#2 Response to : Interactive comment on "A hydroclimatic model for the distribution of fire on Earth" by Boer et al.

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.

Thank you for your constructive comments (black font). Below we have added our responses to each individual comment in blue font.

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).

Response: In the introduction (L. 62-75) we defined F as the mean annual fractional burned area. That is, the cumulative burned area recorded over the 1995-2016 GFED observation period divided by the number of observation years (i.e. 20). $F_{0.99}$ is the 0.99 quantile value of F. We modelled $F_{0.99}$ as a function of two hydroclimatic variables: mean annual precipitation, P, and potential evapotranspiration, E_0. $F_{0.99}$ can thus be interpreted as the maximum or potential value of the mean annual fractional burned area (F) for a given set of hydroclimatic conditions. We also added a reference (L. 65) to an introductory paper on quantile regression and its applications in ecology (Cade et al. 2003)

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).

Response: Our model is built on the 4-switch concept (Bradstock, 2010. doi.org/10.1111/j.1466-8238.2009.00512.x), which describes biogeographical variation in fire activity as a function of a hierarchy of fundamental constraints (i.e. the 4 switches or limiting factors). We focused on the two primary constraints, fuel production and desiccation, and proposed a hydroclimatic work to model their effect on global burned area patterns. We demonstrated that climatic constraints on fuel production and desiccation explain circa 80% of global variation in potential mean annual burned area.

As mentioned in our conclusions, the predicted $F_{0.99}$ and the difference between $F_{0.99}$ and F provide useful starting points for investigating the extent to which human activity could alter fire activity levels by manipulating ignition regimes or vegetation. This will be subject of future studies.

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).

Response: The reviewer suggests that the estimates of the mean annual fractional burned area (F), or of the 0.99 quantile of F ($F_{0.99}$), in environments characterized by rare but large fires, and long fire return intervals may depend on the length of the observation period.

We agree that the uncertainty in estimates of F and $F_{0.99}$ for any particular grid cell will decrease with the length of the observation period, in particular for grid cells with relatively long fire return intervals.

However, our hydroclimatic model predicts $F_{0.99}$ for combinations of mean annual precipitation (P) and potential evapotranspiration (E_0). As explained in L. 151-153, the quantile regression model was fitted to binned E, D data and we only used E, D bins with a minimum of 100 observations (i.e. grid cells). This means that the estimate of $F_{0.99}$ for a particular E, D bin was based on a sample of at least 100 20-year observation periods, which should provide a robust estimate of $F_{0.99}$, even for climates characterised by long fire return intervals, as long as fire events are a stationary process.

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".

Response: As explained in section 2.3.1. (L. 147-155) our bootstrapping approach was designed to ensure that we sampled evenly across hydroclimatic space, thus avoiding dominance of observations from tropical savannas in Africa and other continents where much of global fire occurs. We have added some clarification on this point to the Discussion (L. 191-192): "Our model fitting approach, which sought to avoid dominance of the most common fire-prone environments on Earth such as tropical savannas, will have contributed to the relatively good performance of the model across a broad range of hydroclimatic conditions."

Minor comments

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

Response: L8 has been adapted, now referring to human activity.

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).

Response: The introduction provides a detailed definition of $F_{0.99}$ (see L. 62-75)

L79-80: P and Eo are not yet defined.
Response: P and E_0. are defined in L.66-67.

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?
Response: At very low mean annual P and E_0 we observe very low levels of fire activity and the fitted response surface (Fig. 1C) is therefore relatively flat which implies that the position of the boundary between the two domains is relatively uncertain.
We do not expect areas of (very) low P to fall by default in the domain of PL-type fire, because that depends on the level of E_0. Even at low mean annual P, sites can be humid if mean annual potential evapotranspiration is very low too due to cold temperature, low radiation inputs, etc.

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/
Response: Thanks, noted.

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.
Response: In the Discussion section (L. 296-323) we are pointing out that the Nesterov Index is a poor predictor of fuel moisture content and suggesting that this could be a reason for poor performance by some of the existing global fire models in forest biomes where fire is primarily constrained by the frequency and duration of dry fuel periods. We did not suggest that the mean annual climatic water deficit would be an option to model daily fuel moisture conditions in global fire models. However, we do show in Supplementary Material S4 that mean annual climatic water deficit is strongly and linearly related to the mean annual frequency of days of predicted dead fuel moisture (DFMC) below 10%. We have added a statement (L. 307-310) that the DFMC model we used for that analysis in Supplementary Material S4 (Resco De Dios et al. 2015, doi.org/10.1016/j.agrformet.2015.01.002) would be an alternative to the Nesterev Index for modelling daily fuel moisture content in global fire models.

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?
Response: Alvarado et al. (2019, 10.1111/geb.13034) based their classification of fire regimes in tropical savannas and grasslands being fuel- or moisture-limited entirely on whether sites had a positive or negative relationship between interannual variation in burned area and precipitation. Their approach did not lead to a clear dichotomy or mapping of fuel- and moisture limited fire regime as areas of positive and negative relationships with precipitation were found to occur within the same biome. We agree with the reviewer that the underlying reason for Alvarado et al.'s lack of separation between fuel- and moisture-limited fire regimes is likely because they did not take variation in potential evapotranspiration in to account. As shown in our Figure 1D, potential mean annual burned area ($F_{0.99}$) can either increase or

decrease with mean annual precipitation depending on the level of mean annual potential evapotranspiration. This shows that the dichotomy between fuel production and fuel dryness limitations is a function of the combination of climatic availability of water and energy.