Response to Reviewers of ‘Global and Regional Impacts of Forest Expansion on Future Wildfires’ by King et al.

We thank the anonymous reviewers for their time and effort in peer-reviewing our manuscript. In this document we provide responses to their comments, outlining changes made to the manuscript. The reviewers’ comments are in italics with our responses below. Text added to the manuscript is in green and text removed is struck through, with existing text in blue.

Reviewer 1

Major Comments:

The term nature-based solution (e.g. line 38, 86)

This is a widely applied term, but in the context of climate change mitigation, it is especially misleading. Indeed, some scholars frame forestation and similar land use changes as "solutions", but literature clearly shows that these cannot replace emission-reduction to reduce net emissions fast enough to keep limits defined in the Paris Agreement in reach. Especially given your results, a more critical framing, e.g. with "solution" in quotation marks is justified to avoid the reproduction of such misleading framings.

We agree with the reviewer that ‘nature-based’ or other LULC-based approaches to climate change mitigation cannot by themselves be a ‘solution’ to climate change and are careful not to suggest that they could be conceived as an alternative to emissions reductions, as outlined in the conclusion section. We use the term ‘nature-based solution(s)’ as it is widely applied in the peer-reviewed and policy literature to refer to the type of mitigation that this study investigates; it is thus intended to enable the reader to see how the study fits into the existing research and policy landscape without implying endorsement or dismissal of the value of such approaches. We have placed the phrase ‘nature-based solution(s)’ in quotation marks as suggested to indicate the critical framing of the paper.

Forest expansion halts deforestation (e.g. line 46/47, 530-538, 567-569, 625/626)?

It is hard to grasp what you mean. In most literature and in practice, avoided deforestation is discussed as distinct from forestation. With this as background, please reformulate or avoid entirely to clarify. Otherwise, what you present as finding seems to be trivial, as it is basically in the scenario assumptions. If there is an important reason to keep it, please rephrase it to transparently show the triviality or what else you deem to stress here.

In our forest expansion scenario ('Max Forest'), deforestation stops because the scenario expands forest cover from a fixed starting point while holding other types of LULCC constant. This is different from the SSP land use change scenarios in which some deforestation occurs on a regional scale even in scenarios where forest cover increases on a global scale (e.g. SSP1). We are not referring precisely to 'avoided deforestation', which is in practice an important consideration for e.g. carbon credit-based offsetting, but a reversal of deforestation trends. What we mean by this is that, in Max Forest, locations where deforestation had been occurring prior to the start of the scenario have that trend halted and reversed. We agree that this is an outcome of the assumptions encoded in the scenario as we have explained, but disagree that it is trivial because it has important consequences for changes in simulated fires under forest expansion. We argue that it is further relevant because of the Glasgow Declaration on Forests and Land Use, in which parties to the UNCBD agreed to halt and reverse deforestation by 2030. We have added some more detail in the methods section to emphasise what we mean by 'halting deforestation', noting that the scenarios used in this study have been detailed at length by Roe (2021), Weber et al. (2024), and King et al. (2024):

Line 183:

This scenario increases forest cover by 750 Mha by the end of the 21st century (Weber et al., 2024) by expanding fractional coverage of pre-existing tree PFTs into neighbouring gridcells following climate and land-use constraints (Figure S6). Since other LULCC processes are held constant, this has the effect of halting and reversing deforestation trends as a result of the assumptions used to generate the scenario (Roe 2021).

Line 532:

Since No LULCC and Max Forest scenarios eliminate future deforestation by definition, as a result of scenario assumptions, these fire types are reduced almost to zero.

Misleading comparison of cumulative carbon sink reduction to present-day emissions

In understand the desire to communicate the carbon flux in terms of well-understood present-day emissions. However, please use great care with comparisons of temporally integrated carbon uptake potential with emission rates. Such claims of "equivalency" artificially suggest that 5.6 years of current emission are added from the land carbon budget until 2100. It is precisely the comparison of integrated flux with flux rate that misleads here: The carbon released by fires in expanding forests until 2100 enter the atmosphere at different times in the chronic of global warming *then present-day emissions and has different impacts on peak warming, overshoot duration, and so on. In awareness of the sadly widespread usage of this misleading comparison, I suggest the following: In the abstract, use the maximum over the period 2015-2100 of annual emission rate of additional fire.*

Complementarily, in the discussion, you can draw this comparison and debunk the misleading typical claim of equivalency of cumulative carbon uptake over the 21st century with present-day emissions. Moreover, a comparison with other cumulative carbon sink changes will be informative (see below).

We appreciate the point about future emissions and/or sequestration not being equivalent to present day emissions because of changing baselines and consequent different impacts on warming. This is why we reported changes in the total size of the land carbon sink, rather than (for example) converting the fire emissions to an estimated warming rate. We are not arguing that 5.6 years of the current carbon budget are added from the land carbon budget until 2100; rather that, in 2100, inclusion of the effects of wildfires in an estimate of the potential land carbon storage under forest expansion causes that estimate to be substantially reduced. The statement about the magnitude of the difference between the 'fire' and 'no-fire' estimates being equivalent to the magnitude of carbon emitted in 5.6 years at current rates is purely illustrative. However, to strengthen the paper we have added the metric suggested by the reviewer, and detail below where we have made changes to this part of the analysis following the reviewer's specific comments.

Deeper and more ambitious analysis and interpretation needed

For large parts of the paper, the authors have accumulated statements about findings that are not novel at all. For a large part, they are directly deducible from the model structure, without simulations.

For example, this is the case for like "Our results also highlight the importance of population change as a key driver of future fire activity" (line 548) and relate it to "Our model assumes that fire suppression scales with population" (line 556/557). It remains unclear however, where the analysis of the simulation output actually provides new learnings beyond the assumptions introduced to the study in the model structure.

However, this is also the case for the effect of fire on long term mean carbon stocks (cumulative sinks). The authors only state the obvious, the sign ("reduction"), and quantify the reduction in stock (roughly 60 PgC) for one scenario, which is expected to be a particularly model-dependent metric. However, they neither provide information on systematics in scenario-dependence of this value, nor do they adequately contextualize it as fraction of changes in stock from climate change, land use change or population change, nor do they quantify the size of interaction terms.

Deeper and more ambitious analysis is needed to use the simulations for added learning, which they are clearly potent for, in particular the quantitative assessment of relative contributions of population, forest cover and climate changes to fire changes in maps and aggregated in meaningful regions. Below, where applicable, I tried to suggest specific additional analysis and steps one could undertake to enhance the value of the presented analysis.

We appreciate the reviewer's comment that our simulations have potential to generate additional insights into these processes with additional or reframed analysis. We agree that the fire suppression impact of population density growth is an assumption of the model and can therefore be deduced as a result, and indeed we highlight how this 'non-physical' process is as important as physical processes such as climate and fuel load changes in this particular model. However, we argue that using different population scenarios and applying control experiments with fixed population add value by quantifying the magnitude of this effect in different parts of the world. It is important when using complex models to state the assumptions, but that does not necessarily mean that results that arise largely due to these assumptions are not worth discussing, in particular when discussing the limitations of using a single model (as we have sought to do here).

We thank the author for their detailed feedback on how to strengthen the analysis of the changes in the size of the land carbon sink due to land use and fires under different scenarios. We have responded and made changes in the specific comments section below.

Minor Comments

Line 49/50: this is an interesting finding. For a stronger statement, please consider reporting the factor applying in low-warming scenarios. If even in such moderately heated world, fire emissions increase drastically, readers will be able to anticipate the even more compromising effects in a strongly heated world.

