Review of "Global wildfire patterns and drivers under climate change"
by Bhattarai et al, egu-sphere-2025-804

The authors have addressed many of the concern raised by all the reviewers, but some issues remain and some more have become apparent after more methodological details were included and after further consideration.

1. The inclusion of benchmarking of the seasonal cycle is welcome step, but an evaluation of the time series is still sorely lacking. By this I mean the an evaluation of the year-to-year interannual variability of the burnt area (ideally the global total and in some subregions) and an assessment of the trends. Since the key result of the paper is how the annual global totals change in the future, some confidence is needed that the model correctly reproduces the dynamics that produce these changes in the totals through time.

   The seasonal cycle benchmarking (the new panels e, f, and g in Figure 1) don't inspire a lot of confidence given the mismatches in timing. That suggests the model might be sensitive to changes in the climate conditions at the wrong time of year (for example summer conditions as opposed to spring and autumn conditions in the NET), calling in to question the validity of the projections. The key thing in the paper is the large scale responses to changes through time, so this data-model mismatch might be acceptable if the model can produce the interannual variability and trends of the present day correctly. But this is not demonstrated.

2. With some more details and further thought I can no longer credit the XGBoost analysis (or the other ML methods) as being useful for explaining the reasons for the reported modelled changes in BA (i.e. specifically determining the drivers of the large increase in NET burnt area). This is because these results will be highly driven by the spatial variability (which will be large over that extensive region) and the seasonal cycle (which again will be a large affect). There will be some temporal signal in there (assuming data from all years were used, this isn't specified) but likely the spatial variability and seasonal cycle will be dominating. In other words, the ML analysis exposes how the drivers affect the model in general, not specifically what is driving the change in the SSPs. This is further evidenced by the fact that the two SSP plots are nearly identical, indicating that the analysis isn't sensitive to specific change pathways, but rather the overall model structure.

   In order to do such an analysis meaningfully, one would need to use ML to understand the *change* in burnt area in terms of the *change* in the drivers – i.e. isolate the temporal (note not the seasonal) component.

3. Following on from this, the some key conclusions of the paper are not supported. Consider line 529:

   "Our findings indicate that boreal regions, especially around 60°N, could experience a staggering increase in BA by up to 200% under high-warming scenarios (SSP3-7.0), primarily driven by reduced soil moisture and increased vegetation carbon, creating dryer and more combustible conditions."

   This issue is with the "primarily driven". Discounting the ML methods for the reason above, we can look at Figure 7 which shows both the change in burnt area and the drivers spatially. The increases in TOTVEGC (panels r and t) are almost completely spatial distinct from the increases in Burnt Area (panels a and c) so "increased vegetation carbon" cannot be driving this BA increase. Also the correlations plots in Figure 5 do not show a strong correlation between TOTVEGC and burnt area. Overall, these results are consistent with the conventional wisdom that high latitude systems are not typically fuel limited, and not with the stated conculsion that "primarily driven by reduced soil moisture and increased vegetation carbon". And this paper does not present any convincing evidence to contradict this conventional wisdom.

   Soil water as a driver (panels m and o) is more feasible as these systems are more likely to be dryness limited. But the soil moisture increase is very uniform in space but the burnt area increases are not. So there must be something else going on.

4. The repeating pattern in the interannual time series of the results (seemingly about 12 years) was not adequately explained. Both SSPs might have been started from the same initial condition ensemble member of CESM as the response letter suggests. But this does not explain the repeating pattern, and neither does it explain the similarities between the SSPs since it is well-known that GCMs/ESMs with different forcings rapidly diverge in terms of their internal variability, even when started from the same state. What I *guess* is going on here is that the data in question are some form of present day baseline data which has been adjusted using climate anomalies from SSP scenario runs. But I can only guess. This must described as these projections are central to the manuscript.

5. I am not sure I agree with the decision to detrend the data for the correlation plots in Figure 5. Surely the correlations in the long term changes (such as increasing fuel and decreasing aridity) are exactly what this figure should explore. By detrending the data, the correlations are only sensitive to what happens within the repeating pattern of 12 years of interannual climate variability (see above point) which, given this short period, may result in model artefacts and doesn't investigate the long term changes.

6. The structure of the paper has improved by the consolidation of the Methods, but there is still a lot of discussions in the results section. For example, lines 418 to 423 and lines 504 to 524. These are discussion points, and so they (and similar such text) should be moved to the discussion section.

7. Some of the analysis and discussion (in particular the machine learning and correlation plots) is focussed on understanding what the fire model does *in general* . But this should be known from the design and construction of the model! It is not a black box, it is a processed-based model. It may be somewhat complex, but many of the relationships uncovered in the paper are self-evident. Higher temperature will promote fire. Lower soil moisture will promote fire. If a fire model doesn't produce that (all else being equal) something is badly wrong.

   For this manuscript, the key thing that needs to be demonstrated is that the model represents these relationships reasonably well, at least in terms of their combined effect, compared to observed patterns (in this case most important is the year-to-year temporal patterns). After that, it should determine which of these drivers/relationships are causing the future changes. In its current state I don't believe the manuscript achieves that.

8. I would again stress that this study doesn't take advantage of one of the key advantages of processes-based models. Specifically, when investigating drivers (as this paper does), the most unambiguous method is to perform sensitivity experiments where the effects of individual drivers are changed individually and so their effect on the results can be assessed. Obviously this is trickier when it comes to intermediate variables which rely on multiple drivers (vegetation C, soil moisture, CWA), but that doesn't mean that it can't be tackled with another layer of analysis.