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Comment on acp-2022-648

Anonymous Referee #2

Referee comment on "Predicted and Observed Changes in Summertime Biogenic and Total Organic Aerosol in the Southeast United States from 2001 to 2010" by Brian T. Dinkelacker et al., Atmos. Chem. Phys. Discuss.,
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Here the authors explore organic aerosol over the southeast United States, an area well known for especially high biogenic emissions. Using the chemical transport model PMCAMx, the authors simulate two summers, one in 2001 and the other in 2010, and compare conditions and results between them. The effects of various aspects of the overall SOA mechanism are discussed, and in some cases quantified. Manuscript text is clear and well composed, and figures are generally effective. However, while the topic itself is worthwhile and deserving of study, in my opinion this work is fundamentally incomplete, lacking a central question and failing to produce any novel conclusions. I do believe there are opportunities available to further develop this manuscript with additional modeling work and analyses, but in its current form I do not support its publication in ACP. Some specific big picture concerns follow.

- **Years modeled:** By choosing to examine only two total years, the authors limit their ability to draw meaningful conclusions regarding trends and variability. While it is true that changing anthropogenic emission inventories in particular will show a strong signal when comparing between these years, interannual variability in underlying dynamics must be assumed to impact and confound those anthropogenic changes as well as those of meteorology-dependent biogenic emissions. To better understand trends in emissions and resulting OA concentrations, a more robust temporal domain (for example including the years between 2001 and 2010 as well) is recommended to help resolve some of these influences.
- **Role and accuracy of meteorological variability:** Meteorology and dynamics are inadequately addressed here in general, with mostly qualitative descriptions covering this extremely influential driver of differences in modeled output. Significantly more work here is necessary to better understand exactly how meteorology is affecting both biogenic emissions and precursor transport. Along these same lines, while significant attention is paid to observational comparisons of modeled aerosol, none is given to the WRF output driving emissions and transport. Considering their significance, and the novelty of the dynamics generated here to drive the CTM, this is a glaring omission and worthy of considerable evaluation.

- **Significance and novelty of conclusions:** Perhaps most importantly, on the whole I really struggle to find a key takeaway message contained here. The model output is well presented, with clear maps and figures comparing the two examined years, but I see nothing surprising or helpful in terms of advancing the state of knowledge on the region or on the modeling of OA in general. The NO_x-dependence of SOA yield is removed as a case study, but the reason and value for this is completely unclear to me. It seems trivially obvious to me that keeping only the low-NO_x oxidation pathway would increase yields and overall concentrations, and I see no need to confirm this expected result. A major rethinking of what questions these simulations are intended to answer is necessary if this work is to make a meaningful contribution to the SOA modeling literature.

Smaller issues and questions:

- Lines 64-65: "Plenty of uncertainty still exists regarding the role of isoprene in SOA formation." This is a confusingly broad and poorly explained statement.
- Lines 135-145: The relevant bins and species included in the model are difficult to parse. For example, I couldn't tell for sure whether there was one species for monoterpenes and one for sesquiterpenes, or just a single species representing both together. A schematic or diagram would be very helpful here.
- Two days seems to me to be an unacceptably short spinup time, considering the lifetimes of relevant trace gases and precursors.
- The authors note that IMPROVE measurement comparisons were heavily influenced by fires on several days in the modeled domain. Does this mean that biomass burning in general is not included? If they are included, but simply lacked information on those specific fires, more background information on the inventory used is appropriate. If they are not included, this strikes me as a significant problem that should be addressed.