This paper documents the integration of SPITFIRE into FATE within CLM model. It then explores how fire-related traits can influence climate-vegetation-fire interactions using three hypothetical scenarios of different fuel drying parameters mainly over South America. The results are encouraging and show well the importance of representing fire-related ecological traits and integrating size-related fire mortality in land surface models. The paper is generally well written although with some minor errors (see my detailed technical comments). I have some general comments followed by more technical ones listed below:

General comments:

(1) It should be stated clearly that the different drying ratios are used as hypothetical scenarios, simply because only one of them should represent, or be the closest to, the reality. For this reason, I suggest the authors always take caution when they interpret their results by comparing with observations because the model has not yet been fully parameterized and as a result, any disagreements with observations are not a surprise. Rather, the different scenarios are used to explore how fire activity can influence the distribution of forests (and grass) with different fire vulnerabilities.

(2) Table 2 is a bit overwhelming for the readers to know the key differences between the two forest PFTs with different fire vulnerabilities. A short paragraph summarizing these differences with brief explanations on why they represent different fire vulnerabilities would help a lot.

(3) For the key message of the paper, I think using the case of South America would suffice. I don't see a lot additional value by including the simulation for the whole tropics but this makes the whole manuscript more complex. I hence suggest simply removing the results for tropics. This will not reduce the value of the paper. In addition, the model is not subject to a full parameterization, which makes correct simulation over the whole tropics unlikely. Lines 512-514 also refer to the continental differences that are not accounted for in the current parameterization of the model. One may not expect a model to represent the gradient across tropics by using two tree PFTs with different vulnerability. As has also been pointed out by the authors in lines 530-545, the model cannot reproduce the co-existence of tree and grass over the tropics. Compared to Fig. 1 in Staver et al. (2011), Fig. 13a failed to reproduce the large spread in tree cover for the medium rainfall of 1000-2500 mm. Also, the increase in tree cover with rainfall is much steeper in Fig. 13a. The last factor is that tropical forests are subject to anthropogenic fire, deforestation and forest degradation, which are not accounted for in this study.

(4) For model validation: do you use observation corresponding to the simulation period? This is not mentioned in the manuscript but I may have neglected it.

(5) A short paragraph in the Methods describing how forest dynamics related to disturbance are represented in FATE would be helpful for the readers to better understand the work. For lines 268-269: I don't understand whether tree mortality was simulated as partial mortality or treated as stand-replacing mortality. In the latter case, I guess, say, a 10% mortality is simulated, you just take this 10% area and start it with a new patch?

(6) The discussion is in general a little excessive given the hypothetical nature of the study. For example, lines 530-545 focus on comparing tree cover distributions with the observation but from the above we know that many reasons contribute to the disagreement; lines of 622-634 on the anthropogenic effects of fire seem not relevant with this study.

Technical comments:
Line 69–70: it would be nice the authors could expand the discussions here and give a brief description of the status quo on how the effects of tree size and bark thickness on fire-caused mortality are represented, or not represented, in current fire models embedded in land surface models. I understand this needs a bit of work but it can provide a nice overview.

Line 125: regarding the Nesterov Index. I checked that in Thonicke et al. 2010 daily maximum temperature, rather than daily temperature, was used. But in Venevsky et al. (2002), indeed daily temperature was used. So changes are needed here: (1) at least avoid citing both when using daily temperature but rather explain clear the usage in both citations and explain clearly which one was used here. (2) could you explore a bit the influence by following Venevsky rather than Thonicke et al.? This might seem minor but it would be nice to do this.

Line 113: starting from the fact that the data was used in Li et al. is not a good justification. Maybe just say that you used this dataset and Li et al. also used it?

Line 116: “under favorable conditions for burning”: a crucial detail. What do you mean by ‘favourable’ here? How is it implemented in the model? Do you use a constant of 10% or this percentage changes with something (e.g., FDI)?

Line 133: “Weighted averages across fuel classes”: what has been used a weight? There are two options, one can either use surface-to-volume ratio or fuel weight.

