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Discussions

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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 #1

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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:

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.

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.

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.

Minor considerations

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

rephrase or clarify.

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

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?)

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.

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]

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.

10) Line 277: Is there a way to demonstrate that model skill does not degrade as a function of fire return interval, etc?

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.

12) Figure 1: legend: replace grey dot with black

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.

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. Ecosystems, 15(2), pp.322-335.