

Interactive comment on “Tropical climate-vegetation-fire relationships: multivariate evaluation of the land surface model JSBACH” by Gitta Lasslop et al.

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We thank the reviewers for their detailed and constructive comments, which strongly helped to improve the manuscript. We included additional analysis on the correlation between burned fraction and tree cover and on the increase of maximum tree cover with increasing precipitation. We hope our replies to the reviewer comments and the modifications will make the manuscript suitable for publication in biogeosciences. Our responses are inserted below the reviewer comments in italics.

C1

1 Review 1

General Comments:

Reviewer summary: The manuscript presents results from multi-variate comparisons between a simple fire model and complex fire model within JSBACH against those of remote sensing datasets for tree cover, grass cover, and burned fraction for regions within the tropics. The work finds that the resolution of the remote sensing datasets is important for setting precipitation limits on tree cover and burned fraction classifications. The fire models capture broad spatial patterns, but overall the complex fire model has improved performance. The analysis was completed for continental sub-sets and with and without preindustrial land use. Given the results the authors suggest C1 improving the drought response of vegetation, including more complex bark thickness for trees, and a representation of size-structure. The multi-variate analysis used here better identifies model-data mismatches to model processes.

Article contribution and overall impact: This study highlights the challenges of simulation of vegetation-fire interactions across the tropics. Strong climate vegetation relationships and a closely interacting fire regime make the vegetation state of this region difficult to simulate. The manuscript does a good job of presenting the challenges of capturing vegetation and fire in the tropics with simulation and with remote sensing datasets. The discussion would benefit from a more detailed description of the connections between recommended improvements and deficiencies of the simulations, as well as inclusion of more references. Please update the discussion to include a reference back to the figure or table being discussed (some of these are highlighted in detail comments). Specifically, more detailed discussion of size-structure and its importance as a mechanism for tree survival in fire prone regions should be included. A key component of the mortality of woody vegetation to fire is its size at the time of fire and the ability to accumulate size between fires. This is central to the work of many of W. Hoffman's papers in the region (Hoffman et al 2003, Hoffman et al 2009, Hoffman et al 2012). This type of work should be referenced as well as important differences

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between the continents in terms of vegetation survival from fire.

1) *We thank the reviewer for identifying this lack of details. We extended the discussion to improve the connection between improvements and deficiencies of simulated patterns including the recommended inclusion of more references to literature but also to the figures and table in the results section.*

Detailed comments:

Page 6 line 15: Are burned area and burned fraction the same?

2) *Burned area refers to the area, burned fraction to the fraction of the grid cell. It therefore is the same parameter but with a different unit. We change the burned area to burned fraction here, as the burned fraction is displayed in the figure to avoid confusion.*

Page 7 line 12: “stronger relationship between low tree cover and high fire occurrence than observations” Explain this in more detail. By what measure and for which figure/table?

3) *We modified the paragraph to explain it in more detail and refer to figure 4 and table 1. We also performed an additional analysis and include the correlation between burned fraction and tree cover in table 1 to quantify this strength of the relationship. “Models and observations generally agree on the absence of fire for very high tree cover (>0.8) and on the decrease of burned fraction for mean annual precipitation decreasing below 1000 mm. However for regions with tree cover < 0.8 and mean annual precipitation > 1000 mm we find strong differences. JSBACH-SPITFIRE shows a strong negative Spearman rank correlation between burned fraction and tree cover, the observations show a weak negative correlation, and JSBACH-standard shows a positive correlation (Table 1). This can also be seen in Figure 4 where*

C3

for the JSBACH-SPITFIRE simulation the highest burned fractions (> 50% of grid cells year⁻¹) are found in Africa for the lowest tree covers (0.1) and for precipitation between 1000-2000 mm year⁻¹. JSBACH-standard in many grid cells shows low fire occurrence for low tree cover, especially for South America (Figure 4), these grid cells have a high fraction of crops or pasture, which both are excluded from burning in JSBACH-standard (in SPITFIRE only crops are excluded). The observations (also Figure 4) show highest values of the burned fraction for tree cover values up to 0.3 for MODIS and up to 0.5 for LANDSAT.”

Page 9 line 6: Why use the preindustrial land use? The observation datasets are for the period of 1996-2005.

4) *The preindustrial state is a state with low influence of land use, the comparison with the historical simulation therefore indicates the effect land use has on the climate-vegetation-fire relationships. We add two sentences in the beginning of the paragraph to explain the purpose:*

“The simulation with preindustrial land use represents a state with low influence of land use change. The comparison to the historical simulation allows to assess the influence of land use change since 1850.”

We also include a description of the changes in statistical parameters listed in table 1: “The impact of fire on tree cover as quantified by the Spearman rank correlation between burned fraction and tree cover is higher for the simulation with preindustrial land use (Table 1). Land use change did not affect the rank correlation between precipitation and temperature. The precipitation range for 80% of the burned area is only slightly narrower for the simulation including land use change (Table 1). Tree cover, however, is even higher for low precipitation and reaches canopy closure for lower precipitation (Table 1 and Figure 7 compared to Figure 4).”

Page 10 line 4: Update “We here discuss. . .” to “Here we discuss. . .”

