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

General Comments: The authors present a new approach for modeling a measure of fire activity (maximum annual burned area) using a water-balance approach. Through the use of two variables that are linked through energy-water dynamics, they do a nice job using E (a measure of productivity) and D (a measure of drought stress) in elucidating fuel and flammability limited fire regimes globally under late 20th-century climate. There are a few areas where the work is weak and can be improved upon that I provide major considerations for below:

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

Major Considerations

1) I struggled to get a firm handle on the interpretation of F99. I believe it represents some maximum mean annual burned area, but I may be interpreting it (in my head) as the maximum burned area in one year. If referring to the former, it is possible that the results may impact the use of the Budyko curve. The Budyko curve partitioning E and D from P/PET; in areas with very high mean annual burning one might expect substantial departures from the Budyko curve as frequent fire would strongly shape both biomass abundance and soil properties. Better clarification on the exact variable of interest and why it is of value in global fire analysis would be helpful to myself and likely other readers.

Response: We appreciate that readers may not be familiar with quantile regression and have therefore added a citation of an introductory paper on the topic (Cade et al. 2003. https://doi.org/10.1890/1540-9295(2003)001[0412:AGITQR]2.0.CO;2) in Line 65 where we introduce $F_{0.99}$.

In the introduction we defined F as the mean annual fractional burned area. This is computed from 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. Therefore, $F_{0.99}$ is a prediction of the potential mean annual burned area as a fraction of a 0.25° x 0.25° grid cell and does not represent the maximum [fractional] burned area in one year.

In our view, the global model of $F_{0.99}$ contributes to the foundation of pyrogeography by providing a simple yet robust prediction of global variation in mean annual burned area as a function of the two most fundamental environmental limitations on fire, namely the availability of energy and water for the production and (seasonal) desiccation of fuels.

The Budyko curve provides a prediction of the mean annual partitioning of precipitation in streamflow and evaporation for large catchments or land areas as a function of the aridity index (E_0/P). Indeed, many variations of the original Budyko curve have been proposed to account for effects of, for example, climate seasonality, vegetation and soil types, and topography on the long-term water balance. Several studies have evaluated the effect of fire on catchment water balances using the Budyko framework (e.g. Roderick and Farquhar, 2011. doi:10.1029/2010WR009826) but we are not aware of any studies that have incorporated effects of particular fire regimes in the Budyko framework. However, as shown in our study and many others, very high mean annual fractional burned area is limited to seasonally wet/dry (sub)tropical climates that combine high fuel production rates with strong desiccation of fuels in the dry season. In these highly seasonal environments, we don't expect a strong effect of the mean annual burned area fraction on the mean annual partitioning of precipitation in streamflow and evaporation, because the fires burn in the dry season and mainly affect dry grassy fuels that grow back in the following wet season.

2) The Pyromes data from Archibald et al. doesn't seem to provide much value in the current study. There are problems with the pyrome layer that make it hard to understand (e.g., constructed from a short record and biased by the spatial unit of analysis where you have adjacent pixels with similar veg/climate that get classified to different pyromes); do the authors think that maximum annual burned area should be part of what defines a pyrome, or through the lens of those defined? If not, it is a bit of a fishing expedition. For example, in Figure 4 there are strange non-contiguous regions (e.g., ICS spanning huge region both fuel and flammability-limited). I believe this is a limitation/problem with the fire regimes, not the methodology of the current paper. Since there are no very strong arguments to expect F99 to map onto these pyromes, I would suggest analyses relating to pyromes to not be necessary.

Response: We do not suggest that $F_{0.99}$ should be incorporated in the pyrome classification. This would not make sense as the pyrome classification is based on actually observed fire regime metrics, while our predicted $F_{0.99}$ is a prediction of the potential mean annual fractional burned area for a given combination of mean annual precipitation and potential evapotranspiration.
However, we do believe there is interest in exploring how current fire regime syndromes (i.e. pyromes) map to the hydroclimatically defined domains of PL- and DL-type fire and to the predicted potential mean annual burned area ($F_{0.99}$). We expected most of the pyrome classes to be limited to either the PL domain or DL domain, and the range of predicted $F_{0.99}$ to be consistent with the observed values of annual burned area and fire return interval for each pyrome. As presented in the last paragraph of our Results section (L. 249-268) and visualized in Figure 4, three out of five pyromes (i.e. FIL, RIL, RCS) predominantly occupy one of the two domains in accordance with Archibald et al's description, while the pyrome with the strongest human influence according to Archibald et al. (ICS) occurs across both PL and DL domains. The FCS pyrome, which is observed in tropical and temperate grasslands as well as a range of tropical forest biomes (see Table 1 in Archibald et al.), is also spread across the two domains, as expected.

