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Interactive comment on "Varying relationships between fire intensity and fire size at global scale" *by* Pierre Laurent et al.

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Major concerns:

I) The first is that the study is based on the GFED regions but then wants to interpret differences between the regions to be driven by biomass availability and drought. If you want to understand whether differences are driven by biomass and drought then a much more straight forward way to analyse the data would be to group them according to biomass and drought, not according to the GFED regions that average over all Northern Hemisphere Africa, which contains the whole gradient from desert with low biomass and strong drought to tropical rainforest with no drought and high biomass. I am very confident that the results could be much clearer and support your conclusions much better if the study design was rearranged to directly look at the effects of drought and biomass on these relationships, by grouping the data according to these two parameters.

Answer: We agree that relying only on GFED regions tends to mix together biomes with different biomass, fuel types, and with very different drought conditions. The problem with the use of drought datasets is that it is difficult to choose how to perform the separation between different levels of 'drought severity' : we could focus on the length of the drought season, or the severity of the Fire Danger Index, a combination of both, etc .... This choice would seem quite arbitrary, and would require a dedicated analysis. Instead, we propose to use MODIS Land Cover Data to separate each GFED regions in different biomes (Forested, (green) Savannas (light green), Grasslands/Shrublands (orange), see Figure attached to the answer). We clearly see that the relationship varies with the biomes : the results are especially striking in Australia, where we see that the FRP/FS relationship differs a lot depending on the considered biome. Finally, since we do not directly study the relationship with biomass and drought, we removed from the abstract and the discussion the sentences where we claimed that the fire intensity was driven by these quantities.

Separating our analysis depending on land cover is also important regarding the second major concern that you have raised (and that has also been raised by the other reviewers). We considered that FRP could be used as a proxy of the fire reaction intensity in the flaming front, but we did not discuss the limitations of such an approach. Particularly, we realized that the reliability of this hypothesis strongly depends on the land cover : for grassland, most of the energy is released in the flaming front, whereas for forested areas, radiation from smouldering fires also contribute to FRP. Therefore, the separation into land cover also appears very natural, and we can now discuss the hypothesis 'FRP is a proxy of fire intensity' and the reliability of the results depending on the considered land cover.

Note that what we suggest here is a 'double' separation (GFED and Land Cover). We think that keeping the separation into GFED regions (and separating each of them into different Land Cover) is important for two reasons : - They define regions which are widely used within the fire community - Grouping all fires belonging to a given biome without separating in GFED regions would mix together regions with different fire practices/policy/management

II) The second is that the manuscript needs a discussion of reliability of the data, especially for the fire intensity. There are a number of limitations on the observability of fire radiative power. The point that there are still these clear spatial and temporal patterns in my opinion indicate that there is useful information in the dataset, however the problems associated with the dataset should be mentioned and discussed. For instance the energy observed is the energy released in one pixel, this energy might come from a very intense fire covering a small part or a low intensity fire covering large part of the grid cell. The observation of fire intensity strongly depends on the scan angle. Moreover fire intensity has a diurnal cycle and peak fire intensity might differ between the biomes. The satellite overpass might happen at the peak time in some grid cells but not in others. Vegetation structure influences what the satellite can observe, intensity of sub-canopy fires will certainly be underestimated. I am not an expert in remote sensing, but I think that such issues need to be mentioned to provide a balanced discussion of the results.

Answer: A discussion on the data reliability was clearly missing. We realized that we did not defined well-enough what we meant by “fire intensity”, and how does this relate to FRP. Moreover, we did not discuss or reference the spatial and temporal sampling error that might impact the measurements of FRP. We plan to do the following changes in the manuscript :

- First, we would like to replace in the text ‘Fire Intensity’ by ‘Fire Radiative Power’. What we observe is FRP, and then we interpret it as a proxy of fire reaction intensity. We would also like to change the title of the article into : ‘Varying relationships between fire radiative power and fire size at global scale’ if the editor agrees.