C4

5) Updated in the revised manuscript.

Page 10 line 7: Clarify that improvements in the SPITFIRE version cannot improve this mismatch. The standard version does not capture the observations as shown in figure 3.

6) We changed this part (which refers to the mismatch of tree cover in low precipitation areas) to:

“In these dry regions no or only very low burned fractions are observed, and SPITFIRE shows a good response to precipitation while JSBACH-standard already overestimates the burned area (Figure 3). The improved burned area pattern of SPITFIRE did not lead to an improvement in tree cover for these dry regions. It is therefore unlikely that further improvements in burned fraction will improve this model-data mismatch, satellite data however indicate that the intensity of fires increases in these regions and might help to explain the disappearance of trees (Hantson et al., 2017). The mechanisms however are not sufficiently understood to be included in a model.”

Page 11 line 3: update “too high tree cover” to “excessive tree cover”,
and

Page 11 line 5: update “too high dominance” to “excessive dominance”

7) Done as suggested, we also change “too high” to “excessive” in the abstract.

Page 11 line 6-7: Explain how saplings being inferior to grasses would improve the representation of tree-grass competition? How would these saplings alter the resulting tree cover in areas where grasses exist? Are there processes in the model that would need to be added to include grass suppression of saplings?

C5

8) Whether or not the inclusion of saplings would improve the representation of tree cover is certainly a matter of the exact implementation and model tuning. Answers to these detailed questions would therefore be highly speculative. We are therefore careful with our statements and add:

“Including this mechanism could improve the balance between tree and grass cover, but it could also reduce the establishment rate of trees and, therefore, the tree cover in the dry regions with excessive tree cover. Including a PFT-specific rooting depth of vegetation would be an important extension of the model to improve the competition for water between grasses, saplings and adult trees.” In the end of the paragraph on model improvements we also add: “How exactly these plausible modifications would change the patterns of tree cover, fire and their relation to climate likely strongly depends on the exact parameterization and needs to be tested with stepwise model development and factorial simulations.” To make sure readers understand that these are only suggestions and that they first need to be tested to understand how exactly they change the simulation outcome.

Page 11 line 8-9: Include the figure that this relationship is referring to “higher burned fraction and lower tree cover for open canopies, however it is not found in the observations.” Is this for figure 4? Also specify for what regions, as they are not consistent.

9) We updated the sentence with references to figures:

The absence of fire for closed canopies is captured well by JSBACH-SPITFIRE, the modelled strong relationship between higher burned fraction and lower tree cover for open canopies (Figure 4, with the exception of Australia, Table 1), however, is not found in the observations (Figure 2,4). See also reply 3.

C6

Page 12 line 1: Explain how increased bark thickness would be implemented in the model. Include discussion of the relationship between bark thickness and size-structure of trees, and species or regional variability in bark thickness characteristics, and how this might be accounted for in the model.

10) *There are several ways to increase bark thickness, the first would be to modify the PFT specific bark thickness which depends on the tree biomass. Bark thickness could also increase according to previously burned area, assuming tree invest more in bark in regions with high fire occurrence. We modified the existing paragraph to:*

“Bark thickness is a key property of trees for the fire-related mortality. In JSBACH-SPITFIRE bark thickness is PFT specific and depends on the biomass. The adaptation of trees to frequent fires by increased bark thickness, and therefore higher resistance of trees to fire (Pellegrini et al., 2017) would increase the tree cover in regions with high burned fraction. This could be implemented in the model with more specific PFTs or by modifying the bark thickness according to the fire regime. Kelley and Harrison (2014) included bark thickness as an adaptive trait in the LPX model, which increased and improved the tree cover for Australia. Resprouting is another important mechanism that changes the balance between mortality and recovery and also leads to an increase in tree cover in fire affected areas in a modelling study (Kelley and Harrison, 2014).”

see also reply 45

Page 12 line 4: “This feedback is included. . .but might be too weak.” Support this statement with more detail. What information indicates that the feedback is too weak? Is this true for all regions? Which figures lead to this assertion?

11) *This still refers to the correlation between burned fraction and tree cover and the highest burned fractions for rather high tree cover in the observations. We suggest*

C7

two possibilities of what could cause the higher correlation in the model, adaptation of bark thickness to fire regime or the feedback between fuel load and tree mortality. We slightly changed the sentence to clarify.

Page 12 line 5-6: “. . .long-lived adult tree state could increase the survival of trees.” How long do trees live in JSBACH? Provide some background on existing parameter-ization of tree life span and mortality mechanisms to support this statement. Include discussion of Hoffman’s work on the ‘fire-trap’ within savanna systems.

