

Interactive comment on “Future vegetation–climate interactions in Eastern Siberia: an assessment of the competing effects of CO₂ and secondary organic aerosols” by A. Arneth et al.

Anonymous Referee #1

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In this study, the authors assessed the competing effects of CO₂ and SOA in the ecosystem-climate interactions in Eastern Siberia from global warming. By using field observations, dynamical vegetation model, global aerosol-climate model, the authors examined how global warming may lead to increase in CO₂ concentrations and increase in BVOC emissions because of the ecosystem-climate feedbacks and further estimate radiative forcing from changes in CO₂ and SOA. The paper provided an interesting example how to quantify several climatically relevant ecosystem processes under the same framework. I would recommend its publication after my following con-

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cerns are addressed:

Major comments: 1. The paper is generally very difficult to read, and I would suggest the authors to make substantial editing to the manuscript. As CO₂ increases and SOA increases are quite different subjects, it is understandable that the paper covers a lot of different aspects. But this also requires the authors to pay attention to the organization of the paper (see some of the specific comments).

2. The modeling part and the observation part should be better integrated. Although the authors argued that the paper provided an example to study different process in a consistent observational and modeling framework, I feel the paper needs further work to better integrate observations and modeling results (see also some of the specific comments).

Specific comments: Section 2: The descriptions and discussions in this section are generally too long. I would suggest to move some of these discussions into Section 3 & 4 and combine them with discussions in these two sections.

Line 158: the model is set up to test CO₂-inhibition hypothesis. It is not clear to me how this is achieved.

Line 247: where does 1.9 come from? Also, from Section 3.1, from line 274 to line 283, the range of E from 1.9 to 9.6 is also discussed. I feel it is better to move some of the discussion in Section 3.1 to Section 2.

Line 252, CCN at 1%. 1% seems too large.

Line 271: For the “larch” plant functional type, an emission potential of E=2.4. But it in 247, E=1.9?

Table 1: description texts: the range of E from 1.9 to 9.6, and then a weighted one 6.4. Which one is used?

Section 3: Observations and modeling results are not well integrated. For example,

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why not compare simulated aerosol concentrations with observed present aerosol concentrations? Model results are needed to be documented in more details. For example, I would like to see how present day BVOC and aerosols are simulated compared with observations.

Section 3.3: Is CO-inhibition implemented in the sensitivity tests? This is not clear to me from reading Section 3.3. But line 158 seems to suggest this was tested in the paper.

Section 4: How is CO₂ forcing calculated?

Line 399: the sensitivity of CCN to E. This is not clear to me.

Lines 424-434: This paragraph needs further editing. For example, it is not clear why the authors want to discuss changes in wildfire emissions and anthropogenic SO₂ emissions. Later, it seems that the effects of BVOC are only assessed by assuming wildfire and anthropogenic CO₂ unchanged. Many more paragraphs have similar issues. This is why the paper is really hard to read.

Technical corrections: Line 29: miss “.” Between “effect” and “Combining”.

Line 32-35: The sentence of “On the global level, . . .” needs some further work.

Line 97: Make sure the citation format is correct.

Line 102: “BVOCs e.g.” → “BVOCs (e.g.)” ?

Lines 120-123: What are the units of Q, CGR, and CS?

Line 213: “components black carbon” → “components of black carbon”

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