- Second, we will provide a dedicated section in the discussion (with references) to thoroughly discuss these issues. Note that the separation into land cover strongly helps to discuss the reliability of FRP as a proxy of fire reaction intensity, since this is expected to depend on Land Cover. Here comes a draft of the dedicated discussion:

“In the previous section, we hypothesised that FRP could be used as a proxy of fire reaction intensity. We now focus on the limitations of such an approach. First, the energy released by a wildfire can be decomposed in three parts : convection, conduction, and radiation. FRP only represents the radiative part of the energy emitted by a fire. Moreover, the fire reaction intensity used in Rothermel’s equation does not share the same spatial extent as FRP : fire reaction intensity pertains to the flaming front of the fire, while FRP integrates all the radiative energy emitted over a 1 km2 window. This means that radiation emitted from smouldering can also contribute to FRP, not only the flaming front. The impact should differ for different vegetation types : smouldering fires are more frequent in forested areas, whereas in grasslands most of the detected radiative power will be released by the active fire front. Another issue appears from the integration of radiative energy over the 1 km2 window : it is impossible to know if the detected FRP arises only from a fire covering the full 1 km2 area or only from a smaller fraction of the FRP pixel. However, we can expect this effect to be mitigated by the fact that our analysis does not account for very small fires, since the FRY database does not provide fire patches smaller than 107 ha for MCD64A1. Finally, a recent study (Roberts et al. 2018) used 3D radiative transfer simulations to show that the canopy structure intercepts part of the FRP emitted by surface fires. This means that the FRP measured from remote sensing for forested areas and savannas could underestimate the real FRP. We can also expect this underestimation to vary with tree species. For example, it is probable that the amount of radiation energy intercepted by the canopy differs strongly between canopy fires from highly flammable black pines from BONA (Rogers et al. 2015) and surface fires from pine needle bed in BOAS. All these considerations emphasize the importance to split the study of the relationship between fire size and FRP in different vegetation types, since the reliability of using FRP as a proxy of fire reaction intensity depends on it."

"The amount of radiative energy reaching the MODIS instruments is much smaller at large scan angles than at Nadir. This means that the MODIS instruments will be less sensitive to low values of FRP at high latitude (Giglio et al. 2003, Schröder et al. 2005). This could explain the difference of the distribution of FRP associated with fire patches in BONA (Figure 2) : the stronger asymmetry of the distribution in this region (i.e. the larger tail toward high FRP values) could arise from missing active fire data from less intense fires in this region. The temporal sampling of FRP also differs with the latitudinal coordinate : the number of satellite overpass is larger at high latitude than at the equator (from 2 observations per day until 15 at the poles, Giglio et al. 2006). This should rise the probability to recover FRP information for fire patches at high latitude, assuming that their radiative intensity is high enough to exceed the higher detection threshold at larger scan angles. Also, in some regions (such as NHAF and SHAF) fires exhibit a strong diurnal cycle (Giglio et al. 2006). The detection rate of active fires will therefore be higher if the peak of diurnal intensity is synchronized with satellite overpass. However, we can expect the sampling error rate and the variation of FRP sensitivity with latitude to be more homogeneous within each GFED regions that at global scale."

Please find our point-by-point answers to specific comments in the following.

1) l.17: thresholds differ between regions: what defines the regions? climate, humans, vegetation types?

Answer: We meant GFED regions. As stated in our main answer to the review, we also separate each GFED regions in different biomes (see Figure 3). We have added in the abstract that the relationship changes with the region and the considered vegetation type.

2) l. 20: seasonal effects, could there be an influence of anthropogenic fire use too? Percolation theory explains why fires are most intense or why fires are smaller in the

late season? I guess the latter.

Answer: Yes. For example, in the discussion (l. 252), we mention the use of prescribed burning at the beginning of the fire season in Africa to limit fire size. Concerning the percolation theory, the sentence in the abstract was not clear enough. Indeed, the term "this effect" was referring to the decrease of fire size toward the end of the season. We have modified the text to make it clearer.

3) l. 25-27: not sure I agree 100% with the reasoning: fire models have been included before in DGVMs, for instance Arora and Boer (2005). I think the reason was more the strong impact on vegetation and overestimation of tree cover in savannas in many DGVMs.

Answer: We agree. We removed 'As a result' from the sentence.

4) l. 28: prediction of vegetation dynamics and the carbon cycle.

Answer: The text has been changed.

5) l 47: also the impact of fire varies with the size, the fire size characteristics therefore could be more informative than only burned area.

Answer: Yes, we agree that the shape of the fire can have an effect on its impact on the vegetation. We have added a couple of references (Greene et al. 2005, Cary et al. 2009), and we have added a sentence in the text to mention this effect.

6) l. 52: maybe drivers of propagation and ignition are not driven by the same climate variables, but the fraction of ignitions turning into fires is determined by similar drivers, burned area and fire counts therefore have quite similar spatial patterns.

Answer: We agree, but in this paragraph, we do not discuss/compare Burned Area to fire counts. We just stated that separating ignitions from propagation would bring more information than just using BA or fire counts. However, this comment is related to comment 14 (where we actually compared fire counts and fire ignition), which brought some modification in the manuscript.

