

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

Anonymous Referee #1

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The manuscript is unusual in that it considers the indirect effect of wildfires on the boreal carbon balance via emissions of atmospheric pollutants. The results are novel and the simulated effect is surprisingly large, which makes the results interesting for ACP. There is some validation of results against observations, and some evaluation of effect strength directly using site-based observations. This strengthens the paper, which otherwise relies on a very complex modelling system. I consider the subject material to be fully within scope for ACP. However, the title does not correspond to the above assessment but sets different priorities. Possibly it reflects the original idea for the manuscript on ecosystem health but the focus has changed due to the negative results regarding ozone pollution. The manuscript therefore seriously lacks focus.

Major comments:

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I can see at least the following scientific questions either being addressed, or requiring attention:

- 1) Do wildfires affect ecosystem health in boreal environments beyond its direct impact through higher ozone concentrations, i.e. far away from the fire or long after the fire has ended?
- 2) How can the effect be quantified, i.e. is NPP a valid proxy?
- 3) How do the direct and indirect effects compare?
- 4) Will the strength of this effect change in the future?
- 5) How do wildfires affect the carbon balance of boreal environments indirectly through atmospheric pollution away from the burned area?
- 6) How does this effect compare to the direct effect on the carbon cycle in the burned area?
- 7) How will this change in the future?
- 8) Are the results representative of all boreal regions?

Questions 1-4 correspond to the title, but Questions 5-8 to the actual focus of the paper (but still not all of them are being answered).

In order to become publishable, either the title needs to be changed to reflect the true focus of the paper, or the focus of the paper needs to be changed and much more detail on ecosystem health effects need to be included. The latter is probably beyond scope, so the best way forward must be the former. In that case, however, more depth is required regarding the carbon cycle, as NPP is only one of many components, and all of Questions 5-8 need to be answered. If the impact on the carbon cycle were to be the focus, then the title would have to be adapted and the manuscript would have to include more discussion that puts the results into the perspective of the regional and global carbon cycle. Some of it is there, but not enough to give the reader a sufficiently

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good feel for how important this really is. So if the focus is to be on the carbon cycle, more results need to be included or a more detailed and in-depth discussion is needed. Or, what is also possible, restrict the paper to impacts on NPP alone. After all, you also include GPP, and that is already a step that involves changes in plant respiration, which also need to be projected. What happens here?

Finally, the result must be backed up more by measurements. The main effect is surprising, but it will be crucial that there is a thorough evaluation of how the model simulates the impact of changes in diffuse and direct light on GPP, as opposed to the measurements.

The third possibility would be to simply focus on the effect of atmospheric pollution from wildfires on GPP (not NPP) in boreal North America (and change the title accordingly).

Another major comment: the different chains of events discussed here are enormously complicated and the effect is very indirect. I suggest the authors show this in a suitable graphic. We have changes in climate affecting fire weather, but also affecting vegetation composition and fuel load. In addition we have changes in land use, in particular forestry and fire management (See Fig. 3 in Doerr and Satin showing for the U.S. increasing burned area, fewer fires, and an enormous rise in fire suppression costs). Both impact burned area and fire emissions. But then we also have atmospheric circulation patterns which are influenced by all sorts of things, among them greenhouse gas concentrations and aerosol load, some of it from boreal forest fires. And all of these together influence boreal forest NPP which in turn impacts the regional and global carbon cycle. Given this enormously complex web of causes and effects, I am not sure what we really learn here. It is up to the authors to clarify and give us a clear picture of what this paper is really about. Do that, I suggest considering the main questions and sub-questions as above, and then re-structuring the paper in order to answer them all in a systematic way. Much of it is there, but the information is too scattered.

Increases in boreal wildfire activity: this manuscript builds heavily on Yue et al. (2015),

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which in turn builds heavily on Yue et al. (2013). This compartmentalisation of research is necessary given the said complexity of the subject. However, the foundations and basic assumptions on which the story rests here get a bit lost. This is particularly true for the fundamental assumption of increasing wildfire emissions, which here is stated as a matter of fact. While total burned area and even more average burned area per fire in the U.S. have increased in recent decades, it is far less clear whether burn severity has increased as well (again: Doerr and Santin 2016). And burn severity is linked to the total amount of fuel combusted which is proportional to the emissions of carbon (but not necessarily to O₃, NO_x etc.). For all these, burned area is a necessary but not a sufficient predictor.

The fire prediction used here by the authors is based mainly on fire weather indices. The approach is statistical, and scientifically certainly valid. However, there are other approaches that need to be mentioned and recognised. For example, the method used by the authors neglects the influence of changes in vegetation and fuel load on fire spread (please correct me if I got that wrong). But wildfires don't only need favourable fire weather to spread, they also need sufficient fuel and a continuous fuel bed. If it burns more often, there will be less fuel to burn and fire spread may be reduced. Has this negative feedback been taken into account? Has the impact of changing vegetation cover on burned area been taken into account? All these need to be better discussed.

Specific comments:

L29: This is a factual statement about the future. These should be avoided in the scientific literature.

L36: this is not 'boreal' area burned. North America does not even comprise half of the boreal zone.

L38: ambient [O₃] - could this rise above critical thresholds close the active fires? The statement sounds as if it was referring to average conditions and it does not take into account the episodic nature of wildfires. This is later discussed (L350ff), but it would

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be good for the reader to learn this already here.

L53: please provide more recent examples, there are plenty.

L64: I suggest dropping the topic of plant health altogether in this manuscript. Sitch et al. (2007) is about the carbon cycle and stomatal closure, and does not address the question of plant health.

L76: would drop the word "changes" here: aerosols impact the nature of the radiation, which impacts NPP. But changes in NPP do not necessarily mean changes in C uptake. This depends on changes in respiration. Needs discussion.

L95: usually, ensemble averages fear better when it comes to whether, seasonal or even decadal climate prediction. If this also applies to climate projections, however, is not something we know for sure.

L127: "The number . . . is much fewer. . ." Awkward. Better: "There are much fewer . . .".

L154: -> "A cloud mask applied to..."

L182: What is was trying to understand here is whether fuel load is constant through time. It sounds like. This is an important point that needs to be clarified and discussed through the manuscript.

L292: In addition to the observed GPP-PARdiff and GPP-PAR relationships, there should also be a sub-section on modelled GPP-PARdiff / GPP-PAR relationships. I say should, but in fact this will be crucial in order to establish the credibility of the present manuscript.

L305: This paragraph could mention that the AOD-GPP slope at CA-Gro is not significantly different from zero.

L319 "within 20%" requires continuation with "of . . .".

L417: Yes, but what about the model?

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L443: I disagree. Long-term radiation changes will certainly be reflected in shade/sun adaptation of the leaves. If there is less PAR, then saturated rates of photosynthesis will decline making photosynthesis more efficient at lower rates of radiation. This is already included in the original model by Farquhar et al. (1980), which you cite here.

L467: I agree, intuitively, but I think there is no way we could quantify those uncertainties.

L516: I would really like to understand what you mean by a "missing land carbon source due to future wildfire pollution". Is the source missing now, or will it be missed in the future. And who will miss it anyway? Can you see how cloudy this statement is? But this is a good start for getting more in-depth as far as the carbon cycle is concerned (see major comments). Doesn't your model simulate the full carbon balance, including soil carbon? What happens there? Or if not, what could happen?

References:

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