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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Future biogeochemical forcing in Eastern Siberia: cooling or warming?" *by* A. Arneth et al.

Anonymous Referee #1

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This provocative and insightful paper investigates possible atmospheric feedbacks from changes to boreal ecosystems in a warmer future world. The study builds on the boreal-aerosol-climate feedback ideas of Kulmala et al. and Spracklen et al. and is the first to examine the impacts co-changes in boreal monoterpene emissions (SOA precursors) alongside the CO2 releases from the boreal permafrost using a state-of-the-art process-based land model (LPJ-GUESS) and global aerosol microphysics model (ECHAM5.5-HAM2).

The main strengths of the paper are the presentation of the new tower measurements from the Siberian site, and the interesting analyses of the effects of BVOC emissions changes on local particle populations in a changing background-aerosol environment (Section 3.3). In addition, the study finds that when altered vegetation dynamics and

composition are accounted for in LPJ-GUESS, there is a net uptake of carbon (i.e. the permafrost release of CO2 is more than offset by increases in land storage and productivity), which is a novel and important result.

The major problem with the study, which persists throughout the manuscript, is a lack of understanding of basic climate dynamics, for example the differences between global climate change and regional radiation budgets. A change in the radiation balance (i.e. a forcing) is directly linked to the surface temperature change on the global annual average ONLY. Local and regional changes in the radiation balance are NOT correlated with the local surface temperature response. So, for example, it is scientifically inaccurate to use the words 'cooling' and 'warming' in reference to a regional change in the atmospheric radiation budget. Regional forcing does not imply regional climate response (e.g. see review by Fiore et al., 2012). In the case of the spatially concentrated aerosols, the regional climate response does not closely follow the spatial pattern of the radiative forcing (e.g. Taylor and Penner, Nature, 1994; Shindell et al., 2007+ many others). It is not possible to know what the regional surface temperature response would be to regional changes in the longwave and shortwave radiative budgets without running a fully coupled dynamic atmosphere-ocean global climate model.

If the paper is about changes to the Siberian boreal regional radiation budget, then "warming" and "cooling" need to be removed from the manuscript. Alternatively, if the authors want to talk about sign of temperature change and "warming" and "cooling", then they must instead compute the global radiative feedback effect for each component. However, in the global case, the boreal SOA effects are totally irrelevant. Moreover, based on the actual radiative feedback estimates provided in the study (CO2 = -0.09 Wm-2; local aerosol cloud albedo effect = -0.04 Wm-2) , these effects are miniscule compared to natural variability in local radiation budgets of boreal forests i.e. the aerosol-cloud-albedo results presented in the study are also negligible in terms of local energy balance for the forest. It is telling that these estimates have not been provided in the abstract.

There is a major disconnect between the use of sophisticated land ecosystem and aerosol microphysics models, and then using an inappropriately semi-quantitative back-of-the-envelope calculation for the aerosol indirect effect. Why isn't the global climate model employed to calculate the aerosol-cloud radiative changes? The back of the envelope AIE forcing is littered with unqualified assumptions including "calculated CCN values correspond to changes in cloud droplet number" and "Ac = 0.65". The albedo forcing is linearly dependent on Ac, so choice of Ac has a large impact on the results. Where does the 0.65 value come from and does it have anything to do with reality in the Siberian boreal? There are other methodological concerns described in the section below.

To move forward, I recommend to re-write the paper in the framework of: 'the atmospheric aerosol impacts of future boreal ecosystem emissions'. A single paragraph could be included in the discussion at the end of the paper about possible climatic feedbacks from these atmospheric changes.

Other major comments:

1. Page 19151. "We provide a comparative assessment of how the magnitude of the BVOC-SOA cooling compares with forcing from changes in the CO2 balance of Eastern Siberia."

Why? What is the rationale for comparing CO2 and local aerosol radiative effects? The CO2 forcing is global and the local temperature and weather will respond to this global forcing through dynamic atmosphere and ocean feedbacks in the system.

2. There is no mention of the local climate impacts of the albedo decrease associated with the regional enhancements woody vegetation and primary productivity. In addition, how do the associated biophysical land-surface changes impact the aerosol production and lifetime?