12) *We include discussion on the general principles proposed and supported by the work of Hoffmann, however his studies do not offer an explanation why the highest burned fractions are observed for rather high tree cover, which is the main surprise in the comparison here and the subject of the paragraph. The general principles dealt with in the work of Hoffmann are included in the model already. We include his work now in the discussion: “The absence of fire for closed canopies is captured well by JSBACH-SPITFIRE, the modelled strong relationship between higher burned fraction and lower tree cover for open canopies (Figure 4, with the exception of Australia, Table 1), however, is not found in the observations (Figure 2, 4, Table 1). Many general processes determining the savanna-forest boundary are included in the JSBACH-SPITFIRE model: Increased tree cover leads to a suppression of fire by excluding grasses, higher flammability of grasses leads to increases in fire occurrence with increasing grass biomass (Hoffmann et al., 2012). In JSBACH-SPITFIRE bark thickness is PFT specific and depends on the biomass. Tropical trees are represented by two PFTs one of them has a lower sensitivity to fire due to a higher bark thickness and a higher stem leading to a lower probability of crown scorch. This is also observed in field studies where savanna species show a higher ratio of bark thickness to stem diameter (Hoffmann et al., 2003). ”*

Page 12 line 7: “For Australia. . .for both fire models is strong.” Include the fig-

C8

ure this is referencing. Figure 4?

13) Yes, the reference to figure 4 is included now.

Page 13 line 3: Update to “The rank correlation. . . compared to model outputs (Table 1).” Include the reference to Table 1.

14) We included the reference to the table and updated the sentence.

Page 13 -14 line 1: “adapts to changes in climate with usually PFT specific time scales.” What does this mean? Are there variable PFT longevity within simulation?

15) Changes in PFT distributions are not instantaneous the response of vegetation to any climate change is therefore delayed and the delay depends on the PFT specific time scale. We change the sentence to “constant PFT specific time scales”.

Page 14 line 1-2: Include references to examples of DGVMs which include human dimensions.

16) most of the DGVMs and land surface models do as representing land use change is now really a standard, we therefore do not think that references here are useful. The reference to Hantson et al. (2016) already includes a number of models including human properties. Listing individual model references would only lengthen the references section and would be an arbitrary choice of models.

Page 14 line 2: “. . . population density is a commonly used driver.” Driver of what? Ignitions? Land use change? Please clarify.

C9

17) We extend the sentence with: ...commonly used driver for human ignitions and suppression of fires.

Page 14 line 3: Start a new paragraph with the sentence beginning “Our model simulations. . .” and update this sentence to “Our model simulations also show that the modelled climate. . .”

18) This paragraph was rewritten.

Page 14 line 6: Update sentence to “. . . not affected by land use or by the type of fire model. . .”

19) The sentence was removed.

Page 14 line 7: “. . . seasonality that is not resolved by the mean annual precipitation.” The model has no seasonal variation in precipitation and is only using MAP? Please clarify.

20) This part of the discussion was removed. For clarification: the model uses daily precipitation however the comparison was based on MAP.”

Page 14 line 8-13: Include discussion of how the results differ due to the use of only preindustrial land use. Qualify the text in this section to clarify that the JSBACH simulations use preindustrial land use and these products use recent land use (Andela et al 2017 uses the past 18 years). Explain why the comparison is still valid.

C10

21) *We do not compare the simulations with preindustrial land use to recent satellite products. The decrease in burned area due to land use however is supported by several satellite data analysis. We add a sentence to explain that this is not a direct comparison but refers to the isolation of the effect of land use change: "We separated the effect of land use change by comparing the historical simulation to a simulation with preindustrial land use. We find that land cover change is influencing the differences in the modelled fire regime between Africa and South America."*

Page 14 line 12: "The mechanism behind the reduction due to croplands. . ." Reduction of what? Fire occurrence? Please clarify.

22) *We added "burned area" to clarify*

Page 14 line 13: ". . . fragmentation of the landscape, which is not explicitly accounted for in the model." Include discussion of how fragmentation affects forests in reality, and how this may be a challenge for models such as JSBACH. Is this an area for potential improvement?

23) *Fragmentation can certainly affect forests and for instance their biodiversity in many ways, as the sentence is about the effects of fragmentation on burned area, we indicate how fragmentation affects fire. Fragmentation effects on forests are not mentioned here and for a model such as JSBACH we don't see a direct benefit. The fragmentation effects on fire are however very direct as fragmentation often stops fires from spreading. We add:*

"Fragmentation of the landscape by for instance roads can act as a fire break and therefore reduce the potential fire size. The exact relationships between humans, land use and vegetation fires are still unknown and therefore not well represented in models."

C11

Page 14 line 17: ". . .spatially varying ignitions." Do ignitions vary temporally?

24) *The paragraph is about differences in the spatial patterns between continents. The lightning ignitions vary seasonally and the human ignitions vary annually due to changes in population density. However, as we do not address temporal variability of burned area at all in the manuscript and we only evaluate spatial patterns we do not see a benefit of discussing the temporal variability.*

Page 14 line 18: ". . .these differences in ignitions. . ." Differences between what? One is not spatially varied ignitions? Please clarify.

25) *This again refers to the spatial variations in ignitions, we update the sentence to: "...these spatial differences in ignitions..."*

Page 14 line 32-33: Add at the end of the sentence what the values are for the satellite datasets. It is not possible to read them from the figures to compare to this measure of 100 mm and >650 mm per year.

26) *We add:*

The remote sensing datasets show for Africa an absence of tree cover for precipitation less than ca. 300 mm and canopy closure for 1500 mm year⁻¹ in the model resolution (Figure 4).

