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Interactive comment on "A hydroclimatic model for the distribution of fire on Earth" *by* Matthias M. Boer et al.

Anonymous Referee #2

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Review for Boer et al: A hydroclimatic model for the distribution of fire on Earth Summary: The authors present a hydroclimatic model to estimate potential maximum burnt area across climate space. They link the outcome of this model to fuel vs moisture limited fire regimes. Overall, this is a nice and coherent manuscript presenting interesting results using a sound methodology. The authors can find some minor comments below, which might help to further improve and/or clarify the manuscript.

Main comments

The interpretation of F99 is a bit hard to imagine. I kind of interpret it as the maximum an area can potentially burn considering the mean climatic conditions, but it would be nice if the authors could indicate what they think is the best interpretation of F99 so that the reader doesn't need to imagine this. This would especially help for people who

don't have the time to read the methods section to understand what F99 actually is (when one starts reading it is confusing what F99 actually is).

Related to this topic, I think the discussion covers some interesting topics, but I miss some depth in how we could use this F99 estimate to improve our understanding of the drivers of global fire activity beyond the results of the paper. E.g. difference between F and F99 can indicate human impact, but can also differences in vegetation type, structure and traits under similar climate conditions, and can possibly guide us to explain some of the continental differences observed in burnt area (e.g. Lehmann et al., 2014).

I think the methods are sound and the results overall robust. However, I have some doubt for areas with very long fire return intervals such as the boreal region. In these areas, large fires result in very high burn fractions within a 0.25° gridcell for a given year, which you then divide by the length to the time series to get your F. However, doesn't this mean that your F99 will depend on the length of the time series used for these regions with long fire return intervals and large fire sizes? I point this out because you use the GFED data from 1995, but the pre-MODIS data is much less reliable, so I would suggest to only use the MODIS era data (just a suggestion if it is not too much trouble).

When looking globally at burnt area, and especially at extremes, one tends to only see Africa, which has so much burnt area than any other continent that it tends to completely dominate any analysis. In your methods you implement a bootstrapping, but I do wonder how different your F99 estimates would look without Africa (or the other direction, how much does it matter to include the rest of the world in the analysis?). This is always a problem, and no criticism, but for interpretation of the results it could be nice to know this kind of "uncertainty".

Minor comments

L8: maybe add human, as population density is supposed to be a good indicator of ignitions and suppression.

L12-13: 99 percentile over? Time/space? It should be explicitly indicated how this is calculated. After reading the methods section and your previous manuscript over Australia I notice that it is a 99 percentile quantile regression, you should make this clearer in the sections which come before the methods (and possibly even for the results for people who don't want to go over the methods to interpret the results).

L79-80: P and Eo are not yet defined.

L111-113: Does it matter that P and Eo come from different sources, e.g. a physical disconnection between both could influence your estimates for D?

L212-214: I think this separation between production and dryness is nice. However, shouldn't there be a precipitation level where the default it is fuel limited? E.g. NPP is very low and almost exclusively limited by precipitation under very dry conditions. Now it seems that under even very low precipitation values it can you still be moisture limited?

L275: For a quantification of the spatial uncertainty in fire models you might be interested in this recent paper: https://www.geosci-model-dev-discuss.net/gmd-2019-261/

L290-294: I do follow the logic in using D for multiannual mean fire conditions (and I agree that the Nesterov Index is suboptimal), I don't see the logic in comparing predictions which are generally made at (sub)daily timesteps to your multiannual average F(99) estimates. These seem to be two pretty disconnected things and I don't see how you could use D for these short timestep responses in burnt area.

L310: Another comment about regarding this dichotomy between fuel and dryness, there was a recent paper by Alvarado et al which investigated this across the tropics and found important differences between continents, but only looking at precipitation. I wondered whether these results could explain these differences by looking to over an aridity gradient?

References

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