We have added the following:

In contrast, temperate forest expansion, such as in the Mediterranean, central Asia and continental US, can more than double fire carbon emissions under high warming (and increase by 1/3 under lower warming), due to drier conditions and increased fuel loads.

Line 53: "Finally, fires reduce, …."

- "Overall" instead of "Finally"?

- the effect of fire (not to be confused with the fire in added forests)

We have changed this sentence to:

~~Finally,~~ the effect of fire reduces the global land carbon sink…

Line 54: Please clarify with more explicit wording whether this is avoided carbon flux rate integrated over time (which start date?). If you aim to compare it to present-day emission rates, for clarity and interpretability in terms of negative emission potential please chose the same unit (gC/year).

As explained in our response to the major comment above, this refers simply to the difference in the size of the land carbon sink at the end of the 21st century in each experiment and is not a flux rate over time. We have added additional metrics as requested by the reviewer.

Line 57: "interactions" instad of "feedbacks"?

We have changed 'interactions' to 'feedbacks'.

Line 58: "climate change" instead of "CO2"? CO2 is not mitigated, climate change ideally is.

The phrase 'carbon dioxide mitigation' is widely used in the literature (e.g. (Ou et al., 2021). Forests absorb and sequester CO_2 specifically, as opposed to other greenhouse gases. Therefore, any forest-based climate change mitigation strategy is aiming specifically to mitigate emissions of CO_2. Following the reviewer's suggestion, we have changed this to 'CO_2 sequestration' to improve clarity.

Line 62: "process in" instead of "component of".

We have changed 'process in' to 'component of'.

Line 62/63 "It is one of the most significant controls on the carbon cycle": this is a bit of an overstatement. Its acting only on land, and only in certain landscapes. "one of the most significant controls of the land carbon cycles" might me more adequate.

We have changed this to 'the terrestrial carbon cycle'.

Line 64: "feedback responses": this term is unclear to me. Do you mean the alteration of the feedback process fire?

We mean that fire both affects and is affected by these other components of the Earth System. We have changed ‘feedback responses’ to ‘interactions.

Line 86: I propose “as a so called nature-based solution” This place could be the occasion to introduce the criticality of the term that wrongly presumes effectiveness.

We agree with the reviewer’s suggestion to adopt a critical framing of this term (it is also not clear to us what is ‘nature-based’ about large anthropogenic interventions in the biosphere). The term is used because it is widely adopted in the literature, but we have changed this to ‘as a so-called ‘nature-based solution’ ’.

Line 100-102: This concern has been raised in previous literature, e.g. Hermoso, V., Regos, A., Morán-Ordóñez, A., Duane, A., and Brotons, L.: Tree planting: A double-edged sword to fight climate change in an era of megafires, Glob. Change Biol., 27, 30013003, https://doi.org/10.1111/gcb.15625, 2021.

there has been a study without process representation pointing against the concern:

Golub, A., Sohngen, B., Cai, Y., Kim, J., and Hertel, T.: Costs of forest carbon sequestration in the presence of climate change impacts, Environ. Res. Lett., 17, 104011, https://doi.org/10.1088/1748-9326/ac8ec5, 2022.

and there has been a study raising the concern specifically for large-scale forestation-reliant mitigation pathways with forest expansion patterns similar to the one you use:

Jäger, F., Schwaab, J., Quilcaille, Y., Windisch, M., Doelman, J., Frank, S., Gusti, M., Havlik, P., Humpenöder, F., Lessa Derci Augustynczik, A., Müller, C., Narayan, K. B., Padrón, R. S., Popp, A., van Vuuren, D., Wögerer, M., and Seneviratne, S. I.: Fire weather compromises forestation-reliant climate mitigation pathways, Earth Syst. Dynam., 15, 1055–1071, https://doi.org/10.5194/esd-15-1055-2024, 2024.

Please use such literature to introduce existing knowledge on the concern that is specific about forest expansion.

We appreciate the reviewer bringing our attention to relevant literature which we had not previously come across. These have been added to the introduction:

Fire adds an additional layer of complexity and must be considered when evaluating the suitability of forestation as a CDR strategy both globally and locally (Hermoso et al., 2021; Golub et al., 2022), particularly in the context of trends towards increasing fire weather (Jäger et al., 2024).

Line 117/118: Jaeger et al, 2024 raised and showed this.

We have added a citation to Jäger et al. (2024) here.

Line 147-150: In this context, please clarify that with this setup, one cannot consider atmospheric feedbacks to LULCC. As some of them can be important mitigators of fire (local BGP cooling in the Tropics, global BGC cooling) the reader needs to be informed about these key limitations here and in the discussion.

We have clarified this point:

As we use a land-only model configuration, there is no feedback from the land to the atmosphere; fully dynamic coupling of the fire model with atmospheric dynamics and chemistry was not available in CESM2 at the present time.

Line 165-167: Maybe add one sentence about how close this brings the 2015-25 evolution to observed environmental change.

We have added this line:

This represents a continuity scenario in which global trends do not change significantly from recent historical patterns (Riahi et al., 2017). We note that the actual evolution of global fire patterns is dataset-dependent (Figure 3) and the large contribution made by small-scale fires in the most recent estimates mean our 'present-day' model simulation of burned area and fire emissions is an underestimate of roughly 30% compared to GFED5 (Figure 3; van der Werf et al., 2025).

Line 177/178: this is wrong. A central goal of the PA is to limit global warming to "well below 2°C [...]". With most likely warming lying around 2°C, this is not achieved.

We appreciate this opportunity to correct this common misconception. We have amended this to:

The SSP1-2.6 scenario (hereafter 2C) represents a world in ~~which the goals of the2015 Paris Agreement are broadly achieved~~ rapid decarbonisation is achieved through global deployment of renewable energy

Line 180: add "°C" in appropriate distance from "4".

We have added the missing degree sign.

Line 183: I recommend to be precise here about a claim about the model world: "increases prescribed forest cover in the model"

We have amended this to:

This scenario increases forest cover in the model…

Line 187 "it": Reference unclear. The 'Max Forest' scenario?

We have amended this to:

The scenario represents…

Line 191/192: please clarify in your wording that only atmospheric impacts on land, not vice versa can and are assessed.

We have amended this to:

…to assess how fire behaviour under extensive forest expansion ~~interacts with~~ is affected by climate change.

Line 195/196: please add the reference for the land use change patterns. Is it Popp et al. (2017)?

Popp et al. (2017) is the general reference for land use change in SSP-RCP scenarios. However, the exact land use change patterns under a given SSP will differ depending on the model (and model version/configuration) being used, hence why we report the figures based on our own calculations rather than using calculations from Popp et al. (2017).

Line 209: This is hard to understand. If it's a projection, shouln't it be extrapolation?

We agree that this is too complicated and was trying to explain something that already has an entire paper dedicated to it – we have changed this to:

Population projections are derived by harmonising historical population data (Goldewijk et al., 2017) with SSP projections (KC and Lutz, 2017).

Figure 2 and from here on everywhere: The label says km^-2, but you probably meant km^2. But please express as grid cell areal share to avoid dependence on grid cell area and to allow for comparison between latitudes.

We are interested in total area as opposed to fractional area – burned area is a meaningful quantity and the total burned area in a given region is important both for evaluating impacts and for calculation of emissions.

Line 244: from this, can we deduct the inability of CLM5 to represent the effect of small-scale fires?

Yes – as we state in this paragraph, this is deducible both from the coarse spatial resolution of the model in this configuration and the explanation of why the observational estimates differ. The resolution of CLM5 can be varied and it would be interesting to see how increasing/decreasing the resolution of the global grid affects the global estimates, but this is beyond the scope of the present study.