Page 15 line 6-7: ". . .spatial scale needs to be considered. . ." Add discussion on how increased spatial scale (finer resolution) might improve the model results. Why not perform simulation at 1km similar to the Hirota dataset? Should simulation be finer than 1km? How small of a resolution can you achieve before you see compromised results for simulation?

C12

27) Also here the reviewer asks for answers that can only be addressed by doing simulations in high resolution and testing the influence of model resolution on the simulation results. The only thing we can conclude from our analysis is that some metrics of the comparison depend on the spatial resolution. Such high resolution simulations are also still very computationally intensive and we would not have the necessary computation time available. We now add a sentence to suggest that running the model in higher spatial resolution could improve the performance as the thresholds in the model are closer to the ones found for higher resolution or local scale observations:

“Moreover, as the thresholds found for the model are closer to the ones found for site-level and high resolution satellite datasets the model performance could improve if the spatial resolution of the model is increased.”

Page 15 line 11-12: Are there plans to compare to biomass datasets? Identify potential datasets.

28) Including our plans for future work is an uncommon suggestion and we do not see any use of including them. We add references to datasets (SAATCHI, AVITABILE, BACCINI):

“... and pan-tropical datasets are available (Saatchi et al., 2011; Baccini et al., 2012; Avitabile et al., 2016)”.

Page 15 line 26-28: “The multivariate comparison helped to . . .” Re-word this sentence. It is not clear what is meant by “too strong effect of fire on tree cover”. Split into two sentences to identify problems, and then another to suggest improvements. Clarify where and how increased bark thickness can be included.

C13

29) We changed the sentence as suggested to: *“The multivariate comparison revealed a too strong impact of fire on tree cover for gridcells with very high fire occurrence, which leads to too low tree cover. Possible model modifications to boost the tree cover in exactly these regions with high fire occurrence are an adaptation of trees to fire by increasing bark thickness in response to high fire frequencies or a stronger negative feedback between fire occurrence and fuel load. This stronger feedback should then reduce fire intensity and consequently fire mortality.”*

Page 15 -16 line 1: “although known variations in vegetation characteristics are not represented in models. . .” Provide a brief description of what is not represented? Bark thickness variability, size-structure? Consider adding a stronger concluding sentence to identify how these improvements will be helpful to models.

30) This is meant to refer to the differences found for instance by (Lehmann et al., 2014). These known differences are however not well enough understood to be implemented in models. We changed the sentence to: *“Known variations in vegetation are not sufficiently understood to be represented in models. However, our finding that models do show differences in the fire-vegetation-climate relationships between continents shows that further exploration why models show differences can be helpful to better understand causes for intercontinental differences.”* We can only suggest improvements whether they will really be helpful or not needs to be tested with such modifications implemented in models and comparisons of simulations with and without these modifications.

We add as a last concluding sentence: *“Overall the multivariate model evaluation highlights the potential for more targeted model improvements with respect to the interactions between climate, vegetation and fire, which are crucial for our understanding of future vegetation projections.”*

C14

2 Reviewer 2

The ability of JSBACH to reproduce the observed relationship between fire, tree and grass cover and mean annual precipitation (MAP) was assessed using two different coupled fire models, with the implicit aim of guiding future model development. Analysis was split between continents to assess different regional climate-vegetation-fire relationships, and using present day and pre-industrial land use to assess the models ability to reproduce human impact on burnt area within bioclimate. The authors successfully demonstrates the potential of this approach by identifying too high tree cover at low precipitations, high burnt area in areas of low tree cover and cropland representation as key model weaknesses, before speculating on likely causes and solutions. The approach is relatively simple but, as the authors point out, is also quite a novel way of identifying areas for improvements in vegetation-fire models which will hopefully be adopted by other modelling groups. I also like that the paper is solely dedicated to model assessment, despite the distraction of including JSBACH-standard (see comments below), and I look forward to seeing if this results in better informed, targeted model improvements in the future. If so, it could be a process the rest of us in the fire modelling community could learn from.

I do, however have a serious concern about the choice of driving data that needs to be addressed before I recommend publication. I also have a few other major comments, although some might just require brief clarification through author response and small changes to the m/s. Given the potential changes to the manuscript required to address the first major comment, I have only included a few key specific suggestion for now, largely for the introduction.

JSBACH-fire was driven using simulated climate from the MIP Earth System Model. However, almost all the evaluation is of JSBACH-fire component alone. This is clearly

C15

a problem for the basic spatial evaluation in most section 3.1 and figure 1, where it is often unclear if mismatches in vegetation cover or burnt area is because of JSBACH itself or because of biases in the Earth System Models (ESM) climate simulation. As the rest of the paper is evaluating JSBACH in climate space, it could be argued that the choice of driving data doesn't matter. However, simulated climate biases could still be playing a role even here. For example, the authors only use MAP as a climate proxy. Inherent in MAPs influence on fire are the extreme conditions, specially the length dry periods, that increases susceptibility to burning. This is part of the reason for the wide range in fire and tree cover at a given MAP in all but the driest and wettest climates, and is invoked by the authors to explain different tree-MAP relationships in Australia.

