

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

Anonymous Referee #1

Received and published: 13 September 2018

Review of Laurant et al.

Laurant et al. provide a novel data analysis based on a recently published database of fire parameters derived from remote sensing data. So far mostly burned area data were investigated to understand the global "occurrence" of fire. However burned area is not always the most informative and important parameter, fire size and fire intensity might better capture the impact of the specific fire event on ecosystem and society. The work presented here is therefore an important step. I have a couple of comments, most of them minor, that will hopefully help to improve the manuscript, but I also have two major concerns:

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

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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.

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.

Specific comments:

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

I. 20: seasonal effects, could there be an influence of anthropogenic fire use too?

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Percolation theory explains why fires are most intense of why fires are smaller in the late season? I guess the latter.

I. 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.

I. 28: prediction of vegetation dynamics and the carbon cycle.

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

I. 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.

I.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).

I. 95: explain the difference between fire intensity and fire radiative power.

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

I.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.

I.112: patterns of size and FRP look not so similar to me.

I. 117: use either mean or average.

I.119: could this peak simply be because lower intensity is simply not detected by the

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satellite. What is the explanation for this peak at intermediate fire intensities?

I.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.

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

I. 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.

I. 174: higher FI threshold for forests: can this be explained by Rothermel?

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

I.185: GFED regions are not biomes

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

I.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?

I.220: not all the tropics has rainfall all year long.

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

I. 223: vegetation is less flammable where?

I. 237: FDI increased everywhere?

I.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 am 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?

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

I. 267: fire-prone ecosystems: actually you didn't group the analysis by ecosystems, 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.

I. 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.

Interactive comment on Biogeosciences Discuss., <https://doi.org/10.5194/bg-2018-334>, 2018.

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