Line 253/254: Do I as a reader get more information about this effect and the reasons behind somewhere else? Without more, it's unclear to which degree this is speculation or is grounded in understanding.

This is informed speculation based on what we considered the most likely explanation for a minor result based on the experimental setup. This is why we write that the model shift 'may' reflect the change in input datasets.

Line 255 "simulate": to avoid confusion with modeling data from freely running simulations, maybe use "report".

'Simulate' is clearly the wrong word here – we have replaced it with 'report' as suggested.

Line 263/264: please provide a statement whether this discrepancy is well explained or whether it is an obscure standing issue.

The latter statement is more accurate. We have added the following, though we note that detailed technical evaluation of the model is beyond the scope of the current study:

This discrepancy was also observed by Bhattarai et al. (2025), though the reasons for it are unclear.

Line 283: Reference to figure: which panels?

This should be figure 4A and has been corrected.

Figure 4: Overall, to not confuse more than report, this figure needs substantial improvement.

Line 286/287: The abrupt changes, especially from 2015 to 2050, suggest there areimportant evolutions inbetween. Given the fact that you simulated all the time steps, please provide the information in form of a continuous line plot. Given the close-to-linear-with- amplitude behavior of uncertainty of the decadal mean, it could be represented with shading in a year-to-year line plot (with e.g. running 10 year mean as filter) for e.g. 2 out of 6 scenarios. For interpretability, please additionally consider splitting the information in two panels, with one providing the baseline evolution (No LULCC) and another one providing the anomaly in the SSP1 and MF forestation scenarios. Furthermore, please consider using relative change as metric that allows to compare A,C with B,D appropriately.

Please improve also the visual coding ogf the line - label correspondance. The markers are hardly distinguishable, in continous line plots, consider combining color and dashing/dotting for clearer distinction.

Line 290/291:

The stippling is

- hardly visible in size

- indistinguishable from grid lines

- unintuitively dominant in regions of comparably small change

Please improve:

- the resolution of the figure (consider vector graphic)

- the illustration of significance

We appreciate these points about fig. 4. The figures in the MS file are quite small because of the requirement of the journal to submit figures for review integrated into the text of the MS. The full-size figures, which will be available in the final version, are high-resolution – Figure 4 is 4135 x 2009 at 600DPI. We have already increased the size of the figures at the request of the handling editor prior to this stage of review, but will make the stippling clearer by changing its colour so it has higher contrast and adjusting from dots to hatching. We will also add labels and a title to the latitudinal line plot. The significance test is standard, and there is no statistical reason why small changes cannot be significant because they are based on the absolute magnitude of the change. This is a consequence of the global scale of the plot. The regions where the changes are large also show statistical significance. In this plot we are aiming to show a broad global overview, in which case the spatial pattern of the change is of more interest than the significance. Following suggestions from the reviewer, we have strengthened the statistical power of this analysis by applying a false discovery rate check to the p values.

On the suggestion to show a continuous time series, we decided to prioritise clarity and comparability across scenarios. Using decadal means provide a clearer representation of the long term signal, while filtering out low-frequency variability. The current format in which the baseline and perturbation experiments are plotted on the same axes was chosen to allow direct comparison across scenarios.

Line 293-295: how does it evolve in between?

To clarify the temporal evolution, we have revised Figure 4 to improve the presentation of the time series (see response above). In the text, we focus on decadal changes to highlight the long-term signal, which is the primary objective of this study. This approach also avoids over-interpreting short-term variability associated with the anomaly forcing. The inclusion of error bars on the decadal means provides an indication of variability within each period. We have clarified this in the text:

Line 293:

Both 2C and 4C Max Forest scenarios show declines in burned area between 2015 and 2050, followed by increases towards the end of the century, with intermediate variability captured within the decadal uncertainty ranges.

Line 295-309: please provide the uncertainty you anyway calculate with it.

We have added the uncertainty bounds to the text here as requested:

2C Max Forest ends the century with a net decrease in burned area (-0.28 ±0.15 million km $^{-2}$ yr^{-1}; -6.3%), representing the lowest burned area of any scenario; 4C Max Forest results in a small but insignificant increase (+0.20 ± 0.19 million km $^{-2}$ yr$^{-1;}$ +4.5%) relative to 2015 levels. The 2C Forestation scenario (i.e. SSP1-2.6) has a similar effect on burned area as 4C Max Forest (+0.18 million ± 0.18 km $^{-2}$ yr $^{-1}$; +4.0%), while the 4C Deforestation (i.e. SSP3-7.0) scenario has an insignificant decrease (-0.04 ± 0.20 million km $^{-2}$ yr $^{-1}$; 0.9%). The mid-century decreases in the Max Forest scenarios, and the 2095 for 2C Max Forest, are statistically significant, that is, values are greater than 1 standard error from the 2015 mean. Fire emissions follow a broadly similar pattern. Most scenarios show decreases between 2015 and 2050, followed by increases from 2050 to 2095. The exception is 4C Deforestation, where emissions peak in 2050 and remain the highest through the end of the century. By 2095, all scenarios show lower fire emissions than in 2015. 2C Max Forest has the largest absolute reduction (-0.58 ± 0.09 PgC yr^{-1}; -24.3%).

Line 325 "spatial correlation": As far as I understand Fig. 5 you computed correlation across time for every grid cell. If this is true, write "spatial patterns of temporal correlation".

We have changed this to 'spatial patterns of temporal correlation' as requested.

Line 327 "highly similar": behave similarly.

We have changed this to 'highly similar' as requested.

Line 328: "correlations" is doubled.

We have removed the duplicated word.

Figure 5: Consider a correction for the false discovery rate with multiple testing at this level. E.g. Hochberg-Benjamini-correction (same for all maps).

We have applied the Benjamini-Yekutieli correction to figure 5, which can be considered as a non-parametric equivalent of the Benjamini-Hochberg correction, because the test is performed gridpoint-wise on geospatial data meaning that adjacent gridpoints are not strictly speaking independent samples. We have also changed the stippling to yellow hatching to better show the spatial pattern of significance.

The figure caption has been updated to reflect the application of the correction:

Figure 5 – Pearson correlation coefficients between changes in tree cover and burned area for the Max Forest scenario under 2C warming (2C Max Forest; 2026-2100). Hatching indicates grid cells where the correlation is statistically significant at 95% confidence level after applying the Benjamini-Yekutieli correction for the false discovery rate.

The text has been updated to reflect that some results are no longer significant at 95% confidence once this correction has been applied:

Significant negative correlations are found in tropical ecosystems correlations (r ~ -0.6; Figure 5), particularly in the Amazon, Congo, ~~Southeast Asia~~ Myanmar and Thailand, and northern Australia, indicating that the extensive forest expansion in these regions is associated with decreases in fire.

Positive correlations are found in temperate and subtropical zones (r ~ +0.6), including the Mediterranean, central Asia, central and western North America, parts of west and southern Africa, coastal regions of South America, and the east coast of Australia. In these regions, forest expansion tends to increase fire activity.

Line 336/337: Please try to avoid jargon and stick with model-explicit wordings, as what you report is model behavior, particularly modeling with one-way effects of atmos on land, without feedbacks.

We have amended this to:

In these regions, simulated forest expansion tends to increase fire activity in our model.

Line 341/342: consider computing this metric also for SSP1 and SSP3 LULCC. Then, you could provide also estimates for other regions than the ones you alter with MF.

We have extended the correlation analysis for the 2C Forestation and 4C Deforestation, again applying the new false discovery rate correction, and added these results as supplementary figures S8 and S9. We have updated the text to mention this briefly:

In the SSP land use scenarios (Figures S8/S9), similar results are obtained, with with generally positive correlations in the midlatitudes, and some regional differences across scenarios. In particular, forest cover is positively correlated with burned area in the Amazon under 4C Deforestation, potentially reflecting expected drying and increasing fire susceptibility under higher warming.