31) We agree that biases in the forcing can have an influence on the model evaluation and that the same simulation driven with reanalysis data would have different results. While the traditional variable by variable evaluation for instance shown in figure 1 is highly dependent on spatial biases our approach presented here largely overcomes this limitation. The focus of this paper is on the multivariate comparison that evaluates the model in climate space. We use the standard JSBACH setup, which is the combination of JSBACH with MPI-ESM meteorology. As the fire is sensitive to a number of variables, evaluation of the model in a different setup wouldn't help to guide model development for a model that is almost only run in the coupled setup. The evaluation of the model within the climate space helps to reduce the impact of climate model biases on the model evaluation and therefore to focus on biases in the land surface model. Moreover, our motivation here is to evaluate the land surface model, a detailed evaluation of climate biases in the ECHAM model is therefore out of scope. Understanding potential influences of certain climate biases (such as extremes) on the simulation would require specific factorial experiments. While this would certainly increase our knowledge, it would not lead to an improvement of the coupled model system unless the climate biases can be improved. Mean annual precipitation explains

C16

a large part of the tree cover variability and therefore is a useful proxy for climate. Moreover we can relate model-data mismatches to this simple proxy, it is therefore informative. While certainly more parameters influence tree cover distribution an increasing number of variables included to explain patterns would require a totally different approach, as ours is largely based on the possibility to visualize the relationship between the variables. Three variables are a natural limit here (x-y scatter plot + color scale). We introduce our motivation for showing the geographic patterns and for our evaluation approach now in the beginning of the results section: “We first give an overview over the geographical distribution of the used observation and model output datasets. The comparison of geographical patterns is an important assessment of model performance, it is however difficult to assess whether the interactions between precipitation, fire and tree cover are well captured. Moreover as the JSBACH model is usually used as a land surface model for the MPI-ESM and therefore also here forced with MPI-ESM output, biases in model forcing can cause geographical biases of vegetation and fire variables even with a perfect fire and vegetation model. To reduce the influence of biases in forcing data on the model-data comparison and allow to more closely evaluate the interactions between model components we propose a multivariate evaluation of climate-fire-vegetation relationships. We assess the robustness of observed relationships for two tree cover datasets and two spatial resolutions and compare them to the model simulations. The last paragraph of this section addresses the influence of land use change on the simulated relationships.”

2.1 Choice of JSBACH driving data

JSBACH-fire was driven using simulated climate from the MIP Earth System Model. However, almost all the evaluation is of JSBACH-fire component alone. This is clearly a problem for the basic spatial evaluation in most section 3.1 and figure 1, where it is

C17

often unclear if mismatches in vegetation cover or burnt area is because of JSBACH itself or because of biases in the Earth System Models (ESM) climate simulation. As the rest of the paper is evaluating JSBACH in climate space, it could be argued that the choice of driving data doesn't matter. However, simulated climate biases could still be playing a role even here. For example, the authors only use MAP as a climate proxy. Inherent in MAPs influence on fire are the extreme conditions, specially the length dry periods, that increases susceptibility to burning. This is part of the reason for the wide range in fire and tree cover at a given MAP in all but the driest and wettest climates, and is invoked by the authors to explain different tree-MAP relationships in Australia. General Circulation Models (GCMs) are notoriously poor at simulating dry periods, with many underestimating the length and/or severity of dry periods due to poor simulation of convective vs persistent rainfall and a problem with persistent drizzle (DeAngelis et al. 2013; Gutowski et al. 2003). Length of dry periods is fundamental in the calculation of ignition probability and each fire's area in SPITFIRES rate of spread model (Thonicke et al. 2010). The “standard” fire model used in this study sounds like it could be similar to GLOBFIRM? If so, this is also very sensitive to number of dry days, with a rapid increase in burnt area in longer dry seasons (Thonicke et al. 2001), which would explain at least part the underestimation of maximum burnt areas. Either way, driving and comparing JSBACH with ESM output could skew the MAP relationships with fire and potentially tree cover in figure 3-6 and A1-2. This is by no means the only problem with driving the model with ESM data, but it is the one that springs to mind. Using MPI also required the authors to make a rather awkward decision between performing comparisons on different time periods (1996-2005 from JSBACH runs; 2001-2010 from observations) or on the few years of model-observation overlap.

I have two suggestion for how the authors could address this problem:

1. Continue to use the MPI driven runs, but reframe the paper to evaluate the processes and identify weaknesses in simulation of tree cover and fire in the ESM as a whole. Some of the arguments I have made above as to how ESMs simulated climate

C18

could affect tree cover and fire could be included. However, there are likely many more, some specific to MPI. If there are any special required configuration of JSBACH to simulate vegetation dynamics under MPI then these should also be included. The authors briefly touch on two arguments that could also be expanded: lines 11-14 on page 6 uses figure 1c to briefly discuss whether MPIs MAP biases as a reason for some of the mismatches between observed and simulated burnt area; and lines 7-15 on page 13 where the mismatches between driving data introduced straight into the fire model (popdensity, lightning etc) and those driven from MPI climate output. Section 3.1 should just need re-formulating with no new analysis. Subsequent sections may require fresh analysis, potentially looking at multivariate relationships within the space of MPIs driving data.

2. Run JSBACH with climate observations, including using common precipitation observations for driving data, and analysis of observed and simulated fire, MAP and vegetation cover. According to (Rabin et al. 2017), JSBACH model output should be included in fireMIP, in which case, vegetation cover by PFT and burnt area from observation driven JSBACH will be available from fireMIP.