7) l.57: is it fire intensity of fire line intensity? and based on the equations this is not expected for large fire size? or do you mean the Rothermel equations were only tested for small scale (laboratory to stand scale) and it is unclear whether the equations hold true for larger scales (not for larger fires).

Answer: Actually, this is reaction intensity of the fire front. We meant that Rothermel's equation was only tested at local scale, as stated at l. 38-39. We have modified the text to recall this on l. 57.

8) l. 95: explain the difference between fire intensity and fire radiative power. Answer: We will provide a more accurate definition of fire (reaction) intensity and fire radiative power when the terms are introduced in the manuscripts (i.e., at line 57 for fire intensity and line 85 for FRP). Following the received comments, we have decided to focus on FRP throughout the presentation of the results, and introduced the use of FRP as proxy of FI in the interpretation of the result only. The limitation of this hypothesis is discussed in the discussion section.

We added also some references on the use of FRP as a proxy for several fire severity applications to justify the use of this index in our analysis:

-field work to estimate fire intensity on fire severity and impact on soil: Barret & kasischke 2013, Sparks et al. 2018 (in biogeosciences).

-fire risk modelling of fire size and intensity based on FRP: Hernandez et al. (2015)

-biomass combustion rates from FRP in Africa: Roberts et al. (2005)

-relating a fire spread equation (Byram) to fire intensity from infrared remote sensing: Johnson et al. (2017)

9) l. 96: are there any spatial or temporal patterns in the the discarded fire patches ? This might indicate biases in the FRP detection.

Answer: Yes, there are some spatial patterns. We now discuss this in the methodology and discussion section, and we have added a couple of supplementary plots with :

- a map of the ratio of missed matches between fire patches and active fire pixel data.

- a histogram showing the global fire size distribution of fire patches and the distribution of fire patches without recovered active fire information.

10) l.110: text says median, figure caption says mean. The figure could also include a burned area map to show that the patterns are different, between the characteristics.

Answer: This was a typo. We have added a map of yearly burned area over the same type period as MCD64A1, and briefly discuss the difference with fire count in the text.

11) l.112: patterns of size and FRP look not so similar to me.

Answer: We have modified the text, and try to provide a more accurate description of the figure.

12) l. 117: use either mean or average.

Answer: This was a typo. We kept 'average'.

13) l.119: could this peak simply be because lower intensity is simply not detected by the satellite. What is the explanation for this peak at intermediate fire intensities?

Answer: Thresholds of FRP detection vary between 9 and 11 MW (Schroeder et al. 2010, Roberts and Wooster 2008) for MOD14 and MYD14, below which no data are available. In turn, remotely sensed finer resolution analysis actually concluded that MOD14 may underestimate by 20% captured fire pixels, particularly for small fires (Wooster et al. 2012, Peterson et al. 2013). Beside spatial resolution, different sensors can differentially capture FRP (Li et al. 2018) due to solar angle and vegetation types. The 9-11MW threshold falls in the 1st bin of FRP in Figure 2, and could therefore explain the peak at intermediate FRP.

We are now discussing this in the manuscript (in the result and discussion section), and we have added the aforementioned references.

14) l.121: change "number individual.." to " number of individual..." I assume fire counts is related more closely to burned area as two counts could be individual fires or the same fire, so some differences are also expected.

Answer: We agree that active fire counts are closer to burn area than individual number of fire patches. We removed the sentence from the text.

15) l.124: It would be useful to consistently use FI or FRP, now it is FI in the text and FRP in the figure.

Answer: Following the comments of the reviewers, we choose to use FRP throughout the text. We agree that we interpret FRP as a proxy of FI, not directly as FI itself. Moreover, we added a paragraph in the discussion about the differences between reaction intensity and FRP (see our main answer).

16) l. 125-28: I don't see that the fire size is clearly decreasing. it is a bit tempting also to interpret the error bars as error bars. Maybe having three lines for 25th percentile, median 75th percentile could avoid that misunderstanding. Probably showing the 4th order polynomial with uncertainty bands could give a better impression whether decreases and maximum are robust. My confidence based on the plots shown is rather low, and now these threshold become quite important for the following discussion. Showing some kind of robustness and uncertainty on this threshold would therefore be important.