Line 342-345: shorten and reformulate to make this part of the sentence meaningful, otherwise it provokes the thought "of course unchanged regions do not undergo forestation" ...

We have revised the sentence to make it more concise while retaining clarification that unchanged forest regions are not included in the correlation analysis:

Note that these correlations only indicate areas where forest cover changes in the Max Forest scenario; ~~forest-dominated~~ regions with little or no change in forest cover ~~that remain unchanged, such as the~~ (eg central Amazon, ~~parts of Southeast Asia, and~~ boreal forests~~ regions in Russia, Canada, and Alaska~~), are therefore not represented ~~do not undergo forestation in this scenario~~ (King et al., 2024).

Line 353: “characterized …dynamics.” move up into "we use the GFED regions..."

We have changed this as requested:

We use the GFED region classification (Van Der Werf et al. 2010; Figure S5) to evaluate regions characterized by distinct forest types and climate-fire dynamics. Figure 6 shows projected changes in fire emissions through the 21st century in four GFED regions (Europe, North Africa/Middle East, the continental USA and sub-Saharan Africa).

Line 353-356: can you make this paragraph denser in information?

We have slightly revised this paragraph to more clearly highlight the relevance of these regions:

Figure 6 shows projected changes in fire emissions through the 21st century…, representing contrasting fire regimes and sensitivities to climate and land-use change ~~with differing fire responses to climate~~.

Figure 6: comments apply as in Fig 4. However, reading the full paper, it seems to me that no helpful information for the large findings is given here. Most important results are presented in the maps in Fig. 4 already. Consider omitting this figure entirely.

We agree that the key results are already captured in Figure 4 and that Figure 6 is not essential to the main narrative. However, we consider the regional analysis useful for comparison with widely used GFED regions. We have therefore moved Figure 6 to the Supplementary Material as figure S10 and removed some of the detailed discussion of these results:

In temperate forest biomes, (Figure S10A, S10C), fire emissions increase between 2015 and 2050 then diverge, with increases under 4C warming but decreases under 2C Forestation and No LULCC. Max Forest scenarios produce the highest emissions at both warming levels. Emissions in Europe double by 2100 under 4C Max Forest and 4C Deforestation, and nearly double in the USA under 4C Max Forest. In North Africa and the Middle East (Figure S10B), 2C Max Forest and 4C Max Forest have significantly higher fire emissions than the other land-use scenarios under the same warming levels by 2095. In sub-Saharan Africa, (Figure S10D), only 4C Deforestation shows an increase in fire emissions between 2015-2050, with significant declines in both Max Forest scenarios. This domain combines Northern and Southern Hemisphere Africa GFED regions and is dominated by tropical forest (i.e. broadleaf evergreen tropical tree PFTs), with the aggregated signal largely driven by the Congo basin despite regional differences.

~~In both Europe (Figure 6A) and the continental USA (Figure 6C), where temperate forest dominates (i.e. broadleaf deciduous temperate and needleleaf evergreen temperate tree PFTs), fire emissions follow the same trajectory. All scenarios show increases between 2015 and 2050, with little difference between 2C and 4C warming by mid-century. Under 2C warming, emission remain relatively stable from 2050 to 2095, while 4C scenarios continue to rise. Max Forest scenarios produce the highest fire emissions at both warming levels. By 2095, 2C Max Forest emissions increase by 24.1 TgC yr-1 (+70.6%) in Europe and 15.5 TgC yr-1 (+34.8%) in the USA compared to 2015. In Europe, both 4C Max Forest(53.9 TgC yr-1; +158.1%) and 4C Deforestation (82.4 TgC yr-1; +140.2%) result in more than doubling of fire carbon emissions by the end of the century.~~

~~In the USA, only the 4C Max Forest scenario (40 TgC yr-1; +90.3%) produces a comparable near-doubling in emissions. In North Africa and the Middle East (Figure 6B), forest expansion is strongly associated with increasing fire emissions. Both 2C Max Forest and 4C Max Forest produce significantly higher fire emissions than the other land-use scenarios under the same warming levels by 2095. The 2C Max Forest emissions in this region are comparable to 4C Deforestation. By 2095, 2C Max Forest emissions are 21.1 TgC yr-1 (+89.8%), while 4C Max Forest shows an increase of 48.1 TgC yr-1 (+204.2%), highlighting the strong fire sensitivity of this region to forestation.~~

~~A contrasting pattern is sub-Saharan Africa (Figure 6D). This domain combines Northern Hemisphere Africa and Southern Hemisphere Africa GFED regions and is dominated by tropical forest (i.e. broadleaf evergreen tropical tree PFTs). In this region, only 4C Deforestation shows an increase in fire emissions between 2015-2050 (354.21 TgC yr-1 ; +49%) associated with widespread deforestation in the mid-21 st century. However, emissions then decline toward 2095. In contrast, both Max Forest scenarios show the largest declines between 2015 and 2095: 2C Max Forest decreases by 222.91 TgC yr-1 (-30.8%) and 4C Max Forest by 111.51 TgC yr-1 (-15.4%), indicating the negative response of wildfire to tropical forest expansion across climate scenarios.~~

Line 366 “emission”: “fire emissions” (just to be clear about driving fossil fuel emissions and responding fire emissions)

This is indeed a typo, which we have corrected to ‘fire emissions’ as suggested.

Line 366 “relatively stable”: please try to be more precise in analysis (plotting see comments to Fig 4) and wording. "relatively stable" is vague for e.g. USA fire emissions under 2C, no LULCC dropping by roughly 60-80% of the 2015-2050 rise.

This has been addressed above.

Line 369: Seeing the complexity of this scenario labelling, is there something structurally in your type of analysis that reduces the validity of a global warming level (GWL) analysis? This would greatly help the accessibility, brevity of summary and interpretability. As fire seems to respond quite lag-free to global / regional warming, I see no need to strictly stick with time here and strongly suggest to complement and summarize your findings in global warming space.

We agree that a global warming level framework can improve comparability. We have added a brief note in the Model caveats and uncertainties section to clarify that our analysis is based on fully transient simulations, in which fire responses depend on both warming and time-evolving land use and vegetation.

Line 605:

Further work could explore these dynamics by… and applying constant climate forcings. A further limitation is that results are presented in a time-based framework rather than by global warming levels (GWL). While GWL approaches can improve comparability across studies and alignment with climate policy targets, their application here is limited by the transient evolution of land use and vegetation, which strongly influence fire activity alongside temperature.

Line 369-374: same as above, please provide uncertainty estimates.

Having moved figure 6 into the supplementary material, we no longer discuss these results in detail – see amended text above.

Line 378/379: how is this comparison relevant?

We have removed this sentence for clarity.

Line 383-385: is this combination helpful and meaningful? In CESM2 and CLM5 analyses of land use change impacts, often, contrasting signals could be found in West Africa vs. the tropical forest edge around the Kongo basin. This also seems to apply to your anlyses (Fig. 4 and 5). In this context, your plot depicts a residual of two strong opposing signals, which is quite hard to understand.

This is a fair point. We agree that combining these regions may mask contrasting signals. We have clarified this in the text and note that the aggregated response is dominated by the Congo basin, while regional differences are explored elsewhere in the manuscript – see our edited and more concise text above.

Line 396 "dynamic": it's not dynamic in the sense of "in interaction with climate" as far as I am concerned. Maybe choose "transient" for a clearer distinction between "changing in prescribed manner" and "responding in prognosed manner".