32) See our previous reply. We include some of the points mentioned by the reviewer to improve the discussion on our setup choice (see below). Evaluating the details of climate biases of the MPI-ESM is out of scope of this manuscript. Clearly there can be biases due to climate biases in the simulations of JSBACH. However evaluating the model in a different setup seems less promising and less targeted to us than our approach to evaluate the model in climate space using the setup it is usually used with. As the reviewer also acknowledges we mention the limitation of the input datasets determining the ignitions as inconsistency. However regarding the conclusions we draw from our comparison we don't see a strong point that they would be strongly affected. The reviewer states that the rainfall seasonality is especially important for the "wide range in fire and tree cover at a given MAP in all but the driest and wettest climates". We show that the relation between precipitation and burned area is captured quite

C19

nicely at least with SPITFIRE and for the relation between MAP and tree cover we look at exactly the thresholds of these driest and wettest climates, not at the variability inbetween. For the intermediate rainfall regions we focus on the relationship between tree cover and fire. We don't see a good reason why other climate biases should decrease the correlation between fire and tree cover, which is the point of our focus here. FireMIP simulations in this setup are unfortunately not available. Within the first round of FireMIP simulations (Rabin et al. 2017) the model was set up with prescribed vegetation. For recent simulations we did also similar simulations with dynamic vegetation however this model includes a number of changes, such that the versions are not comparable anymore. We include a paragraph on the model biases in the discussion of model improvements:

"Many climate models have problems to represent extremes, length of dry periods and tend to generate a permanent drizzle (DeAngelis et al., 2013; Gutowski et al., 2003). With our approach we only include mean annual precipitation, other aspects of the modelled climate are neglected but might contribute to model-data mismatches in the relationship between precipitation and other variables. Mean annual precipitation is however a strong driver of vegetation patterns especially in the tropics and including more climate parameters would require an entirely different approach and possibly limit visualization and interpretation of the results. Including more climatic parameters could especially help to interpret more of the variability for mean annual precipitation amounts that allow tree establishment but do not lead to complete canopy closure. The reasonable relationship of mean annual precipitation and burned area however indicates either that additional climate biases are not important as fire is quite sensitive to the length of dry seasons or that that the fire model cancels out additional climate biases."

C20

2.2 Choice of fire dataset

Is there a reason for use of GFED4 instead of GFED4 with small fires (GFED4s) (van der Werf et al. 2017)? There may be a good reason for not including small fires, but given the prevalence of GFED4s in other fire evaluation studies (Rabin et al. 2017; Kelley et al. 2014; Kloster & Lasslop 2017), it might be worth including some justification. Also, are the certain weaknesses in fire detection in GFED that might affect the results? The missing small fires for example should be mentioned as a caveat in relation to results from figure 3.

33) We use here the global burnt area dataset with the highest accuracy (Padilla et al., 2015). The dataset does underestimate small fires, and a recent version of GFED4 (GFED4s) tries to take these into account. However, the small fire detection procedure has been strongly criticized and is highly uncertain see eg, (interactive discussion van der Werf et al. (2017)). Therefore, we decide to use GFED4 for the moment as it has a proven high quality and refrain from using GFED4s until its accuracy has been shown. The spatial patterns of the two datasets are very similar (see Randerson et al., 2012, Figure 7), the main difference is that the GFED4s has a 25% higher burned area. These small fires are often related to croplands or deforestation. The models used here do not model deforestation or cropland fires, therefore aiming at this high burned area that includes these fires would not be an advantage. As far as we know there is no quantification of other weaknesses of the burned area datasets, therefore speculating about the extent and whether they would influence our results would be difficult. We add a reference to an evaluation study and mention the main sources of uncertainties. We add in the discussion section:

“The latest release of the GFED burned area and emissions datasets includes an extension for small fires (Randerson et al., 2012). However these small fires are often related to cropland fires or deforestation fires. Neither of these fire types are modelled

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explicitly in our model approaches and therefore could cause an unwanted mismatch. Cropland fires are not expected to strongly influence the vegetation cover, while deforestation is prescribed as described in the model and simulation paragraphs and therefore the influence on vegetation cover is considered. Burned area datasets are generally uncertain mainly due to the limited spatial and temporal resolution (Padilla et al., 2015), the difference in global burned area between the dataset including small fires and the one not including small fires is 25%. The spatial patterns are less affected, but missed burned areas due to high cloud cover certainly introduces also spatial biases. How important such errors are for a comparison as present here is unknown.”

2.3 Quantification of similarity in multivariate relationships.

The observed relationship between MAP and tree cover is described as either “linear” for Australia and “sigmoid” for other continents, with the ability of each model to describe each curve used as evidence when identifying model weakness. However, I’m not sure I can see these relationships. Observed Australia looks more like the start of a sigmoid (albeit with a shallower gradient when compared to e.g. Africa), “chopped” at low tree covers. South Africa looks more linear. A simple curve fitting and correlation could help determine how closely each continent resembles each function, and if the model is reproducing this relationship, which would place subsequent discussion on firmer ground.

