

Interactive comment on “Trends and spatial shifts in lightning fires and smoke concentrations in response to 21st century climate over the forests of the Western United States” by Yang Li et al.

Anonymous Referee #1

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This paper presents future projections of burned area and smoke concentrations from lightning fires on national forest and national park lands in the western US. The paper is generally well written and presents some interesting results. However, I think it could use some clarification before publishing.

Major Suggestions:

- I'd really like a figure that shows specifically the domain that they are looking at with all the national forest and national park lands outlined. This might be the green line on Figure 3, but it is not labeled as such in the caption. Additionally, I think any parks/forests that are mentioned by name in the text (example line 282-283) should have their state

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location listed and be labeled on a map (it should not be assumed that all readers know these locations by name).

- I know this will make it wordy and redundant sounding, but I think the authors need to be explicit throughout the paper, every time they mention results, that all their results are only from fires on national park and national forest land in the western US. I think this is especially important in their discussion on smoke concentrations and their comparisons with other studies. It should also be specific in the title.

Line 55- 58 states that one of their aims is to provide results at a higher resolution. I think with this being one of their stated goals, there needs to be more discussion of resolution. They did model simulations at two resolutions, so how do these two resolutions compare? What value does the finer resolution add? How might this finer resolution impact comparisons with other studies?

Minor suggestions:

- It should be “western United States” not “Western United States” throughout the paper. It is incorrect in the title and abstract and switches back and forth throughout the text. I also think national parks and national forests shouldn't be capitalized unless the authors are referring to specific national parks or forests.

- About half-way through the paper, the authors stop using “National Forests and National Parks” and just use “National Forests”. I think they should stick with parks as well.

- A flowchart of the modeling set up in the supplement would be beneficial. I found it difficult to follow the input/output of each step in the modeling process. They also need to be clear throughout the text about what each model is actually simulating. For example, they say that LPJ-LMFire simulates meteorology (line 339), but I think they mean that it simulates the effects of meteorology and the meteorology is input. Likewise they say that LPJ-LMFire simulates emissions (line 88), but I think it simulates

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area burned, and then they apply the Akagi emission factors to create an emission inventory for GEOS-Chem. (Example: line 39, lightning-caused fire emissions aren't simulated with GEOS-Chem, they are put into GEOS-Chem)

- Table 1 should also have the total BC+OC emissions. I don't think the Dm for the Sierra Nevada needs to be included here. I'd suggest instead adding a supplemental table with several of the large national forests and their results.

- I don't think Table 2 needs to be in the main text.

- I think Table S1 needs to be in the main text since 2 whole paragraphs discuss it.

-Figure S3 is mentioned in the text as an evaluation with GFED4s, nothing about IMPROVE. I was really confused when I read the acknowledgement section that a large section devoted to IMPROVE when there was no mention of it in the text. This evaluation should be mentioned in the main text, likely under section 2.3.

Line by Line Comments:

Line 19-20: restate that this is for national park and forest lands in the western US.

Line 21-22: This is confusing. Isn't the dry matter burned by lightning-caused fires? A shift in fuel loading could lead to more fires, but if it is already burned, should it not lead to fewer fires?

Line 29-32: Brey et al. (2018) suggests that it is about 30% caused by human ignition in the west. They also note that there are similar drivers for lightning and human caused fires, thus climate changes would likely have a similar impact on both.

Line 35: Studies of what? Be specific.

Line 81: Is a second source missing here (there is a comma and the sentence says "Several studies")? If not, the sentence should read "One study predicted". Also, is there not any more recent papers on lightning and climate change?

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Line 83-85: It might be worth noting that this lightning parameterization does not include any potential impacts of aerosols since this work is suggesting an increase in aerosol concentrations.

Line 86-87: I think it would be beneficial to restate this at the end, that lightning isn't increasing, but the area burned from lightning fires is.

Line 91: Is a couple years a long enough spin-up for a vegetation model?

Line 88-94: seems like this should just be in the methods section.

Line 94: Is a five-year time slice long enough to represent the range of interannual variability?

Line 108: What does the "coalescence of fires" mean?

Line 115-117: How does the model go from lightning density to fire? Does every lightning strike initiate a fire if there is fuel there?

Line 139: can you use "grid" instead of "raster"? Also, this needs clarification. Is this grid used to create the emissions or just for choosing the analysis area? I'm assuming this is for creating the emissions and the authors use the fraction of the grid box multiplied by the dry area burned and then that gets multiplied by the emission factor to create the emissions to be put into GEOS-Chem? And then for the analysis, do they use any grid box that has any fraction of national park or forest land?

Line 161-162: is this lack of difference for the CTM or LPJ-LMFire and for what variable (20% for emissions seems significant?)?

Line 164-167 should be moved to line 158.

Line 176: why do the GFED4s emissions need to be included at all? If you are just looking at the difference and those are being held the same, it doesn't seem necessary to include them in the simulation at all. Line 178-179 says that they can be compared to observations, but this isn't actually done in the text at all.

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Line 196-213: What is causing these increases? Just the warmer climate or is it the shift in biomass type? Does the decrease in precipitation not have a large impact?

Line 213: will not "limit fuel load" for what or with respect to what?

Line 236: changes in what?

Line 243-247: This is a long, confusing sentence.

Line 263: what region? The SN or WUS?

Line 267-268: and the model doesn't simulate this right? Otherwise, you'd need to also include gas-phase precursor emissions calculated for your fire emissions.

Line 273-274: Figure 3 does not show lightning fire activity. It shows changes in dry matter burned and total living biomass.

Line 303-304: be specific that this is for the western US. Also, can you show a map of this, maybe in the supplement? Is this because your area includes any grid box that has any fraction with national park or national forest land? Less than 1% seems really low (protected lands make up <20% of the US)?

Line 312-316: It seems strange to put in the same sentence that there are low smoke emissions compared to some studies, but similar area burned to another study. Do the two studies for the smoke emissions also provide area burned estimates? Otherwise, these should be discussed separately.

Line 308-333: The domain difference and difference in years should be noted along with the difference in resolution.

Line 334-345: Also, there is no feedback of smoke/aerosols on climate included. Also, transport pathways may not vary much, but there are likely some mismatches in the CTM simulation in that the meteorology that is conducive to fires may be more conducive to smoke transport, and the CTM is not using the same input meteorology that was used with LPJ-LMFire.

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