We have changed 'dynamic' to 'transient' here.

Line 397: is sustainability really the key aspect of the SSP1 population trajectory? As far as I understand, it's the overall label for SSP1 projections, but for population more precise terms will help to contextualize the meaning of it.

We thank the reviewer for this comment. We agree that "sustainability" is too broad in this context and have revised the wording to more clearly describe the SSP1 population trajectory as a lower-growth scenario driven by improvements in education and health:

a ~~dynamic~~ lower population growth scenario reflecting investments in education and health leading to a lower global population (SSP1Pop)

Figure 7: see Fig. 4 for comments

We address this in our response to the comments about fig. 4.

Line 406 "modest increase": given the calculated uncertainties shown in 7A, its insignificant. Please phrase adequately.

We have changed this to:

A modest, statistically insignificant increase

Line 409 "decreases with": revise for clarity

We have changed this to:

By contrast, the SSP3Pop scenario results in significant decreases, with…

Line 411 "follow a different trajectory": this wording is confusing to me. As the processes underlying BA and fire emissions are the same, we can read from this a different relative contribution of the population signal.

We have amended this to:

Fire emissions (Figure 7B) ~~follow a different trajectory, with~~ show a sharp decrease

Line 415: do you discuss this below? If yes, please refer me there. Otherwise: could this be due to more intense fires with carbon accumulating under human control`? Or is this represented differently in the model?

In the model, population density directly affects burned area through ignition and suppression, while fire emissions are calculated as a function of burned area and fuel load. Population therefore influences emissions only indirectly. We have clarified this as:

The differences in response between burned area and fire emissions arise because population density directly affects burned area in the model, whereas fire emissions are calculated as a function of burned area and fuel load, rather than being directly affected by population.

Line 418-460: please shorten this section significantly. As far as I am concerned, there are two learnings from the population experiments: 1) both fire metrics are anticorrelated with population density (probably this is quite directly implemented, hence starkly imprinted on the results) and 2) the relative population contribution to overall fire response under MF + Pop varies by region. The descriptions you provide however, are some details that for me do not seem to matter for what your paper contributes. So please consider to cut this down to key results and maybe aggregate your results like in Fig. 5 as correlation with population density.

We thank the reviewer for this suggestion. We agree that this section is detailed relative to the main conclusions. We have therefore shortened the description in the main text to focus on the key findings, and moved the more detailed regional analysis (line 418-460) to the Supplementary Material, as done for Section 3.2.

Population changes exert a strong influence on fire activity, with burned area generally decreasing with increasing population density, although the magnitude of this response varies regionally. A more detailed regional analysis is provided in the Supplementary Material (Figure S11 and discussion.)

Line 422 “However”: "Consistently" would make more sense.

We have removed ‘however’.

Figure 8: Like for Fig. 6, consider omitting this figure entirely. Otherwise, please argue for the added learning from this and improve it along the comments given for Fig. 4.

As noted above, we have moved this figure and the associated detailed regional analysis to the Supplementary Material.

Figure 9: this figure is much nicer than the previous ones: easier to read. However, please still fix the hardly visible stippling in the map. Also, add labels and a title to the latitudinal line plot.

We have added labels and a title to the latitudinal line plot. We acknowledge that the stippling is difficult to see at the current resolution and will ensure that this is improved in the final published version, as noted above.

Line 486: please be precise. In 2015, per definition, the land carbon sink was zero. So its +17% (add uncertainty!) on carbon stock, not sink.

We appreciate the reviewer pointing this out. We have changed this to:

the total land carbon ~~sink~~ reaches 516 PgC (+17.6% compared to 2015)

Line 487-489: please give an aggregated estimate of this difference with an uncertainty estimate across years / ensemble members and scenarios.

We thank the reviewer for this comment. The reported value (~60 PgC) represents the difference in total land carbon between simulations with and without fire and is similar across all scenarios considered. As our study does not include ensemble simulations, we cannot provide a formal uncertainty estimate.

Line 489-494: as detailed for the abstract, this is inadequate context. More valuable context is SSP1,2 or 3 cumulative emissions up to 2100. Or, the most informative context is the cumulated additional carbon sink from MF. In this context, two key questions you can answer with your data, are, A) by how much is the additional land carbon sink from afforestation reduced by fire and B) how much does the importance of fire change through forestation? So, ideally, I would like to see two values, where C stands for carbon stock: fraction answering question A = (C_MF - C_MF_noFire) /

(C_MF - C_noLULCC) fraction answering question B = [(C_MF - C_MF_noFire) - (C_noLULCC - C_noLULCC_noFire)] / (C_MF - C_noLULCC)

This is a very helpful suggestion. As noted above, we have revised the manuscript to avoid direct comparisons with present-day anthropogenic emissions. We agree that the proposed metrics provide a more meaningful context and have therefore included these additional analyses, as follows, based on the carbon stocks at 2100 plotted in fig. 9:

Fraction A = -0.43 (2C), -0.40 (4C)

Fraction B = -0.03 (2C), -0.04 (4C)

We have added some text describing this here, beginning at line 494:

We further show how much the additional land carbon storage due to forest expansion is reduced by fire, following Equation 1:

$$\frac{C_{MF}-C_{MF\_NOFIRE}}{C_{MF}-C_{NOLULCC}} \quad (1)$$

Where C_{MF}, C_{MF_NOFIRE}, and $C_{NOLULCC}$ represent the land carbon storage at the end of the 21st century in their respective experiments. The fraction is -0.43 for 2C of warming and -0.40 for 4C of warming, meaning that fire reduces the potential carbon sequestration of Max Forest by ~40% under both warming scenarios. In addition, we further demonstrate that the importance of fire is constant across LULCC scenarios following Equation 2:

$$\frac{(C_{MF}-C_{MF\_NOFIRE})-(C_{NOLULCC}-C_{NOLULCC\_NOFIRE})}{C_{MF}-C_{NOLULCC}} \quad (2)$$

This fraction is -0.03 (2C) and -0.04(4C), meaning that the effect of fire on land carbon storage by 2100 is largely independent of the land use change scenario.

This has also been added to the abstract as suggested in the major comments:

Finally, fires reduce the additional carbon storage from forest expansion by 40% by 2100, and the magnitude of the land carbon sink by ~60 PgC…

Line 500-502: see above and the general comment in the abstract. There is a manifold of more meaningful contextualizations. This one is a dangerous framing at utility to those interested in the delay of emission reduction.

We thank the reviewer for this comment. As noted above, we have revised the text to avoid implying direct comparability with present-day anthropogenic emissions and clarified that this comparison is not climatically equivalent. In addition, following the reviewer's suggestion above, we now report alternative metrics that provide a more meaningful context for interpreting the impact of fire on the land carbon stock.

Figure 10: the information in this figure is almost entirely already covered in Fig 9 and *is much harder to grasp in this one. Please consider alternatively to plot above-mentioned fractions A and B regionally or--even better--as a map.*

We agree that this figure is not essential to the main narrative and that its information is largely captured in Figure 9. We have therefore moved Figure 10 to the Supplementary Material as Figure S11.

Line 522: please rephrase. What you find is actually a scenario-dependence for parts of the impacts.

We have rephrased this as requested to:

Our experiments show that forest expansion impacts burned area and fire carbon emissions in varying ways depending on the region and climate warming scenario.

Line 542-547: do you trust your results to the same degree in West Africa for example?

This is a useful question. We are less confident in these because of the additional limitations of uncertain precipitation projections for sub-Saharan Africa, particularly for extreme events. West Africa is projected to become both hotter and wetter in future, but the rainfall projections are dependent on model simulation of processes such as the West African Monsoon and the African Easterly Jet, which are not well resolved in CMIP6 models (Almazroui et al., 2020). This uncertainty is mentioned in line 572.