34) This is an interesting idea and indeed the purely visual comparison was indeed not that firm. As likely only the maximum tree cover for a certain precipitation amount is limited by precipitation and lower tree covers are likely modified by other factors we used a quantile regression to characterize the relationship between precipitation and

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maximum tree cover. We use a linear regression and a local regression to illustrate the difference between the linear and nonlinear/sigmoid increase. We include a paragraph in the methods section:

We use quantile regressions to characterize the relationship between precipitation and maximum tree cover. The quantile regressions were computed with the R package *quantreg* (Koenker, 2018). We use the local quantile regression to characterize the shape of the increase in maximum tree cover for increasing precipitation. Moreover we quantify the deviation from a linear increase by also including the linear quantile regression. Both regressions were computed for the 0.9 quantile. For the local quantile regression the bandwidth parameter was set to 300 and the number of points where the function was estimated was set to 10.

Adopted the paragraph in the results section:

“Models and observations show differences between continents in the relationship between precipitation and maximum tree cover (Figure 5). For Africa, South America and Asia the relationship between maximum tree cover and precipitation shows a saturation for high precipitation. For Australia maximum tree cover increases linearly with increasing precipitation for models and observations, but the precipitation range also does not reach values where a clear saturation is reached for the other continents. For JSBACH-standard the curves are very similar for the different continents. JSBACH-SPITFIRE shows a stronger variation, this must be due to the differences in fire as the model is otherwise the same. The observations show an even stronger variation between continent, with clearly lower tree cover values for Australia followed by Asia. For Africa local quantile regression clearly differs from the linear quantile regression for the satellite data, indicating a sigmoid shape, while the other continents show a rather linear increase until the saturation (Figure 5). JSBACH-SPITFIRE reproduces the higher tree cover for South America compared to Africa for mean annual precipitation lower than 1000 mm, but also JSBACH-standard shows a small difference.”

In the discussion we remove the paragraph on the discussion of the linear increase

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for Australia in comparison to the Lehmann et al. (2014) and focus on the point that models do also show some differences between the continents. We add in the end of the discussion:

The comparison of the increase in maximum tree cover with increasing precipitation shows that the model shows some variability in climate-vegetation-fire relationships between continents, it misses a large part of the variability. Finding the correct balance of the many influencing factors, e.g. climate, fire, land use, evolutionary differences, will remain a challenge for the future.

The remaining multivariate comparisons is also largely based on visual comparisons on plots. While this is an important part of assessing differences in simulated vs observed relationships, I feel like the comparison could do with some quantification using some simple multivariate metric, expanding on the two-variable assessment in Table 1. I am by no means an expert in multivariate statistics though, and perhaps a “simple” comparison isn’t possible. But if the authors have any thoughts on this, it would be good to hear (and perhaps include them in the m/s?)

35) This is a very interesting suggestion. However we believe that such a metric would still need to be developed. We did not find an applicable, promising approach in a web search. Also we are not sure what this multivariate metric could represent. Correlations can only capture linear, rank correlations monotonic relationships. This made sense for precipitation and tree cover, the relationship with fire however is more complex. Probably an approach based on regression methods, including also nonlinearities could be a way forward. This seems promising to us, however it would deserve more attention and in depth testing. Nevertheless we now also include the rank correlation between fire and tree cover for a certain precipitation and tree cover range in the Table 1 in addition to the correlation between precipitation and tree cover. This quantifies the stronger impact of SPITFIRE on tree cover compared to the observations and also reveals that JSBACH-standard has a reversed relationship

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between fire and tree cover, likely due to the exclusion of pastures for burning.

2.4 Use of two models

More could be made of the use of the “standard model” (JSBACH-standard) to help analyse MPI, JSBACH or even SPITFIRE performance, which is obviously the model the authors will use in future studies. As a start, JSBACH-standard could do with a little bit more description to help inform later discussion. Are the curves describing relationship between relative humidity, fuel carbon and fire similar to GLOBFIRM (Thonicke et al. 2001), or are they more similar to those simpler rate of spread models such as CTEM (Arora & Boer 2005). Are parameters used by the model based on literature, site comparisons, or optimization of remote sensing? If the latter, is its poor performance likely due to biases in JSBACH simulation of vegetation or MPI climate and dizzle biases? If the former, is it additionally due to fire model structure or bad parameterizations? How much is PFT fraction removed after fire? Is 100% of burnt PFT removed, or just a fraction? If a fraction, does this vary? And does it vary by life form, PFT, burnt areas or some other relationship?

36) The simple fire parameterization is described as: “The JSBACH-standard fire computes burned area based on a minimum burned fraction which increases as a function of the litter carbon pools and relative humidity averaged over the last three weeks.” And there is really not more in terms of burned area. So probably it is closer to GLOBFIRM, but it is also unclear how to quantify whether it is close to one or the other model. We add that the model was tuned to yield reasonable global emissions estimates and improve the tree cover, there was no comparison with site level or remote sensing products. We already included that:

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*“In the JSBACH-standard fire scheme the burned area directly translates into a reduction of the cover fractions of the plant functional types (PFTs) ...”
we add: “(100% of the cover fractions on burned area are removed)”*

A better comparison between the two models in the discussion and/or conclusion might also further strength the case for use of multivariate approach. Despite its poor performance is there any part of the standard model multivariate relationship that could be used to guide development of SPITFIRE, particularly with respect PFT tree mortality? Is there any conclusion that can be drawn on the use of complex fire models to represent complex processes such as fire and fire-feedbacks, or does any part of the standard models performance (i.e, locations of fire occurrence) suggest that emergent behaviour of fire on coarse scales does not require the use of complex models? Does a comparison of strength and weakness of the two models say anything about the coupling to JSBACH or required configuration for use of JSBACH-fire in MPI?

