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Atmospheric Chemistry and Physics Discussions

Interactive comment on "The direct and indirect radiative effects of biogenic secondary organic aerosol" *by* C. E. Scott et al.

Anonymous Referee #1

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This manuscript presents a very well-performed analysis on the radiative effects of aerosols resulting from biogenic organic precursor emissions and associated uncertainties. I recommend accepting this paper for publication in Atmospheric Chemistry and Physics after the authors have addressed the following, relatively minor, issues.

Section 2.1.1, first paragraph. Is there a specific reason for selection the yields 13% and 3% as base case values?

Section 2.1.2. The authors could add which year the primary emissions are supposed to represent.

Section 2.1.3, first paragraph. Note that also BHN rate is calculated from a parameterization. The main differences between the four parameterizations are the assumed

nucleating species and data source (theory, lab experiments, field measurements) from which the parameterization has been derived. A slight modification could be made to avoid confusion by the reader.

Page 16974, lines 4-6. This statement is unclear. Practically all the regions are coincident with some sort of primary emissions, so it is rather question of the magnitude of primary emissions. Please modify the sentence a bit.

Page 16977, lines 3-5. The statement "When monoterpene oxidation products are allowed to participate directly in nucleation, the contribution of biogenic SOA to CCN concentrations is substantially greater. . ." needs to be clarified a bit further. The survival of particles nucleated at diameter d* up to the diameter 3 nm is an exponential function (equation 4), and therefore the survival probability is sensitive to the particle growth rate, condensation sink and, importantly, to d* as well. The authors should investigate whether larger enhancements in CCN concentrations in case of organic nucleation are due to higher nucleation rate in those cases or, rather, due to larger assumed values of d*. In practice, assuming larger value of d* for an organic nucleation mechanism is equal to assuming that organics participate in both nucleation and initial steps of nuclei growth, so one might also consider stating "When monoterpene oxidation products are allowed to participate in the very early steps of new particle formation, the contribution. . .".

Page 16982, lines 23-29. Besides the two papers mentioned here, also comparison to the few other measurement-based estimates of the AIE could be added here (see Lihavainen et al. 2009 in the reference list, and references therein). Those measurement-based studies could also be added to introduction (page 16964, lines 25-27). The paper by Lihavainen et al. predicts much lower direct radiative effect compared with AIE over boreal forests, which seems to be in line with the results in figure 7. The authors should discuss this issue as well in this paragraph.

Page 16983, line 1. Are the authors referring to oceanic regions surrounding boreal

forest? Please be more specific here.

Section 6. It would be interesting to see a brief discussion on how the relative importance of DRE and AIE due to biogenic SOA varies spatially. Based on Figure 7, there appears to be large differences between different world regions, yet purely visual inspection of the figure provides only qualitative information on this issue.

Section 8, second paragraph. Referring to my previous comment, the authors should check out whether larger enhancements in CCN in case of organic nucleation are really due to larger nucleation rates, or whether larger assumed values of d* are the primary reason.

Interactive comment on Atmos. Chem. Phys. Discuss., 13, 16961, 2013.