Review of:

Global patterns and drivers of climate-driven fires in a warming world

By Bhattarai et al, egusphere-2025-804

The authors present future simulations under two different scenarios with the fire-enabled DGVM CLM5, and report burnt area and speciated emissions. They examine the drivers of global burnt area using simple univariate linear regression, and further focus on the northern latitudes with three machine learning models. The study is timely, relevant and well-presented. The chosen methods are appropriate. However, I have several scientific concerns with the paper in its current form.

1. I agree strongly with comments from Reviewers 1 and 2 concerning additional benchmarking. Whilst the spatial patterns are indeed reasonable (for a fire-enabled DGVM), both the seasonal patterns and more importantly, the temporal patterns - both interannual variability and trend - should be evaluated over some a reference period. Perhaps 2001 to 2015 which according to the methods was simulated as part of the spinup but not presented here. Regardless of the protocol, when presenting these future simulations, some degree of evidence should be offered that the simulations reliably capture the temporal dynamics and interannual variability. Ideally this should be done regionally as well as globally. Otherwise the future projections are hard to credit.

   It may be that such benchmarking already exists, but it doesn't seem to be in any of the references provided in the manuscript. As part of the FireMIP project, Hantson et al. 2020 found the the CLM fire module, whilst amongst the better performing fire-enabled DGVMs, only managed to better the mean value null model with respect to two of the four burnt area dataset to which it was compared (and even that barely). Can the authors provide evidence that the model has been improved since then?

2. I also question the utility of parts of Figure 5, at least as a main manuscript figure. Many of the relationships would entirely expected. Especially because the correlation uses monthly values, and so the seasonal patterns should be expected to dominate the correlation in seasonal areas, this includes of course, soil moisture. I suggest using remaking this figure with annual values, or perhaps better would be fire season months or fire season aggregates, or perhaps deviations from the long term mean. As things stand Fig. 5 is not useful.

   More than this, the discussion should be more nuanced. For example, line 332-33

   > "However, in tropical regions, where fuel is already abundant, increased precipitation primarily raises soil moisture, further suppressing fire activity rather than promoting it."

Actually in the arid tropics fuel can often be limiting so this is not a reasonable comment. Furthemore, line 330-332

> "Interestingly, in boreal regions, precipitation exhibits a positive correlation with BA, as it enhances vegetation growth, increasing the availability of fuel."

Again, this is against the conventional wisdom here. The boreal zone is not considered to be fuel limited because actually there is a lot of both live and dead biomass available as fuel.

3. More detail is needed about the ML methods in Fig 8, but I am also not convinced about their utility. I *guess* they were run on the model outputs and climate inputs as a kind of emulator, and then feature importance was extracted. But emulators should be used to make a short cut to produce results, *not* extract process knowledge. Since they essentially work on correlations, there is no guarantee that they are getting the right result for the right reason, therefore they are truly capturing the model's cause and effect. This is in indeed evidenced by the author's own results which show quite different results between ML methods. Having said that, the results are fairly consistent in that they say veg C and soil water content are the most important variables, so perhaps this is all good enough. But since those two things are in themselves very likely to be very correlated (assuming a fuel limited system), I am not so sure what are really learning. More details here please. In fact there are likey to be strong correlations between many of these variables, this should be explored with some pair-wise correlation plots.

4. Instead of running statistical models on model output, I would *strongly recommend* that the authors lean into the strength of a process-based model such as CLM and run sensitivity experiments i.e. fixing temperature, precipitation and CO_2 to present day conditions. This will isolate the effects of the changes to the individual driving variables in a much cleaner way.

5. In general CO_2 effects - both increased water use efficiency which will lead to increased soil moisture and fertilization which will lead to increased biomass – are not adequately discussed. How will these be affecting the results here. How much is the effect here climate change *per se*, and how much is CO_2?

6. Similarly, soil moisture does not *directly* affect fire (apart from ground fires which maybe included in the peat fire module but isn't clearly discussed). It does affect live fuel moisture and serve as a proxy for dead fuel moisture, but these are somewhat indirect. The role of soil moisture as a proxy needs to be discussed, particularly how this plays out in the model logic.