Ofcourse, if the authors feel like nothing substantial can be learnt from comparing the two models, then they could consider removing JSBACH-standard from the m/s. However, there is nothing technically wrong with its inclusion, so I'll leave that for the authors to decide.

37) The comparison with the simple (poor performing) fire model mainly shows that improvements in the fire model lead only to small improvements in vegetation patterns. We think that answers to the very specific questions of the reviewer would be highly speculative and would require additional analysis. It is also often unclear how much difference in performance is due to better tuning and how much due to a better model structure, as none of the models is optimized.

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2.5 Specific comments

Page 2, line 24-25: The development of complex fire models actually started before widespread use of remotely sensed products. MC2 (Lenihan et al. 1998) forms the basis for most rate of spread models (Hantson et al. 2016), and SPITFIRE is itself a development of Reg-FIRM (Venevsky et al. 2002). Neither invoke the use of remote sensed burnt area.

38) *We modify the sentence, the recent implementations of SPITFIRE for instance have made strong use of satellite data: "The development of remotely sensed global burned area products facilitated the implementation and evaluation of complex fire models within DGVMs (Hantson et al., 2016)."*

Page 2, line 30: "the importance of benchmarking effects on vegetation has been noted". Not just noted, but also done (Kelley et al. 2014).

39) *We extended the sentence in the revised manuscript with:
... and applied in model development studies (Kelley and Harrison, 2014; Lasslop et al., 2014)*

Page 3, line 11: Please use -180 to 180 coordinates longitudes.

40) *We updated the coordinates for the South America region.*

Page 4, line 2-3: Replace (or include along side) "(Rabin et al. 2017)" with "(Thonicke et al. 2010; Lasslop et al. 2014)". (Rabin et al. 2017) does provide description of SPITFIRE alongside several other fire models, but the authors should also give credit to the model developers.

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41) *We refer to the two older publications in the beginning of the paragraph, where SPITFIRE is mentioned the first time. (Thonicke et al. 2010 is included there now too.) Rabin et al. provides the most up-to-date, complete and detailed description and therefore deserves a reference too.*

Page 4, line 14: "During the 1000 year spin up period . . ." How was the spin-up determined? Where carbon or PFT fractions/burnt area in equilibrium by this point? How was this tested?

42) *The spin-up period was determined based on experience, we did not apply a formal test criterium. PFTs are largely in equilibrium after 1000 years, small changes especially between woody PFTs can still take place in some grid cells. Global tree cover on the other hand equilibrates after around 300 years. We included:
"At the end of the 1000 years PFT distribution was largely in equilibrium with only minor shifts between woody PFTs in few grid cells."*

Page 5, line 31 - Page 4 line 1: Please add a citation to the r-package paper. I think (Tuck et al. 2014) is the correct reference, but the authors should check. Also include a direct reference to the r package used. Typing in the following in an R terminal should give you the require bibtex information:
Å citation(Årpackage nameÅ)

43) *We included the publication indicated with the citation command (Mattiuzzi and Detsch, 2018). Tuck et al. 2014 seems to be the citation for the MODISTools package.*

Page 5, lines 12-13: Is any scaling applied when translating from LIS/OTD flash count to ignition sources? I.e, are cloud-cloud flashes removed?

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44) *Cloud-cloud flashes are removed, however, we refer to the model description papers for the exact model formulations and here only document the input files. We also don't detail how population density is converted into ignitions and want to keep a similar level of details for the lightning ignitions.*

Page 11, line 10 - Page 12, line 1: (Kelley et al. 2014) collected cite based bark thick- ness data to reparametrize bark thickness in a SPITFIRE based model. There might also be some Australia specific improvements in this paper that could be considered.

45) *Yes, we include the reference to Kelley et al. 2014 for the bark thickness as an adaptive trait and the resprouting mechanism which acts in a similar way to increase tree cover:*

"Kelley and Harrison (2014) included bark thickness as an adaptive trait in the LPX model, which increased and improved the tree cover for Australia. Resprouting is another important mechanism that leads to an increase in tree cover in fire affected areas (Kelley and Harrison, 2014)." see also reply 10.

Page 14, lines 21 - 23: The non-independence of vegetation cover datasets should be included when introducing the datasets on page 4 and 5.

46) *We mention the similarity of the two datasets in the methods section in the revised manuscript.*

"The datasets rely on different sensors, however, the algorithms to derive vegetation cover are very similar and the datasets therefore not completely independent. Nevertheless using the two datasets can give a first insight on the robustness of the investigated patterns."

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Page 17: Please complete author contributions.

47) *We completed the author contributions.*

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