3) The Budyko curve is a generalization of the partitioning of precipitation into runoff and actual evapotranspiration. However, there are numerous studies that show that there are substantial deviations from this curve that materialize due to vegetation, soil water holding capacity, the seasonal synchronicity of P and PET, and precipitation phase. At the very least it is worth acknowledging this and how it may impact estimates of E. reference to this as it will impact your estimates of E. At the most, the authors may also consider using gridded modeled estimate of E and D that are available globally at spatial resolutions sufficient for the penultimate scale of GFED.

Response: The Budyko framework is widely accepted as a reasonable global model for the partitioning of precipitation in runoff and evapotranspiration for large catchments or land areas (e.g. Wang et al., 2016, DOI: 10.1177/0309133315620997). It thus provides a reasonable approach to modelling global variation in mean annual actual evapotranspiration from two input variables, mean annual precipitation and potential evapotranspiration, that are available globally at adequate spatial resolution. We acknowledge the point that local climate, soil, vegetation and terrain can cause mean annual actual evapotranspiration to deviate from the estimate predicted by the Budyko curve. We do not believe we introduced any particular bias in our hydroclimatic model of global fire patterns by using the Budyko curve to predict mean annual actual evapotranspiration. Such deviations are to be expected for any highly simplified global model, but discussion of underlying causes and/or proposing alternative forms of the Budyko curve are beyond the scope of our study. We now provide a description of the terms in equation 1 and refer (L. 91-102) to the abovementioned paper by Wang et al (2016) as a source for further details on the assumptions underlying the Budyko framework, as well as a review of applications in hydrology and hydroclimatology.

Minor considerations

4) Line 33, This sentence is unclear "The partial success in current models: : :", please rephrase or clarify.

Response: We have substituted "The partial success of current models" to "The limited ability of current models" (L. 35) to refer to the statement in the previous sentence that current models can reproduce observed fire activity patterns in some environments but not in others.

5) Eq 2 & 3, How do your results compare using these parametric equations to a completely non-parametric approach?

Response: Of course, there are alternative models/equations to describe the relationship between fractional burned area and (hydro-)climatic predictor variables. We opted for a parametric model because we know from theory and previous work what shape the relationship between burned area and E or D is likely to have. In our previous study on Australian fire regimes (Boer et al. 2016, doi.org/10.1088/1748-9326/11/6/065002) we showed that a logistic or other sigmoidal function provides good fits for the effects of E and D on burned area; the form of the logistic model is consistent with the 4-switch concept underlying our hydroclimatic model (i.e. simulating a threshold response in burned area once E or D exceeds a given level, i.e. a limiting factor is overcome. The logistic model proposed by Yin et al. 2003 provides a good level of flexibility with two parameters that can be interpreted in biophysical terms of how climate constrains fire activity.

6) Line 105, The GFED data for the first several years of record is highly suspect due to inhomogeneities in data source. I would repeat the analysis using only GFED from the MODIS-only era. Also, do your results differ if you include small fires GFED (GFED 4s?)

Response: The GFED4 data base has been used for numerous studies on global fire, including (modelling) studies on climate-fire relationships (e.g. Abatzoglou et al. 2018, Global Change Biology, https://doi.org/10.1111/gcb.14405). The majority of the data base (2000-2016) was built from MODIS burned area products, which was extended with 5 years into the pre-MODIS era (1995-1999) by calibrating monthly active fire counts from the VIRS and ATSR sensors to MCD64A1 burned area data (Giglio et al. 2013, Biogeosciences 118). We have used the GFED4 data for its intended purpose as stated by Giglio et al. (2013): "As with previous versions of GFED, the data set is primarily intended for use within large-scale atmospheric and biogeochemical models and for interpreting regional and continental-scale controls on fire activity from climate and different forms of land management".

Finally, since we are not modelling temporal trends in burned area, but model mean annual burned area, potential errors resulting from the combination of more than one data source in GFED4 are likely to be small.

We did not use GFED4s, so cannot compare with GFED4 results. We note that including or excluding small fires is likely to be particularly important in agricultural areas, which were excluded from our analyses (see section 2.2.3.).