Line 549-551: that is not true for fire emissions, where Fig. 7 shows its clearly the smallest contributor...

We have corrected this so that it refers specifically to burned area:

On a global scale, the magnitude of the burned area response to population scenarios is similar to that from warming and LULC change (Figure 7).

Line 556-557: From this discussion I get the impression that all your results on fire response to population could be obtained without actually running the model. If this is the case, what can we actually learn additionally from these simulations? Isn't it about the balance of the drivers? I strongly suggest to discuss solely this and to avoid *pretending the response of fire to population density trends is a result. It is the assumption you put in the model.*

This is a good point. We agree that the relationship between population density and fire activity is partly prescribed in the model formulation. We have revised the discussion to clarify this point and to emphasise that the simulations provide insight into the magnitude and regional variability of this effect, and its interaction with climate and land-use change:

Our results also quantify the influence of population change ~~as a key driver of future~~ on future fire activity within the model framework (Zhang et al., 2025; Veira et al., 2016)~~. On a global scale, the magnitude of the fire response to population scenarios is similar to that from warming and LULC change (Figure 7). In sub-Saharan Africa, population growth under SSP3 results in a strong reduction in burned area, primarily through increased fire suppression associated with rising population density (Figure 8). A similar but smaller suppression signal occurs in SSP1. These scenarios contrast with Europe and East Asia, where population decline is associated with increases in fire activity, as has been observed in recent decades (de Diego et al., 2023).~~ In the model, ~~assumes that~~ fire suppression scales with population density (Li et al., 2012). Within this framework, the simulations quantify how this relationship influences fire activity across regions and its interactions with climate and land-use change.

Line 567-569: this is hard to understand. Does this finding come from the comparison of MF with SSP3? In noLULCC, there should be no deforestation fire, should there? Or is this again the trivial finding that forest expansion is not deforestation?

We have clarified the wording of this statement in our response to reviewer 2 below. It might be expected that fires increase in a hotter world with more fuel carbon available due to forest expansion, but we suggest here that stopping deforestation fires outweighs any effects due to increased fuel load or changes to soil moisture.

Line 576/577: by how much? I suggest quantifying with fractions A and B as introduced above.

This has already been quantified in the results section.

Line 580: please also or alternatively present a relative quantity.

We have added a percentage here.

Line 585 Model caveats and uncertainties:

1) Please also discuss how your findings could be affected and altered if you included the feedbacks through the atmosphere: global-scale cooling from carbon uptake under MF and spatially diverse biogeophysical feedbacks.

We have added the following in the second paragraph. We note that the former effect mentioned here, which would require a fully emissions-driven Earth System model, is mostly beyond current modelling capabilities:

…as recommended by Li et al (2024), to improve the reliability of their projections. In addition, our land-only model configuration does not include feedbacks from land to the atmosphere. As a result, potential effects such as large-scale cooling from carbon uptake under forest expansion, as well as regional biogeophysical feedbacks (e.g. changes in albedo or evapotranspiration), are not represented. These processes could influence fire activity and carbon dynamics and should be considered in future fully coupled simulations.

2) If you decide to compute fractions A and B, consider discussing how carbon prices might change based on the additional area needed to sequester a ton of carbon under fire in total (A) and fire changes from forestation (B).

We have added a mention of this in the discussion section (line 583).

This also has implications for the value of forest-based carbon credits, since we show that a) fire could reduce the amount of additional carbon storage available from forest expansion by 40% by 2100, b) the effect of fire on carbon storage is independent of warming or LULCC scenarios; consequently, the area required to sequester an additional tonne of carbon will be significantly larger if fire effects are taken into account on the multidecadal timescales required to grow trees to maturity.

3) Given this discussion section has "uncertainties" in the title, consider discussing the large global and regional mean uncertainties you quantify in the Figures. Oftentimes you report detailed numbers when the uncertainty bars of experiment and control simulation overlap. This would be the place to summarize the meaning of uncertainties from different sources for different findings.

We thank the reviewer for this suggestion. This point is addressed in the final paragraph of the Model caveats and uncertainties section, where we discuss how variability across time and scenarios affects the interpretation of the results, including cases where differences between experiments are comparable to this variability

Line 590-594: please think one step further and summarize for the reader the consequences you expect a use of all the FireMIP models would have for your results and findings.

We have added the following:

Model intercomparison studies, such as FireMIP, are thus vital for better understanding the range of potential differences in fire models and assumptions. Extending this study across multiple models would enable determination of whether the main qualitative findings identified here are robust across different model structures or sensitive to how key processes such as fuel load, moisture limitation, ignition, and suppression are represented.

Line 596-598: If you follow my suggestion to analyse your results on warming levels, you can here argue that this (to a large degree) eliminates climate sensitivity as uncertainty in your analysis.

We have added this as an additional suggestion, as listed in our response to a previous comment.

Line 619/620: well put. To argue more in favour of your approach, you could highlight that this makes your approach produce a conservative estimate that already is significant.

We have edited this sentence to highlight this:

Our scenario, though showing significant fire increases in these regions, thus likely underestimates fire risk associated with plantation-style forest expansion.

Line 625/626: again, this depends on the counterfactual, not on the forestation...

We have amended this to:

In tropical regions, applying an afforestation/reforestation scenario tends to reduce fire activity locally…

Line 630/631: rephrase to clarify that only in some regions the signal is of comparable size

This has been amended to:

In some regions, population changes can exert a ~~regionally~~ comparable influence on future fire activity to that of climate and land use.

Line 631-633: in each region, highlight the relative contribution, not the well-known gross effect.

This has been revised as suggested:

For example, in sub-Saharan Africa, population density growth is a dominant driver of decreased burned area (via fire suppression), whereas in Europe, population declines contribute to increasing fire activity alongside climate and land-use effects.

For example, ~~population density growth~~ in sub-Saharan

Africa, population density growth is a dominant driver of decreased burned area (via fire suppression), whereas in Europe, population declines contribute to increasing fire activity alongside climate and land use effects. ~~enhances fire suppression and decreases burned area, whereas population declines in Europe may contribute to increasing fire activity.~~

Line 641-645: this has been argued for previously (e.g. Hermoso et al., 2021 and Jaeger et al., 2024, see comment in line 100). Cite these references and add numbers where you believe your work provides robust quantification (e.g. fractions A and B)

We have added citations to these references:

Fire also significantly decreases the land carbon sink by the end of the 21 st century, as much as ~60 PgC across all scenarios, underscoring the need to include fire dynamics in global and national climate assessments (Hermoso et al., 2021; Jäger et al., 2024). Failing to account for future fires could lead to overestimates of land-based carbon sequestration, especially in countries relying on forest sinks in their NDCs, given that fire reduces the size of the additional carbon sink due to forest expansion by 40%.

<u>Reviewer 2</u>

<u>Major Comments</u>

The study "Global and Regional Impacts of Forest Expansion on Future Wildfires" presents a comprehensive modelling study. I appreciate the systematic investigation *of drivers, including climate, land use, and population effects, as well as the model validation against GFED. Assessing the potential and trade-offs of afforestation, reforestation, and forest expansion with regard to fire is highly relevant. However, in its current form, the manuscript lacks sufficient ecological interpretation of the results and a more critical discussion of the assumptions and implications.*

We thank the reviewer for their positive assessment of our study and appreciate the feedback on the ecological implications of the work, as well as the opportunity to clarify assumptions and implications.

