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Biogeosciences Discussions

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

Anonymous Referee #2

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The paper is a welcome development mostly because it largely highlights the inadequacies of DGVMs, but also recommends necessary changes. The section with recommendations for specific changes in DGVMs, backed up with the current findings, make this paper novel and very useful for the modeling and the fire data community. However, the analysis is mostly confirming prior findings on drivers of burned area at global scale. One additional take home message, perhaps the most important after reading the manuscript, was that "besides human-fire interactions, we identified vegetation effects on fire as a main deficiency of fire-enabled dynamic global vegetation models in simulating temporal dynamics of burned area."

Specific Comments

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

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

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

- 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?

- 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.

- 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?

- 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?

- 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?

- Figure 3: I feel it is a bit misleading to say that vast areas in temperate and boreal 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..

- 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.

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