

Interactive comment on “Future threat to boreal ecosystem health from wildfire air pollution” by Xu Yue et al.

Anonymous Referee #2

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The authors discuss the hypotheses of a strong coupling between increased future biomass burning in boreal regions and feedbacks on the carbon cycle through air pollutant emissions. These feedbacks work mainly through aerosol impacts on diffuse radiation, and according to the authors less so through ozone. The aerosol feedback causes changes in atmospheric transport, leading to changing rainfall patterns and soil moisture.

While the results are overall fairly plausible, but speculative; the assumptions are not always well described and results not always sufficiently discussed.

A number of aspects of this study are particularly worrying:

- The relationship of aerosol optical thickness and NPP is based on correlations ob-

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served at two stations in Canada. The correlations at these two stations are pretty weak, perhaps because there are a number of other factors that are potentially constraining NPP. The extrapolation to other boreal ecosystems is adding large additional uncertainties. This makes the study with regard to AOD highly speculative.

- The results presented in this paper are much about the feedbacks in the earth system, changes in transport etc. Yet the authors use a fairly simplified climate modeling approach in which SST is fixed, and part of the feedbacks on longer time scales are excluded. I am aware of a similar earlier paper by these authors on China, where one of the reviewers has made a similar point- and the authors asserted that these feedbacks are not dominating. But what is the evidence for that? I propose that the authors add at least one coupled ocean simulation, and resolve this issue.

- As the authors convincingly show: changes in soil moisture are dominating the carbon cycle feedbacks. However, I haven't seen at all in this publication a discussion on the accuracy of the present soil moisture simulation. Clearly a good baseline modeling of soil moisture is prerequisite for estimating these future impacts. Moreover, the authors should give a better description of what is happening with the vegetation under dryer conditions and how that in turn leads to increased fire risk and burning.

- While the authors may be right that ozone impacts is playing a minor role at high-latitudes, the discussion is very much handwaving and unconvincing. This needs to be improved.

- The uncertainties and caveats described above should be much better described in conclusions and abstract. The necessary steps in modeling and observations to corroborate the findings here should be outlined better.

Despite these shortcomings, I find the manuscript interesting and potentially important. I would therefore recommend the authors to address my major concerns and resubmit to ACP.

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I have a number of more detailed comments below.

Detailed comments: I. 1 Title is not accurately describing the more limited content of the paper.

I. 29 scattering and absorption.

I. 38 The authors refer to Sitch et al (ozone flux based approach) in the text and here refer to a 40 ppb threshold- probably similar to a AOT40 type of metric. It remains unclear what has been done, and for instance which 'Sitch' (high sensitivity-low sensitivity) has been used. It would be good if the authors could clarify what has been done, and show their actual stomatal ozone fluxes.

I. 43 the authors will capture only partly the feedbacks since ocean temperatures are fixed SST modelling set-up.

I. 45 How much are these direct emissions and how does it compare to the feedback effects?

L 55 see I. 45. What is found in this study and how does it relate to the air pollution change in carbon budget?

L68: more uncertain- this is a value judgement- in reality we also do not know the ozone impact well either. Perhaps what the authors want to say is that the potential impact is even larger, and can swap sign.

L81: on the other hand: to me it looks quite consistent when considering the uncertainties.

I. 82-95: part of the differences can be due to just using different climate scenarios, and are more or less comparing apples and pears. I. 95: perhaps some words why A1B- and how it maps to RCPs (I think it is RCP6.0 equivalent). In the discussion you mention that the various scenarios until 2050 it is statistically almost similar, in my experience it is the 2050s where scenarios start diverging.

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L 113- this is a very short description. What sensitivity was included? How is consistency between the atmospheric model and land model ensured, how are fluxes calculated? Is Sitch still reflecting the newest knowledge? One can write a whole paper on what is here cryptically mentioned in one sentence.

I. 138-142 most relevant to discuss performance of MODIS retrieval over boreal areas even if it doesn't coincide with the flux sites. Summarize here what Strada found?

