objection to the study is that it does not appear to provide any new insights: a very similar (though more thorough) study has been presented in the paper by Goldstein and co-workers (2009), cited in the manuscript. Second, the statistical analysis

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does not appear to be very robust. The numbers in the manuscript are somewhat convincing, but the figures show very shaky correlations: the number of points is very small in Figure 1, and although the correlation for the red and blue data sets in the matching Figure S1 in the supplement looks alright, it is unclear how the data are separated, as now some red dots have AOD anomalies  $<0$ , whereas two blue dots have an anomaly  $>0$ . Third, the results from the model study appear to be rather trivial: it is obvious that AOD due to biogenic aerosols depends on temperature, as this is explicitly parameterized in the model. Fourth, the values of the radiative effects of biogenic aerosols determined from satellite data are about twice as small as their respective errors. Last: please remember that correlation does not prove causality.

Other comments (page 1, line 1 denoted as P111) :

a. In addition to NO<sub>2</sub> columns as a proxy for anthropogenic pollution, it might be of interest to study formaldehyde and glyoxal columns to obtain more information on VOC emissions. See papers by I. De Smedt or M. Vrekoussis, but also Veeffkind et al. (2012, cited in the manuscript), or Penning de Vries et al., *Atmos. Chem. Phys.*, 15, 10597-10618, doi:10.5194/acp-15-10597-2015, 2015

b. Why did your model control run not provide hourly output? Instead of performing runs with and without certain aerosol sources, is it not possible to read out the AOD due to each aerosol type separately?

P217: “natural unperturbed aerosol” – this is not what you are studying in the south-eastern US. As you mention later on, Goldstein et al. (2009) suggested that SOA are more readily formed in the presence of pollution.

P713: “the anomalies of the regional mean” – What is the reference? Or, to be more precise: how did you calculate anomalies?

P714-15: “NO<sub>2</sub> column densities available from OMI (only available from 2005 onwards) were used” – Why did you not use SCIAMACHY data? SCIAMACHY was on

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the same platform and hence has the big advantage that it not only measured the same time period (2002-2012), but it also measured at the same time of day as AATSR. This is important for species that, like NO<sub>2</sub>, exhibit a diurnal cycle.

P915-6: “summertime FRP anomalies did not show any correlation with corresponding AOD or LST anomalies” – This would have been very surprising, as the region does not exhibit much fire activity. I’m pretty sure that even transported smoke (from Canada or Alaska) does not play a role, but since a summertime maximum of fires exists there, it would be worthwhile to at least mention transported smoke as a possible source of AOD over the SE USA.

P10122: “aqueous phase SOA” – What are these?

P1113-6: “there is a delay (. . .) takes at least several hours” – I do not believe this is quite correct. It takes a while for SOA to form, but it also takes a while for them to be removed, so I expect only a very shallow diurnal cycle (if any at all). In fact, your green line (noBIOSOA 2008) shows a minimum at the same position.

P11124: “All the values for the equation, except for S<sub>rad</sub> and phi, were taken from Goldstein et al., (2009)” – Why did you change the cited equation? Goldstein (2009) and Haywood and Shine (1995) both included the local daylength (1/2) multiplied by the solar constant S<sub>0</sub>, which you substituted by S<sub>rad</sub> multiplied by phi. The values are almost the same, but it is unclear where your value for S<sub>rad</sub> comes from.

P12129: “overestimating”- underestimating (I’m guessing you mean the model)

P13112: “Anthropogenic emissions are the main driver of AOD levels in this region” – This is only true if you mean that anthropogenic emissions enhance SOA formation. Which, to my knowledge, is still a hypothesis.

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Interactive comment on Atmos. Chem. Phys. Discuss., doi:10.5194/acp-2016-625, 2016.

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