Atmospheric
Chemistry
and Physics
Discussions

Open Access

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

Anonymous Referee #3

Received and published: 6 September 2016

The manuscript by Mielonen et al. attempts to answer the question of whether the observed temperature dependence of aerosol optical depth over the southeastern US is driven by BVOC emissions versus aqueous phase production above the boundary layer. They attempt this partitioning via the combined use of remote sensing observations and a coupled climate-chemistry model. Such use of a model of this type is warranted for the problem due to the complex interacting sources of aerosol which makes causality difficult to infer through purely empirical means. I find the argument compelling, but also that the manuscript could be strengthen by greater clarity in explaining its approach and why the evidence presented adds up to a coherent storyline.

Much of the empirical argument rests on the idea that different sulfate dynamics in

different time periods lead to different behavior in the aerosol load. One issue here is that those time periods are not treated uniformly; in figure 1 these are before versus after 2008, whereas in figure 4, the late period also include 2002 and 2003. It isn't totally clear why this switch was made. I realize this is discussed on page 10 first paragraph, but I don't follow the logic for why it was handled this way.

In assessing temperature sensitivities, the manuscript needs to be specific about what timescale it is working on. Figures 1-4 are seasonal means, figure 5 is monthly mean, and figure 6 is diurnal-scale. What is the justification for jumping around like this, and what processes will be dominant at the different timescales?

For the modeling part of the argument, the crucial point is whether the no aqueous phase experiment accurately captures the essence of the hypothesis put forward by Ford and Heald. It is not clear to me that it does, so I think a stronger explanation and justification needs to be made of what mechanisms this experiment tests, and why this experiment is accurately representing that hypothesis.

page 9, line 14: do you mean non-anthropogenic AOD anomalies?

page 9, lines 29 and 30: these p values are completely meaningless. the relevant test of a model at this level of complexity should not be that it performs better than a completely uninformed null model, but rather that it performs better than some informed but much simpler model. delete this or else come up with a much better null model that you argue the full model has better skill than. Also the sentence at the end of this paragraph doe not logically follow.

references: note that Ford and Heald 2013 is discussed in the manuscript but does not show up in the bibliography.