7) Line 114: I am unaware of WorldClim data covering this entire period, v1 is 1961- 1990 and v2 is 1971-2000; The documentation for the Eo data says 1950-2000, but I wonder if that is a misinterpretation as there is no WorldClim for this period.
Response: We used WorldClim1 for gridded mean annual precipitation, which according to Hijmans et al., 2005 (DOI: 10.1002/joc.1276) is based on a combination of data bases from the 1950-2000 period.
According to Zomer et al. 2008 (doi:10.1016/j.agee.2008.01.014), the CGIAR-CSI gridded mean annual potential evapotranspiration layer is based on 1960-1990 data. We have corrected section 2.2.2., L. 125-126 accordingly.

8) Line 120: Note that using the GFED database you can disaggregated burned area by MODIS land cover class and excluded agricultural burns. This might be a cleaner approach as there are many assumptions with interpolating MODIS land cover. Also note that there exist MODIS land cover classifications at 0.25 degree resolution to match GFED [https://ldas.gsfc.nasa.gov/gldas/vegetation-class-mask]
Response: We applied the MCD12Q1 Land Cover Type classification to exclude several non-native vegetation land cover classes as specified in section 2.2.3.

9) Line 223: A distinction here is that this approach clearly defines tundra as PL-type; whereas vegetation assembles or other fire regime classifications might define this as fuel limited.
Response: As shown in Figure 3 (which is the result of 1000 model fits and classification of P, E_0 space into domains of PL- and DL-type fire) homogenous areas of tundra vegetation were observed to have very high probability of classification in the domain of DL-type. The bar graph shows that probability, i.e. P(DL-type), to be mostly in the 0.8-1.0 interval. This is consistent with the wet conditions prevailing in many tundra environments. Figure 3 was created

10) Line 277: Is there a way to demonstrate that model skill does not degrade as a function of fire return interval, etc?
Response: Our model does not predict fire return intervals but potential mean annual fractional burned area ($F_{0.99}$), which can be interpreted as a measure of the multiplicative inverse of the (minimum) fire return interval. We have added a plot to Supplementary Material S1 of the standardized $F_{0.99}$ model residuals versus the fitted $F_{0.99}$ values; it shows that model skill does not degrade with predicted $F_{0.99}$.

11) Line 301: The numbers "14-18% and 10-12%, respectively" are hard to follow. Since I do not think the paper relies on them, I might omit them here but keep the citations.
Response: We specify the threshold values in predicted dead fuel moisture content (DFMC) from the cited studies to support our choice of using a DFMC=10% threshold to show that mean annual climatic water deficit is strongly related to the mean annual frequency of predicted daily DFMC dropping below 10% (Supplementary Material S4)

12) Figure 1: legend: replace grey dot with black
Response: Thank you. The grey dot has been replaced with a black dot.

13) Figure 2: I think this is stated in the discussion and probably a place for follow-up work, but it would be clearly of strong value to parse out how other factors (e.g., human footprint, lightning density, etc) explain the variance between F99 and F. Since we are assuming the climate factors shape F99, the hope is that non-climatic factors are not strongly influencing the pixels where observed F99 occurs.

Response: As explained in the introduction, our hydroclimatic model builds on the 4-switch concept (Bradstock, 2010. doi.org/10.1111/j.1466-8238.2009.00512.x) and is designed to capture the long-term effects of the two primary biophysical constraints on mean annual fractional burned area, fuel production and fuel desiccation. In this conceptual framework, the frequency/density of ignitions (e.g. through management) and frequency of favourable fire weather conditions (or fire suppression) add further constraints on mean annual burned area, i.e. they reduce burned area below the potential ($F_{0.99}$) set by the primary constraints of fuel production and desiccation. We therefore assume that in areas where the predicted $F_{0.99}$ is close to the observed mean annual fractional burned area (F), other constraints than fuel production and fuel desiccation are not limiting mean annual burned area.

14) There is some similarity to the approach here and the empirical approach of Guyette et al. (2012), although they use fire histories and try to estimate mean fire return interval

Guyette, R.P., Stambaugh, M.C., Dey, D.C. and Muzika, R.M., 2012. Predicting fire frequency with chemistry and climate.Â˘aEcosystems,Â˘a15(2), pp.322-335.

Response: Indeed, Guyette et al. show that key aspects of continental fire regimes can be explained from first principles of physical chemistry. Pointing out similarities with our hydroclimatic model would go beyond the scope of our paper.