Biogeosciences Discuss.,
https://doi.org/10.5194/bg-2018-427-AC2, 2018

Interactive comment on "Emergent relationships on burned area in global satellite observations and fire-enabled vegetation models" *by* Matthias Forkel et al.

Matthias Forkel et al.

matthias.forkel@geo.tuwien.ac.at

Received and published: 4 December 2018

We thank the referee for the positive review. In the following we cite comments by the referee and provide our responses in normal font.

[Referee 2: "In the abstract (ln 27) you mention "Recent climate changes increases fire-prone weather conditions and likely affects fire occurrence". That is not happening every- where in the globe. Please make clear if that's the case globally or regionally, or in certain latitudes ect."]

The referee it right. Increases in fire-prone weather conditions (estimated as changes fire weather season length, 1979-2013) occur in 25% of the global vegetated area, especially in eastern and southern Europe, the Mediterranean, Mongolia, and Eastern China, central and eastern Africa, Alaska, the USA and Mexica, and in central and eastern Brazil and Argentina (Jolly et al., 2015). Many regions show no significant changes in fire weather season length and some regions (10.7%) show declines in fire weather season length (e.g. western Africa, parts of Central Asia) (Jolly et al., 2015). We changed the sentence:

“Recent climate changes increases fire-prone weather conditions in many regions and likely affects fire occurrence”

[Referee 2: “ Ln 10: Regionally (Randerson et al., 2006), but also globally (Lopez-Saldaña et al., 2015 : https://www.biogeosciences.net/12/557/2015/)]

Thank you for this interesting reference. We changed the sentence: “Fire affects global and regional and climate directly through changing surface albedo (López-Saldaña et al., 2015; Randerson et al., 2006)"

[Referee 2: “ Ln 14: you can add Veraverbeke et al., 2016 (https://www.nature.com/articles/nclimate3329), for changes in lightning ignitions.]

We added the reference.

[Referee 2: “Pg 3 – ln 8 and ln 10: “. . .drivers of fire activity”. There has always been confusion on what fire activity, or fire incidence really represents. Do you study drivers of fire activity, or drivers of burned area?]

We replaced “fire activity” with “burned area”.

[Referee 2: “Pg 5 – ln 20: I Suspect there might be some uncertainty from aggregating different model resolutions. Could please comment on that? For example, type of aggregation (nearest neighbor ect), latitude/area correction ect."]

We added the following sentences: “Aggregation was done by averaging the fractional

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burned area from all high-resolution grid cells that belong to the same coarse-resolution grid cell. Nearest neighbour resampling was done if less than two high-resolution grid cells were within one coarse-resolution grid cell."

[Referee 2: "Pg 7 - ln 15: Why a Spearman correlation of >0.25 is considered as good? Because >0.25 agreement in spatial patterns is acceptable? Most of the BA is happening in Africa anyway and this is an area that burns frequently and therefore most products and models mostly agree. But that also means that more sparse events (boreal fires) show a very low agreement. So, is that what you mean? That given the fact that the very general patterns are described, a >0.25 correlation is a good correlation?"]

The Spearman correlation between simulated and observed burned area was computed per grid cell (not across the spatial patterns). It is easier for the models to reach a higher correlation in regions with frequent fires that have a clear seasonality of burned area such as African Savannahs than in regions with infrequent burning such as boreal forests. Hence we used such a low threshold of the correlation coefficient to define a "good" correlation because this shows at least that a model has a positive relationship with observed burned area. For regional studies with frequent burning a higher threshold of the correlation would be more appropriate. We changed the sentence to: "good agreement for an individual grid cell was defined based on a positive and non-random relationship (i.e. Cor $\geq$ 0.25) and a comparable variance (-0.75 $\leq$ FV $\leq$ 0.75) between simulated and observed burned area."

[Referee 2: "Pg 7 – ln 16: You do not describe how do you treat your data priory to the analysis. Data like population density are extremely skewed. Did you apply any transformation to the data, checked for outliers, false alarms ect?"]

We did not apply any data transformations such as normalization or scaling because the random forest algorithm works also with non-normal distributed data. We applied quality checks to the burned area data as described in chapter 2.2.

[Referee 2: " Pg 8 – ln 26: The GDP data, especially in areas of high fire activity (Africa) are based in country averages. In this case it is weighted by the pop. density. That means that it is eventually following the variability of the pop. density. Did you see this effect, and if yes, did you take it into account?"]

The GDP data was normalized by population density (GDP per capita). Hence any population density-related variability that was included in the original GDP dataset should be taken into account. We added in Table A1 that GDP is per capita.

[Referee 2: "Figure 3: I feel it is a bit misleading to say that vast areas in temperate and bo- real regions in north hemisphere are temperature driven. Essentially, this means fuel moisture variability and the monthly effect of moisture conditions rather than extreme temperatures (of DTR) that might last only few days. You mentions this somehow in pg15-ln5 and pg19-ln20 on, but please make this a bit more clear in the discussion if it is the case or not.. "]

We did not write that fire "in temperate and boreal regions (. . .) are temperature driven". This reading of our manuscript likely emerged from the maps in Figure 3 and the associated text where we only describe the most important predictor in the random forest model but do not provide information on the ranking of the other predictors. We did the following changes to avoid such an interpretation:

- In the caption of Figure 3, we added: "Please note that the predicted burned area in random forest (and in reality) emerges from multiple predictors and that the second-most important predictor (not shown in the maps) might have similar importance."

- We changed the sentence at page 19, line 20 to "Fire results from an interplay of several meteorological variables, thereby maximum temperature was an important predictor globally and especially in northern temperate and boreal ecosystems."

[Referee 2: "The take home message (pg21-ln12), perhaps the most important after reading the manuscript, was that ". . . we identified vegetation effects on fire as a main deficiency of fire-enabled dynamic global vegetation models in simulating temporal dynamics of burned area." It would be great if that would be more highlighted in the discussion."]

To better highlight this take-home message, we made the following changes:

- We added the following sentence in the beginning of chapter 4.3. "Our results demonstrate that the role of vegetation on fire needs to be better represented in fire-enabled DGVMs to accurately simulate the variability of burned area."

- We changed the last sentence of the abstract: "Hence our pattern-oriented model evaluation approach allowed us to diagnose that vegetation effects on fire are a main deficiency of fire-enabled dynamic global vegetation models to accurately simulate the role of fire under global environmental change."

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