My main concern is that throughout the manuscript, fire is treated as an undesirable outcome per se, while increases in biomass are implicitly treated as beneficial. This framing is ecologically too simplistic. Fire is a natural and often necessary process in many ecosystems, and in some systems it is essential for maintaining biodiversity, ecosystem structure, and function. Likewise, the fire proneness of biodiverse, functioning forest ecosystems differs critically from that of fast-growing tree cover in plantation-style or low-diversity systems. The manuscript does not discuss this distinction sufficiently, and it remains unclear how the model can or cannot represent it.

This is an important point. We agree that fire is a natural and often essential ecological process in many ecosystems, and that its role cannot be interpreted solely as negative. Our intention is to assess how changes in fire activity may affect the effectiveness and risks of forest expansion as a climate mitigation strategy.

We also agree that differences in ecosystem type and forest structure (e.g. natural vs. plantation systems) are important for fire behaviour and impacts, and that these distinctions are not fully resolved in the model framework (as acknowledged in the section on model caveats, which has been expanded following suggestions from reviewer 1).

We have revised the Introduction to more clearly distinguish between fire as an ecological process and fire as a risk factor in the context of forest expansion, and to better acknowledge these limitations.

Paragraph starting at line 62:

Fire is an important component of the Earth system. It is one of the most significant controls on the carbon cycle, affecting terrestrial and atmospheric carbon storage, and undergoing complex feedback responses with weather, climate, water, and vegetation (Li et al., 2018). Fire is also a fundamental ecological and evolutionary driver that has operated since the early history of terrestrial vegetation, selecting for fire-adaptive traits and helping to shape biodiversity patterns and the distribution of major biomes over deep time (He et al., 2019; Keeley and Pausas, 2022). Fire is also of great importance to human society, with impacts including health implications of poor air quality (Val Martin et al., 2015; Silver et al., 2024; Tang et al., 2025; Shi et al., 2025), loss and damage to buildings and infrastructure (Kim et al., 2023), and exacerbation of anthropogenic climate change due to release of greenhouse gases and loss of carbon sinks (Nolan et al., 2021; Allen et al., 2024; Park et al., 2023; Liang et al., 2025). Understanding the drivers and impacts of fire is therefore crucial both environmentally and socioeconomically (Haas et al., 2022).

Related to this, I would wish for a more detailed description of vegetation and fire processes and feedback mechanisms. The fire module description is relatively brief, and I was left with the impression that a strong fuel-driven response may be built into the model formulation, shaping the conclusions. I was also unsure about the temporal design of the simulations. The manuscript uses forcing data and scenario inputs across different time periods, selected time slices or decadal means. It is not always clear if and how transient dynamics are handled. For instance, whether burning opens space for regrowth and how post-fire vegetation dynamics or succession are represented (or constrained) in the model setup.

These are very good points. We agree that further clarification of model processes and experimental design is useful. We have therefore expanded the description of the fire module to better explain the role of fuel availability and its interaction with climate and human drivers.

We have also clarified the temporal design of the simulations, noting that all experiments are fully transient with time-varying climate and land-use forcing, while time-slices and decadal means are used only for presentation.

Finally, we have added clarification on post-fire dynamics, noting that vegetation and carbon pools are updated following fire within the model framework, but that detailed ecological processes such as succession are not explicitly represented.

Line 138:

The fire module simulates burned area as a function of weather and climate conditions (e.g. lightning frequency and wind), vegetation conditions (e.g. root-zone soil moisture, and plant functional type (PFT) properties~~), and anthropogenic ignition and suppression. Human influence is parameterised as functions of population density and gross domestic product~~ fuel availability (ie, fuel C), and anthropogenic ignition and suppression. ~~Following the burned area calculation, [...] the fire module calculates changes in land C and N pools due to fire, including speciated fire C emissions, and updates these pools in land ecosystems in the wider model.~~ Following fire, vegetation and carbon pools are updated within the model framework; however, detailed ecological processes such as succession and species turnover are not explicitly represented.

Line 165:

This represents a continuity scenario [..]. We then initialised our experimental runs from the end point of this SSP2-4.5 run. All experiments are fully transient, with time-varying climate and land-use forcing, and time-slices and decadal means are used only to summarise and present the results.

Finally, some ecological consequences of the Max Forest scenario appear potentially problematic. In particular, the manuscript describes conversion of C4 grass- systems such as the Brazilian Cerrado and southern African savannas into tropical forest, with associated fire reductions. While this may emerge from the prescribed scenario and model assumptions, these transitions could threaten fire-adapted biomes and set an expectation that a successful nature-based solution would be foresting up savannas. I strongly encourage the authors to frame and discuss the results with a more critical view.

We agree with the reviewer and appreciate this comment as it shows that our concerns on this point have not been expressed with sufficient clarity. Afforestation of fire-adapted ecosystems such as savannas can have negative ecological consequences, including impacts on biodiversity, ecosystem function, and potentially regional climate.

In our simulations, the reduction in fire activity in these regions reflects the prescribed land-use changes rather than an ecologically desirable outcome. We have revised the Discussion to emphasise that these transitions represent substantial alteration of fire-adapted systems and that our results should not be interpreted as supporting forest expansion in such ecosystems. We also note that, despite these concerns, some current forest restoration initiatives may promote similar transitions.

Paragraph starting line 523:

In tropical latitudes, especially around the margins of tropical rainforests, forest expansion is associated with decreasing fire activity (Figures 4 and 5). In the Max Forest scenario, tropical forest expansion comes at the expense of C4 grasslands, such as those in the Brazilian Cerrado and southern African savannas, which are among the world's most fire-prone ecosystems (Bond et al., 2019), and also support highly diverse open-habitat biotas that depend on the maintenance of open-canopy conditions (Bond and Parr, 2010; Lehmann and Parr, 2016). These reductions in grassland area directly suppress burned area because forests burn at a lower frequency than grasslands; the tropical tree PFTs have lower flammability than the grasses they are displacing (Li et al., 2018). These reductions should not be interpreted as ecologically desirable, particularly in fire-adapted systems such as savannas, where afforestation may disrupt ecosystem structure and function (Abreu et al., 2017). A decline in fire activity early in the simulation is also observed (Figure 4), especially in the tropics (Figure S6), which reflects the removal of deforestation fires, previously a dominant fire source. Since No LULCC and Max Forest scenarios eliminate future deforestation, these fire types are reduced almost to zero. SSP1, though generally a forest expansion scenario, does still include some deforestation (Loughran et al., 2023). The implication of this finding is that halting and reversing deforestation, which was a key commitment made by parties to the United Nations Framework Convention on Climate Change at COP26 in 2021 (Wang et al.,2022), could substantially decrease tropical fire activity.

Paragraph starting line 623

Our findings highlight that forest expansion interacts in complex ways with climate and population to affect both global and regional fire regimes. In tropical regions, afforestation/reforestation tend to reduce fire activity locally, both by removing the deforestation fire signal, and by replacing highly flammable and fire-adapted grasslands with less flammable tropical tree cover. In contrast, forest expansion in temperate regions (e.g. the Mediterranean and continental US) tends to increase fire activity, due to increased fuel availability, drier conditions, and decreased fire suppression.

Minor Comments

Line 231 to 232: please check, is it 2000-2014?

The 2000–2016 period is correct. It combines the historical simulation (2000–2014) with the SSP2-4.5 extension (2015–2016), which we use to provide a continuous present-day baseline.

Figure 2: consider plotting this figure on a log scale

We thank the reviewer for this suggestion. We explored the use of a log scale, but as the values shown in Figure 2 fall within a relatively narrow range, this did not improve the clarity or interpretability of the figure. We therefore retain the linear scale.

