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

Anonymous Referee #2

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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 so-

lutions. 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.

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 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 influance 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 dizzle (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 JSBASH 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 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 (pop

density, 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.

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 smalls fire's for example should be mentioned as a caveat in relation to results from figure 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.

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?)

Use of two models

More could be made of the use of the "standard model" (JASBACH-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 remove 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?

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.

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.

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

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

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.

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?

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:

» citation(«package name»)

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

Page 11, line 10 - Page 12, line 1: (Kelley et al. 2014) collected cite based bark thickness 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.

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

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Page 17: Please complete author contributions.

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