Answer: The problem with lines is that it makes it difficult to represent the burn date information encoded with the dot/colorbar color, which is an important information for the discussion. We prefer to keep the plot as it is now. However we have added the interpolated polynomial "under" the dot, to show the humped relationship of the median fire size wrt FRP. Also, please note that the large range of fire size (due to the

75th quantiles, 5 to 10 times bigger to median fire size) render difficult the sight of the decrease after the threshold is reached.

We also realized that the description of our methodology was not accurate enough. First, we only fit the median value of FRP, not the total distribution. The polynomial fit is only used to smooth the maximum median FRP value from the FRP vs FS relationship. We then perform two linear regression : one in the range [FRP > 0, FRP < FRPmax], one in the range [FRP > FRPmax, FRP < 300]. We obtain uncertainties (with their correlation) on each of the 5 parameters of the polynomial fit, and the uncertainty on the each slope of the linear regression. The uncertainty on the parameters of the polynomial are low (less than 1% in terms of relative uncertainties for all parameters). Similarly, the relative error on the slope fitted over the range [FRP > FRPmax, FRP < 300] is lower than 1%.

17) l. 174: higher FI threshold for forests: can this be explained by Rothermel?

Answer: Rothermel only explains the expected linear relationship between fire size and fire intensity. A higher FRP threshold simply means that Rothermel s equation is valid on a larger range of FRP, and we suggest that this could arise from the fuel array continuity.

18) l. 176-7: Rothermel also uses different parameters for different vegetation types and fuel moisture and is able to reproduce the varying constraint hypothesis.

Answer: Yes, Rothermel is able to simulate the varying constraint hypothesis. but we show that Rothermel is no more valid (or highly affected by another factor; potentially landscape fuel continuity) for high intensity fires. For example, late fire season in Africa is dry and not fuel-limited so this season should experience the highest FS as a function of FI. We believe this paragraph is not fully related to our results, so, for clarity of the manuscript and regarding the comment of the reviewer, we removed the paragraph concerning this comment.

19) l.185: GFED regions are not biomes

Answer: This is true. We have now separated each GFED regions in different biomes using Land Cover data from MODIS. See our main answer to the review.

20) l.195: so you expect a lower threshold for higher fragmentation? is that what you find in your analysis?

Answer: We fully rephrased and developed this sentence. We mention here that landscapes can be intrinsically fragmented (by roads, or cropland mosaic), and seasonally fragmented by successive fires. In turn, successive fires can interact at the landscape scale, so the edge of the first fire acts as a barrier for the second fire propagation (Teske et al. 2012). In savannas, patchy mosaics of burned land are then intentionnaly created early in the fire season as a preventative strategy for large fires emerging later in the season (Laris et al. 2002). This sentence is part of the discussion and we propose here an hypothesis on our findings of the FS/FRP threshold.

21) l.215: I would expect a very high fragmentation in EURO and TENA (lots of big streets) and strongly managed, which is why fire models usually overestimate burned area there. Is this only meant for interpreting the seasonality? so no fragmentation due to burning?

Answer: In these regions, despite being highly populated and urbanized, burned area has been shown to be mostly driven by weather and anthropogenic process rather than landscape fragmentation (cf figure 9 in Le Page et al. (2015)), a result supported by our findings. We added this reference in the manuscript.

22) l.220: not all the tropics has rainfall all year long. Answer: We agree. We have changed the text to 'where drought period are shorter'

23) l. 221: are you suggesting that the savanna species suppress fire? burned are is much higher in savannas than in TENA and BOAS. I don't understand the logic here.

Answer: We are speaking about propagation once a fire has started. Our results show

that fires are larger in forests of TENA/BONA than in savannas. However the number of individual fire patches is much lower in TENA/BONA, which results in a lower burn area.

In BONA/TENA, once a fire has started (mostly on the ground layer), they turn into crown fires which are hardly controlled, while grassland fires can be more easily stopped by changes in weather or landscape obstacles. We rephrased the sentence in the manuscript:

'They can therefore propagate further than ground fire and fire resistant species found in savannas and woodlands in semi-arid tropical regions' => 'they can therefore propagate further than herbaceous fires hardly turning into crown fires in savannas and woodlands in semi arid tropical regions.'

24) l. 223: vegetation is less flammable where?

Answer: In BOAS. We have modified the text.

25) l. 237: FDI increased everywhere?

Answer: Rather than FDI, the article actually focus on fire season length. This has been obsereved in some regions only. We have changed the text.

26) l.251: agricultural expansion leads to a reduction of burnable area. why? Croplands are burned, pastures are burned. Also the more fragmented landscape, is there a study showing that the landscape is more fragmented. I is an assumption in models and to explain the decline in burned area. Give a reference where this fragmentation is observed, or identify it as a common assumption. Could this be an effect of having smaller fires in croplands and therefore the detectable burned area is declining, not the burned area itself?