Line 327 to 328: please add figure for 4c max forest to the appendix

We thank the reviewer for this suggestion. We have added the 4C Max Forest results to the Supplementary Material (Figure S7) and updated the text accordingly. We note that this figure has been revised to improve clarity as part of a Reviewer 1's comment.

Results for 4C Max Forest (Figure S7) and for fire emissions…

Figure 6: icons hard to differentiate

We agree that the figure was difficult to interpret. Figure 6 and its discussion has now been moved to the Supplementary Material, and we have revised it to improve the clarity and distinguishability of the symbols.

Line 456 to 457: double punctuation

The duplicated full stop has been removed.

Line 523 to 526: this needs to be discussed more comprehensively considering its ecological consequences and realism, see comment above

We have addressed this in the response to the major comment above.

Line 544 to 547: I agree, but this discussion is very one-sided. If forest expansion is done in a plantation, low-biodiversity way, the risk of maladaptation is quite high. However, there are plenty of initiatives (in particular in the tropics) where forest restoration is turning fragmented and degraded forests into biodiverse ecosystems, which in turn show better functioning and resilience to disturbances like droughts and, in consequence, fire.

We thank the reviewer for this important comment. We agree that the risks associated with forest expansion depend strongly on how it is implemented. We have revised the text to clarify this point.

Our results suggest that forest expansion as a mitigation strategy could be maladaptative in this region, especially where it leads to low-diversity or plantation systems, as it may intensify wildfire impacts, damage infrastructure, degrade air quality and reduce the carbon storage potential of forests (Turco et al., 2018). In contrast, restoration of biodiverse and well-functioning forest ecosystems may enhance resilience to disturbances such as drought and fire.

Line 565 to 569: please rephrase for clarity

We have rephrased this sentence as follows:

Fuel biomass and soil moisture are important determinants of fire behaviour (Turco et al., 2018). However, in our simulations, the increased biomass and decreased soil moisture resulting from tropical afforestation in the Max Forest scenario (King et al., 2024) did not lead to increased fires locally because the signal was instead dominated by halting and reversing deforestation.

569 to 573: Bhattarai et al. (2025) seems very speculative. Is there another source or observation that supports this?

This is a great suggestion. We have strengthened this part of the discussion by adding observational evidence supporting the relationship between precipitation and fire while retaining the uncertainty associated with future precipitation projections.

Bhattarai et al. (2025) found decreases in tropical fire activity with constant land use under 2C and 4C warming scenarios, with stronger decreases under 4C, attributing this to complex non-linear relationships between fire and precipitation. Observational studies also support this behaviour, showing a unimodal relationship between rainfall and burned area, with increases up to ~1100mm yr^{-1} followed by declines due to fuel saturation (van der Werf et al., 2008), a relationship simulated by our model (Li et al. 2024). However, we note that projections of future tropical precipitation, particularly in Africa, are highly uncertain (Taguela et al., 2025).

On behalf of the authors,

James A. King

Additional references

Abreu, R. C. R., Hoffmann, W. A., Vasconcelos, H. L., Pilon, N. A., Rossatto, D. R., and Durigan, G.: The biodiversity cost of carbon sequestration in tropical savanna, Sci. Adv., 3, https://doi.org/10.1126/SCIADV.1701284/SUPPL_FILE/1701284_SM.PDF, 2017.

Almazroui, M., Saeed, F., Saeed, S., Nazrul Islam, M., Ismail, M., Klutse, N. A. B., and Siddiqui, M. H.: Projected Change in Temperature and Precipitation Over Africa from CMIP6, Earth Syst. Environ., 4, 455–475, https://doi.org/10.1007/S41748-020-00161-X/FIGURES/14, 2020.

Bond, W. J. and Parr, C. L.: Beyond the forest edge: Ecology, diversity and conservation of the grassy biomes, Biol. Conserv., 143, 2395–2404, https://doi.org/10.1016/J.BIOCON.2009.12.012, 2010.

Golub, A., Sohngen, B., Cai, Y., Kim, J., and Hertel, T.: Costs of forest carbon sequestration in the presence of climate change impacts, Environ. Res. Lett., 17, 104011, https://doi.org/10.1088/1748-9326/ac8ec5, 2022.

He, T., Lamont, B. B., and Pausas, J. G.: Fire as a key driver of Earth's biodiversity, Biol. Rev., 94, 1983–2010, https://doi.org/10.1111/BRV.12544, 2019.

Hermoso, V., Regos, A., Morán-Ordóñez, A., Duane, A., and Brotons, L.: Tree planting: A double-edged sword to fight climate change in an era of megafires, Glob.

Chang. Biol., 27, 3001–3003, https://doi.org/https://doi.org/10.1111/gcb.15625, 2021.

Jäger, F., Schwaab, J., Quilcaille, Y., Windisch, M., Doelman, J., Frank, S., Gusti, M., Havlik, P., Humpenöder, F., Lessa Derci Augustynczik, A., Müller, C., Narayan, K. B., Padrón, R. S., Popp, A., van Vuuren, D., Wögerer, M., and Seneviratne, S. I.: Fire weather compromises forestation-reliant climate mitigation pathways, Earth Syst. Dyn., 15, 1055–1071, https://doi.org/10.5194/esd-15-1055-2024, 2024.

KC, S. and Lutz, W.: The human core of the shared socioeconomic pathways: Population scenarios by age, sex and level of education for all countries to 2100, Glob. Environ. Chang., 42, 181–192, https://doi.org/https://doi.org/10.1016/j.gloenvcha.2014.06.004, 2017.

Keeley, J. E. and Pausas, J. G.: Evolutionary Ecology of Fire, Annu. Rev. Ecol. Evol. Syst., 53, 203–225, https://doi.org/10.1146/ANNUREV-ECOLSYS-102320-095612/CITE/REFWORKS, 2022.

Lehmann, C. E. R. and Parr, C. L.: Tropical grassy biomes: Linking ecology, human use and conservation, Philos. Trans. R. Soc. B Biol. Sci., 371, https://doi.org/10.1098/RSTB.2016.0329/23013, 2016.

Li, F., Song, X., Harrison, S. P., Marlon, J. R., Lin, Z., Leung, L. R., Schwinger, J., Marécal, V., Wang, S., Ward, D. S., Dong, X., Lee, H., Nieradzik, L., Rabin, S. S., and Séférian, R.: Evaluation of global fire simulations in CMIP6 Earth system models, Geosci. Model Dev., 17, 8751–8771, https://doi.org/10.5194/GMD-17-8751-2024, 2024.

Ou, Y., Roney, C., Alsalam, J., Calvin, K., Creason, J., Edmonds, J., Fawcett, A. A., Kyle, P., Narayan, K., O'Rourke, P., Patel, P., Ragnauth, S., Smith, S. J., and McJeon, H.: Deep mitigation of CO2 and non-CO2 greenhouse gases toward 1.5 °C and 2 °C futures, Nat. Commun., 12, 6245, https://doi.org/10.1038/s41467-021-26509-z, 2021.

van der Werf, G. R., Randerson, J. T., Giglio, L., Gobron, N., and Dolman, A. J.: Climate controls on the variability of fires in the tropics and subtropics, Global Biogeochem. Cycles, 22, https://doi.org/https://doi.org/10.1029/2007GB003122, 2008.

van der Werf, G. R., Randerson, J. T., van Wees, D., Chen, Y., Giglio, L., Hall, J., Vernooij, R., Mu, M., Binte Shahid, S., Barsanti, K. C., Yokelson, R., and Morton, D. C.: Landscape fire emissions from the 5th version of the Global Fire Emissions Database (GFED5), Sci. Data, 12, 1870, https://doi.org/10.1038/s41597-025-06127-w, 2025.