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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Future vegetation–climate interactions in Eastern Siberia: an assessment of the competing effects of CO_2 and secondary organic aerosols" *by* A. Arneth et al.

Anonymous Referee #2

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This modelling study investigates the response and feedback of biogenic volatile organic compounds (BVOC) and secondary organic aerosol (SOA) to a changing climate in Eastern Siberia. The results show a notable negative forcing in Siberia and the northern hemisphere from changed aerosol direct and indirect effects and reveal a series of open questions. The vegetation-climate interactions are very complicated and of fundamental importance in the atmospheric and climate research. This study is a nice trial and I would recommend publication if my following comments/suggestions can be adequately addressed.

Major comments:

I would suggest the authors to improve the readability of the manuscript through more diagrams and tables: - A schematic diagram summarizing the investigated interaction mechanisms; - A table with details of model configuration and highlighted new/important parameterization/treatment; - A table summarizing uncertain processes or those not sufficiently considered.

Specific comments:

Page 27142 line 24, "Aerosol particles were continuously monitored with a Scanning mobility particle sizer (SMPS) located at the foot of the eddy covariance tower ... ", how about the inlet system for aerosol measurements, were the aerosols sampled also from 30 m above the ground? If not, a difference between above and below canopy will be expected challenging the results of Fig. 2. To test the difference, I would suggest the authors to make similar comparison (as Fig. 2) but for a single species below and above the canopy, e.g., monoterpene (or other VOCs, trace gases).

Page 27142 line 24, please define the acronym "DGVM"

Page 27144 line 4, "A recent data-base estimate was 191, 495, and 1024 GtC in the 0–30, 0–100 and 0–300cm soil layer, of permafrost-affected soils, respectively". Are these estimates from observations or from models? What is the geographical coverage of these values (Eastern Siberia or above 40 degree N)?

Page 27145 line 1, " Multiple interacting processes can thus lead to enhanced global monoterpene emissions in future, or -if the “CO2 inhibition” is included- yield emissions that are more or less similar to present-day or even slightly smaller ". Can you elaborate how the CO2 inhibition was parameterized in your model?

Page 27148 line 11, "The assumption of unchanging oxidant fields induces some uncertainty . . . The model climate is nudged towards ERA-40 reanalysis year 2000 meteorology, an approach that is widely used in aerosol-climate assessments". Did you use

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the same oxidative capacity field and nudging meteorology field for the year 2000 and 2100? Such treatment is OK for a sensitivity study but I am not sure if it is appropriate when you draw conclusions for a specific year 2100. Changes of land uses and VOCs will change the distribution of oxidants and meteorology.

Page 27148 line 27, the authors used 1% supersaturation CCN. The atmospheric relevance of supersaturation depends on the aerosol concentration and updraft velocity. It will be better if the authors could give CCN at multiple supersaturations (e.g., in the supplement).

Page 27155 line 15, " What is more, SOA formation only partly enhances the survival of small particles by providing additional growth (Makkonen et al., 2012a), but partly also suppresses it by increasing the coagulation sink for small particles (Fig. A2, lower left panel; see also O'Donnell et al.,2011)". CCN is defined for a specific supersaturation. Larger particles are better CCN (activated at lower supersaturation) than smaller ones. Though the coagulation removes smaller particles reducing the CCN number concentrations at high supersaturation, it increases the particle size leading to more good CCNs (activated at lower supersaturation). This is another mechanism affecting the indirect radiative effect of aerosols.

Page 27150 Section 3.2, It is not clear for me how this section is linked to the modeling part of this study. Do you use it in the model parameterizations?

Page 27151 line 14, "Hence, the poor relation between the source rate of condensing vapour and levels of radiation (Fig. 2b) indicates that OH-radical concentration did not have a major impact on Q. This agrees with the findings by Ehn et al. (2014) that ozone instead of OH. is an important, if not the main, atmospheric agent oxidising organic vapours into a chemical form that condenses on particle surfaces." I am not convinced by this argument because OH has a short life time and may have a large difference below and above the canopy, how about the correlation between Q and O3? What's the time resolution of data in Fig. 2b and 2d, there seem to be much less data

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than those in Fig. 2c.

Page 27151 line 27, "An overall C loss of 100 PgC assumed to be in the form of CO2 ", what's the reference year?

Table 1: what's the unit of "NPP_global"? To improve the readability, "BVOC in Tg C a-1", should be "BVOC emissions" also correct that in the table. For the unit "ug_C g-1 h-1", what's the meaning of subscript "C" (instead of C in Pg C)? Overall, it is difficult to connect the table with its caption.

Table 2: The paper is an assessment of competing effects of CO2 and SOA. The negative forcing of SOA effect has been evaluated and given. For comparison, what's the forcing due to a change of CO2?

Figure 1: For "maximum summer leaf area index", do you refer to seasonal, monthly or daily data? "a,b" are used in the figure caption while "A,B" are used in figure labeling.

Figure 3: "Areas with statistical significant changes in CCN are indicated.", can you elaborate on the statistical analysis?

Figure 4: Can you also show the absolute increase (in the supplement) for reference?

Figure A1: CO2 on/off labels seem missing in top panels.

Interactive comment on Atmos. Chem. Phys. Discuss., 15, 27137, 2015.