I. 162 would be good to provide the statistics in the supplementary and give summary here. It is really hard to understand here what is meant with 'much fewer' and how it can still be used.

I. 160-190 for clarity: future burning is assumed to be depending on fire-weather alone (regression relationship). Is there a relationship of fuel load with CO₂ and fire management, if not what could be the possible uncertainties from these assumptions? Not clear here if the climate simulations would include a feedback on fires via the fire weather risk.

I. 194 please give the values. What is meant with much higher?

I. 228-232 Give a short summary on what the flux scheme is about. Summarize in a few lines what was the outcome of this benchmarking, and the consequence for this study.

I. 252 how does the RCP8.5 scenario link to the use of the A1B scenario mentioned earlier (I 95).

I. 255 can a short description of the practical implications coming from the climate scenarios be given.

I. 258 does CO₂ impact fires and fire emissions?

I. 260 Explain better the model set-up: if area is burnt, does that also change the land-cover? Would that contradict the use of prescribed landcover?

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- I. 273-274 2 years spin-up and 10 years seems to be a short time scale for ecosystem responses. Can the authors comment to what extent this represents full response.
- I. 235-275 I would like to see a description of how the Yale model is treating regrowth after fires, and how the dynamics work out on time scales longer than 10 years. Can we expect an interaction between changes in age-structure and ozone and aerosol effects?
- I. 287 can you show in supplementary the interpolated fields for the relevant time periods?
- I. 300 would such a light saturation still be valid under changing CO₂ conditions? Please comment, and what could be the impact.
- I. 311 Correlation of AOD and GPP is weak to very weak. The value 3.5+/- 1.1 is just the average of the two slopes? What is the meaning of 1.1 is it one standard deviation based on two observation sets?
- I. 323 Indeed patterns look everywhere reasonable except the western part. What could be the cause of this. Any indication on MODIS data quality? Or missing sources in the NASA model that can explain this? Volcanoes?
- I. 326 What is compared here? 24 hr mean over June, July, August? Did the authors compare at the measurement altitude? I would recommend to focus on daytime values, as more relevant for ozone damage and usually less local conditions. The Sitch approach requires fluxes, and the methodology in this paper needs to be described as well.
- I. 331-341. The increase in emissions needs to be described here. How is the contribution of wildfire emissions present determined (zero-out?). It seems that the % increase in ozone scales near-linear with the NO_x (and other) emissions? But the contribution to AOD much less due to the abundance of secondary organics from BVOC emissions?
- I. 344-355 the ozone damage discussion is extremely handwaving and confusing.

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Where is the 40 ppb threshold coming from, and how does that compare to the use of the Sitch method? Only from Figure 4 I understand that indeed the Sitch high and low sensitivities have been used, but it is not discussed in the text. Anyway it seems that the model has a lot of data point above 40 ppb- but it hard to figure out how good the model performance is where the fire emissions have an impact. I. 357 at this point it is not clear what optical properties have been assigned to particles.

I. 369-381 The circulation feedbacks are an important result of the paper, but due to the approach of constraining SST will include only part of the feedbacks. I would argue that the authors should try to address in one additional simulation why they can ignore these longer timescales.

I. 402-414 We shouldn't expect a full attribution of feedbacks due to aerosol- so this is pretty convincing. However, as soil moisture is the most important feedback- I am missing here completely a discussion on how realistic soil moisture is represented in the current modeling system, and how the soil moisture feedback is leading to increased burning. At this moment a discussion of the short effects on the carbon cycle by increased burning is missing.

L 433 In Amazonia a large fraction (perhaps more than 50 %) is due to deforestation fires, and may not have a link to soil moisture. Discuss

I. 434 discrepancy or just a difference.

I. 457 Again- we need to know more about how good soil water is represented in the model- as the paper relies so much on the changes of soil moisture.

I. 439 actual->observed

I. 524 where is this number coming from.

I. 402-527 Discussion should better reflect the uncertainties of this work, and contrast them to other climatic effects on the boreal carbon cycle.

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