Line 133-135: does FATE allow tree growing over grasses? The description here seems that CWD, dead litter and live grass form an integrated vertical profile. This is also relevant to lines 529-531. Fig. 7 shows biomass for different PFTs rather than their grid coverage and hence it is unclear whether the ground coverage of different PFTs also show a similar pattern.

Line 139: does ‘fine’ fuels mean leaf litter? Does FATE represent branches of different diameters so that it is straightforward to classify dead branches into different wood fuel classes or you need some allocation/partitioning scheme?

Line 145: define what is fuel moisture, because some studies use water/(water+dry fuel), some use water/(dry fuel)

Line 11&12 in Supplement: Eq xx, please check

Line 13 in Supp: what do you mean by ‘fuel moisture consumption’?

Eq. 3 and Eq. 4: explain what ‘fc’ stands for to increase readability.

Line 166: the authors assume the reader know the meaning of ‘drying raito’ by default but I don’t understand

it.

Eq. 4: define SAV_fc

Line 156: $\propto$fc, $\propto$ is a strange symbol. Is it the same sign as infinitive? Could you use something easier to understand, read and remember? What do you mean by 'user-defined'? does it mean that for every model application, e.g., in different regions, this parameter needs to be parameterized ? Or it just means that it is parmaterizable?

Lines 145-165: Overall the readability around the fuel moisture simulation part is poor. Could you improve it? More detailed descriptions will allow others to reproduce your research more easily.

Table: citation of Table 1 is poor. Some names in the first column are strange and I don't know where they are used and what they are for, e.g., Rows of 3-9.

Line 189-191: do you check how many fires were actually suppressed/extinguished due to this threshold?

Line 199: l_b has no unit? L_b should be defined as 'major to minor axis ratio' (consistent with Eq. 14), the text gives the reverse.

Line 202-204: could you please detail about this error? Is it about Eq. (10) or the second half of Eq. (11)? Your second half of Eq. (11) is also different from Thonicke et al. (Eq. 13 in their articles).

Eq. (15): a critical detail here: combing back to my comment on Line 116, is $l_{lightning}$ here exclusively scaled by 10%?

Eq. (19): I found an error in the middle sub-equation when implementing spitfire in the recent ORCHIDEE trunk version. 0.563*0.22= 0.12386, which is smaller than 0.125. So there is chance that you might get negative value from this equation which is not plausible. I suggest replacing 0.125 by 0.12386 to avoid this. I found this because it prompts mass balance error in ORCHIDEE due to a negative value.

Line 284-285: was fire module switched on from the very beginning of the simulation? I don't know if there is a risk that trees are too small at the very beginning and they get repeatedly and easily killed by fire. Do you have this issue?

Fig. 2: are these results (panels a, b, c) averaged only for days with fire occurrence? How should I understand panel d? for the green line, if I accumulate the values across all months, does it mean that all grid cells have been almost twice (with accumulative value is about 2 judging by eyes)?

Fig.3: what are these temperatures? Land surface? Surface air? Fire flame?

Table 3: I suggest changing ‘low fuel drying’ to ‘low fuel aridity’, ‘medium fuel drying’ to ‘medium fuel aridity’. Because there is the parameter of ‘fuel drying ratio’, using ‘drying’ in both of them makes easy confusing.

Table 4: Adding maximum value makes the table a lot more brain-consuming to read. Is it really necessary? Are the data mean values across all grid cells + years?

Fig. 4: Better to use the name of fuel class (1hr, 10hr, …) rather than ‘small branch’, ‘twig’ because the latter gives an impression that we indeed have these being represented in the model but actually we are partitioning the biomass into different fuel classes.

Line 373: still, from the top panels of Fig. 5, there is a tendency of higher intensity with lower live grass fuel moisture?

Line 302-304: What’s the role of wood density in the model or how this relates to the simulation of fire or vegetation processes? This question is also relevant for lines 467-468.

Line 497: ‘mortality threshold’ in this line implies something quite precise but I don’t think there is any threshold in the model to determine whether a tree was killed completely or not. The mortality is simulated as a continuous number (fraction) indicating mortality rate? No?