3. The effects of future changes to atmospheric water vapor and humidity are not

mentioned anywhere in the manuscript. Do they influence aerosols and Ac?

4. The link between LPJ-GUESS model output and ECHAM5 simulations is not clear. The models are not dynamically coupled. Are the BVOC emissions computed in LPJ-GUESS and then input off-line into ECHAM5.5-HAM2?

5. It appears that the aerosol microphysics model does not account for the effects of future changes to gas-phase chemistry and the oxidative environment (e.g. background and local ozone levels), which will have a huge impact on SOA loading. A 15% yield is assumed regardless of oxidative environment. The authors need to comment on the influence of changing oxidation chemistry.

6. The future BVOC emissions will have an influence on ozone and methane, which contribute important large-scale radiative changes that are not mentioned in the paper. For example, ozone is an important forcing in the Arctic region (e.g. Shindell, Local and remote contributions to Arctic warming, GRL, 2007).

7. Natural and anthropogenic NOx emission changes and effects are not discussed. Even in "clean environments" NOx is present from soils and fires. NOx changes could have large impacts on the results. For example, even in "clean environments", where the ozone production is NOx-limited, an increase in VOCs still results in an increase in the total amount of ozone produced.

8. Page 19150. "and hence have a net cooling effect at clean-air locations (Arneth et al., 2010; Makkonen et al., 2012b; Paasonen et al., 2013)."

Do you mean net negative effect on the regional radiation budget? Have the effects on other important long-lived radiatively active agents (ozone and methane) been properly accounted for in these analyses?

9. What about the direct radiative effect of the SOA change? Is that completely irrelevant for boreal ecosystems?

10. There are several problems with Methods on page 19156:

(a) “The generated climate was adjusted to the CRU period between 1960 and 1990”.

How was the generated climate ‘adjusted’? Which variables? Did you force with CRU for present day and ECHAM5.5 RCP8.5 for future?

(b) “Half of the SOA is assumed to be of such low volatility that it can partition to particles already at nucleation size (2 nm), increasing the formation of 2–3nm particles and hence the modelled total number concentration. However, most of the oxidized organic mass will be distributed to larger aerosols, increasing the coagulation and condensation sink.”

These 2 sentences are confusing. Isn’t it the remaining half of the SOA mass and not “most of the oxidized”? Please clarify.

(c) “All simulations are initiated with a six months spin-up, followed by one year simulation for analysis. The model is nudged towards year 2000 meteorology, reducing the noise arising from differing meteorological fields.”

This method is confusing and does not make sense. Firstly, a one-year simulation is not sufficient for an aerosol-climate sensitivity simulation. Meteorology has extremely high interannual variability in the boreal high-latitudes (e.g. Hawkins and Sutton, GRL, 2012; Mahlstein et al., ERL, 2011). Therefore, an estimate of interannual variability needs to be included in the paper as an uncertainty estimate (may be larger than the naked value computed from one simulation year e.g. -0.04 Wm-2). Secondly, exactly what nudging is applied? Is the ECHAM5.5-HAM2 model nudged to large-scale winds? Nudging the future climatic conditions at 2100 towards year 2000 meteorology is a wrong approach. The noise can be reduced by using consistent meteorology from the 2000s of the RCP8.5 simulation for the present day.

(d) The present day run should use the year 2000 anthropogenic emissions developed for the IPCC AR5 inventory that have been properly harmonized with the future RCP8.5 scenario (LaMarque et al., 2010). The AEROCOM emissions used in this study are not

consistent with the RCP8.5.

(e) How is it possible to isolate the impacts of changing wildfire emissions on CCN by holding both wildfire and anthropogenic pollution emissions fixed to year 2000?

(f) Line 22. E* = 1.9. It is only later in the results section that we actually find out what this variable is, including units. Please include units and definition here too.

Minor comments:

Page 19155 Line 21 repetition of “Larch, in this model setup”

Interactive comment on Atmos. Chem. Phys. Discuss., 14, 19149, 2014.