Answer: We now provide in the manuscript some references illustrating the fragmentation of landscapes in savannas worldwide :

- Sulieman, HM. 2018. Exploring drivers of forest degradation and fragmentation in sudan: the case of Erawashda forest and its surrounding community. Science of the total environment. 621: 895-904.

- Oliveira SN, de Carvalho OA, Gomes RAT, Guimaraes RF, McManus CM. 2017. Landscape-fragmentation change due to recent agricultural expansion in the Brazilian Savanna, Western Bahia, Brazil. regional environmental change 17(2): 411-423

- Kamusoko C., Aniya M. 2007. Land use/cover change and landscape fragmentation analysis in the Bindura District, Zimbabwe . Land degradation and development 18(2): 221-223

27) l. 259: BA saturates toward the end of the drought season: is this really reproduced by models? Any reference?

Answer: We did not find any references about this, but this mechanism looks realistic in regions with high BA (NHAF/SHAF) looking at the equations from Thonicke et al. We rephrase the sentence : "Because of the reduction of the available fuel load due to burning by preceding fires, we can expect than BA saturates toward the end of the drought season in DGVMs."

28) l. 267: fire-prone ecosystems: actually you didn't group the analysis by ecosystmes, for the tropical regions you group everything together, tropical rainforest and savannas are not separated. I think it would be smarter to group the data for this analysis based on vegetation and climatic parameters not by geographical regions. grouping high and low tree density together could confound the results.

Answer: We agree. See our main answer to the reviewer's comment.

29) l. 271: FI threshold is driven by biomass and drought severity: Most of the regions have a strong variation of biomass and drought severity. It therefore would be better to use drought severity and biomass to group the data.

Answer: See main answer to the reviewer's comment. We suggest to divide each

region depending on their land cover.

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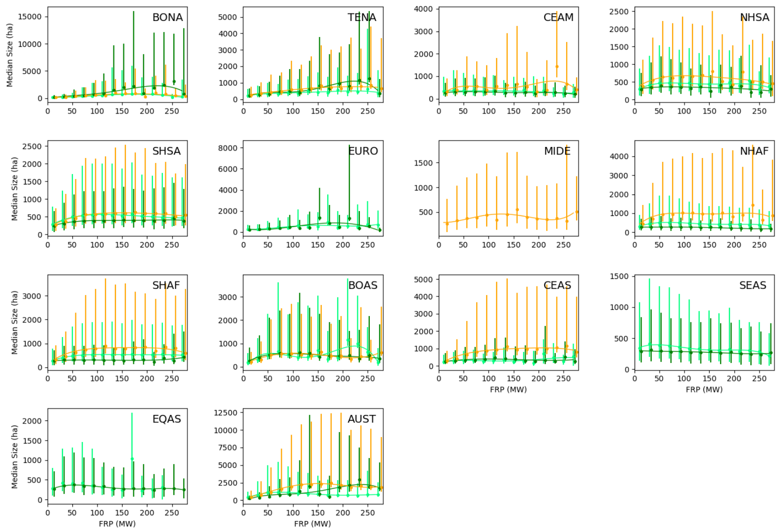

Fig. 1. FRP vs fire size for different biomes

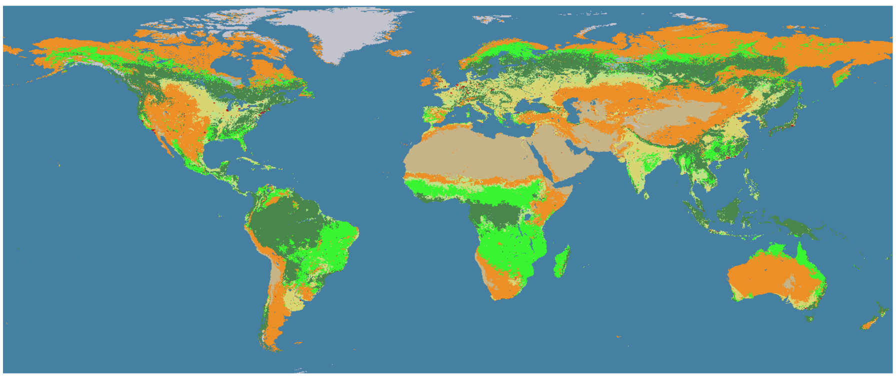

Fig. 2. Map of the land cover biomes