7. It would be very interesting (and important) to give some idea of of which fire types are increasing. Presumably it is the peatland and "everything else" category since land use is constant. But the relative proportions of these, given the strong high latitude signal, is important.

8. The analysis of the "high latitudes"/"Boreal" is rather broad and rather inconsistent. Fig 8 is "northern latitudes" between 30 and 70 North, but this includes all sorts of (seasonally) hot and/or dry ecosystems such as the Mediterranean and the continental interiors. Figure 7 is stated as the boreal region, but at > 40N this definition also includes a lot of non-Boreal ecosystems. This is not just a semantic issue, there is a big difference in driving dynamics, especially fuel- vs moisture-limited systems across these regions, and it does seem like this is a key issue of this paper. I also don't think it is reasonable to say that tundra dominates at 60N (line 251), there is a lot of forest at that latitude.

9. Was dynamic vegetation enabled or not? It isn't clear. This needs to be fully detailed, and if not the potential effect on the results discussed.

10. In Fig. 2, why is the SSP1 line much higher than the SSP3 line? The authors report a higher increase in SSP3 (not unexpected), but it is still lower that SS1 at the end of the century. The authors need to to explain what is going on here. Also, I don't find the 25 year moving average to be useful or convincing, especially when it seems not to be centered on the actual year in question (look at about 2080 for example, time series goes up, moving average goes down).

11. Also in Fig. 2 (and others) what is that repeating cycle in results? And why are SSP1 and SSP3 so similar? It doesn't seem like the input climate data was actually two independent GCM runs. Please explain.

Concerning the manuscript itself:

12. I think the title should be adjusted. What are "Climate-driven fires"? I think the authors are trying to say that they are only simulate changes in fires due to changes in climate, but the title doesn't express that.

13. The language needs some overhaul for clarity and narrative flow. This applies throughout the Results and Discussion sections, but lines 250-259 really typify this. Whilst the text does kind of describe the results, it is unclear what point we are supposed to take from this formulation.

14. Line 314 – what is "detrained"?

15. I don't find that the speciated emission add much value to the paper. They are barely discussed.

16. There needs to be more discussion of the effects of not including changes to the human aspects, especially as it is now well accepted that global burnt area is going *down* due to anthropogenic effects (Andela et al 2017, Science). This suggests that the simulations presented here (which shown the opposite trend with increasing burnt area) aren't actually capturing the dominant global effect. This needs far more discussion, much beyond the few lines 495-498.

17. Line 511 – I can't agree with the assertion that "afforestation/reforestation in fire-prone regions can reduce fire risks while enhancing carbon storage." There is no evidence to support that (in fact the author's own results suggest the opposite). Suggesting afforestation or reforestation in fire-prone areas for enhancing C storage seems like a bad idea.

18. The manuscript structure is somewhat unconventional. The Methods section is rather short, but the Results section is very long and introduces new methods as it goes. As Reviewer 1 points out these aren't fully explained (indeed what exactly is show in Fig 8?) so more detail is required. Additionally, I would suggest moving all methodological details to the methods section.

19. Abstract line 22 – when you discuss the drivers individually it sounds as if they were tested individually, but they weren't, please rephrase.

20. Abstract line 23-25 – this sentence combines both present day evaluation and future projections, please split and rephrase.

21. Abstract line 28-31 – Please reconsider referring to soil moisture as a “driver” of wild fire (see above) and also reconsider your mention of CO_2 fertilization as a driver in the abstract. This was barely mentioned in the results and wasn’t explicitly studied, and increased biomass could also come from warming and wetting.

22. Line 256 – no one could say that decline in the tropics under SSP3 is “sharp”.

23. I don’t find an adequate reason or discussion for burnt area going down in some regions in the tropics and, for example, the UK and Eastern US.

I will refrain from making more detailed comments on the manuscript as I believe the comments above may results in some re-writing and re-interpretation. I will be happy to give such comments if reviewing a revised version of the manuscript.