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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Anthropogenic influence on SOA and the resulting radiative forcing" *by* C. R. Hoyle et al.

Anonymous Referee #2

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This study investigates the change in SOA burden predicted by the global Oslo CTM2 from pre-industrial to present-day and attempts to assess the resulting effect on radiative balance. While the authors explore some interesting sensitivities of SOA formation in their model, I have some serious concerns about some definitions and conclusions in this study. In addition, in light of many recent studies revealing the failures of models to capture the formation and burden of SOA in various environments, it seems premature to attempt to assess radiative forcing of SOA.

Specific Comments:

1. The authors appear to have confounded the definition of "direct radiative forcing" and "direct radiative effect". I direct them to the IPCC definition on pg. 157, which

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Interactive Discussion
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states "The solar direct radiative effect (DRE) is the sum of the direct effects due to anthropogenic and natural aerosol species while the direct RF only considers the anthropogenic components." The IPCC assesses a clear-sky DRE of -5.4 W/m2. The origins of SOA are complex, and definitions of biogenic and anthropogenic may be inadequate to describe the complexity of SOA formation in the atmosphere, nonetheless, SOA formed from precursors emitted by vegetation is to first-order "natural" and thus constitutes a direct radiative effect, not a forcing. The authors therefore mis-characterize their conclusions, from their title, throughout their analysis. Only the change from PI to PD in the fraction of total SOA from aromatic precursors included in the model should be characterized as radiative forcing, and though the authors don't give this split, I suspect the true radiative forcing would thus be nominal in their model.

2. Studies differ as to whether biomass burning should be characterized as "anthropogenic". As the effects of changing biomass burning emissions are included here by the authors as anthropogenic, this definition should be stated up-front in the text and the abstract.

3. The authors often refer to the "pre-industrial atmosphere", however as only the emissions, and not the climate reflect PI times, this is inaccurate. This should be corrected in the text, and the scope of the study should be clarified in the abstract.

4. While an abbreviated model description is acceptable for this follow-on study, the authors should include a few more of the key details, such as whether the SOA is treated reversibly and what enthalpy of vaporization was used.

5. The discussion and details given regarding the optical properties is extremely insufficient. Assumed refractive indices and sizes should be given in the text. There is also a clear inconsistency in treating the aerosol as externally mixed for the calculation of radiative properties, when SOA is assumed to condense on sulfate. I recommend that the authors explore the difference in forcing when assuming externally vs. internally mixed. A more thorough discussion of how the range of organics that may constitute

ACPD

8, S8995–S8997, 2008

Interactive Comment

SOA and the degree of oxidation of these compounds may affect optical properties is required.

6. Page 18915, line5: There is really not enough evidence from either the lab or ambient observations to conclude that SOA condenses on sulfate aerosol. Therefore while adding this process could potentially correct a low bias in the model simulation of SOA, it may be compensating for some other missing process, and thus caution is warranted.

7. Pg. 18921, lines 20-23: Please discuss why such a significantly larger increase in POA emissions in Liao and Seinfeld (2005) would result in a more modest increase in SOA than simulated in this study.

Technical Corrections:

1. Page 18914, Line 26: The references for the partitioning coefficients are missing.

2. Table 2: Run labeling differs from the "name" given in Table 1. Please reconcile.

3. Tables 3&4: The authors should show the separate numbers for biogenic and anthropogenic SOA (particularly in light of the DRE/DRF discussion above in point #1).

Interactive comment on Atmos. Chem. Phys. Discuss., 8, 18911, 2008.