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Interactive comment on "Fire risk modulation by long-term dynamics in land cover and dominant forest type in Eastern and Central Europe" *by* Angelica Feurdean et al.

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GENERAL COMMENT

Feurdean and collaborators present an extremely interesting work focusing on the role of different factors in determining fire risk. They use a newly curated dataset and GAM to assess the role of climate, tree cover and forest composition for the last 12ka in central - eastern Europe. I really enjoyed and learnt reading this manuscript that addresses a critical question on the domain of palaeofire but also in modern day fire ecology under the present Global Change scenarios: what might have been the most important factor determining fire hazard in the past? Not only the question is interesting but the

the data compilation is a really important effort in data curation and synthesis. Really very good job! I really like that you make your code available (despite I have some questions regarding that - see below). I have some general and particular comments, that I do not consider by any means critical to prevent this manuscript to be published, but that I would like the authors to address before the final version is submitted.

INTRODUCTION

Lines 123-127: I only have one comment in this section in connection with how we use the term "negative effect" when referring to fire risk and especially spread in broadleaf forests. I would not say that temperate forest have a clear negative effect on spread. With accrued drought and high plant density, equally intense fire risk and forest fires can happen in both oak and pine forests for instance, which can be even more difficult to extinguish owing to the calorific power of temperate forest tree wood. Despite conifers have volatile compounds and that increases fire risk, in current day forestry engineering assessments it is clear that some drought thresholds and biomass accumulation may trigger fires with that very same probability (see some references below). Pine forests and shrublands do not only burnt more because of their prevalence but because of fire selectivity to these land cover types regarding temperate forests.

- see forest composition assessment on fire risk at a Mediterranean-Atlantic area as Portugal: Fernandes 2009 https://www.afs-journal.org/articles/forest/pdf/2009/04/f08221.pdf

- See the fire-oak fire dependency for instance in Sturtevart et al 2009: https://link.springer.com/article/10.1007/s10021-009-9234-8,

- Last, Rogers et al., 2015 do not focus on fire hazard/probability but in fire response (embracers vs resisters in Northern USA and Eurasia) that have no direct relationship with fire risk.

So, while I understand that you speak on terms of Central-Eastern Europe temperate

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forests it is still imprecise to say that broadleaf deciduous trees have a negative effect on ignition probability and spread (especially the latter). I'd suggest to reword this so you either discuss this statement with more references and making clear that given the same climate conditions, temperate forests are less fire-prone, but once the fire ignites, it spreads intensively as in a needleleaf forest.

METHODS

Overall I find the methods applied appropriate for the questions you pose; both the dataset compilation and the statistical procedures are very good and, given the spatio-temporal dissimilarities of all the records, the smoothing process is the best you can probably do here. I have though some particulars that I'd like to address so we can have an open discussion:

- If you paper is eventually published (and I do hope it is!) I think your dataset should be upload not only to those repositories (GPWK, Pangea, etc. which are fine) but somewhere accessible with the code, so for instance now I have no way to check the dataset, its consistency and reproducibility. Would it be possible that you enter the data somewhere (Zenodo, dryad...) so the referees can have access to it? Also for version control – in case you realize a mistake in the future – is good to have a DOI somewhere so the future readers can have some control on different versions.

- In connection with the data curation, having the new age models you have done in this study available in the appendix would facilitate the quality assessment.

- You use GAMs (I have made some notes in the pdf itself on adding relevant references to the use of this method in palaeo as you might not have been the first group using them see Simpson, 2018 in Front. Eco.Evo) but in your plots (or code for that matter) you do not seem to include any confidence interval? Why is that? GAMs are "data-hungry" methods and I guess that in some case you might be right on the boundary to use them. I think you will have a quite some large error in the curve extremes and your conclusions may have been a bit over-interpreted just for this reason; so I think

it's important that you add your CI to the plots (and code).

- In connection with the GAM model term assessment and the use of AIC, the package mgcv already penalizes (with lasso) those terms of the model that increase complexity but do not improve fitness, why do you then chose to use AIC? I mean AIC is one of those possibly useful measurements but as you have others already implemented to assess GAMs I do not see the need to add another number to it (I found this reading about AIC quite useful: https://dynamicecology.wordpress.com/2015/05/21/why-aic-appeals-to-ecologists-lowest-instincts/) as AIC somehow increases our feeling of quality, and that might not be true.

- While I find that Supplement 2 is a really good tool facilitating reproducibility, is really difficult to find the results you mention in the main manuscript text, e.g. in line 297 you invite the reader to check that "climate alone explains a large proportion of the deviance of biomass burning in the three ecoregions in the time period between 12-8 ka BP", but finding these results in the Electronic Supp. Materials is not easy. Would it be possible to have your code commented and highlighted and hosted in a repository? I think that would increase reproducibility and therefore visibility of these results.

RESULTS AND DISCUSSION

- I find all the argumentation around the forest opening quite interesting but all the examples you bring into place refer to fuel-limited environments and how these change dynamically according to changes in productivity. In my opinion (and at the sight of the land cover reconstructions you provide) these areas have never been under real fuel-limited conditions, and reducing the tree cover may create that mosaic you discuss in lines 356-359 but may equally create effective fire barriers for fire to spread. So, I'd suggest to rather look for modern day fire ecology references showing that this might be the case in the temperate forest of central Europe. An alternative hypothesis to this "counter-intuitive" evidence where increasing tree cover reduces fire risk, may also be that a denser, thick temperate forest, (even under dry conditions) creates microclimate

conditions that reduce fire risk, as you were stating in the introduction.

- Another interesting topic that I'm unsure you discuss in depth (or maybe I over-looked it) is that there's some potentiality for spurious correlations when interpreting your GAMs. In Fig 3 is clear that increasing broadleaf forests cover decreases fire probability, but how do we know that this is not just a climate effect rather than a forest composition matter? In your TraCe reconstruction you evidenced that increasing moisture availability would have implied the expansion of broadleaf forests and that reduces fire probability, but would not be simply that less effective evaporation, i.e. increasing summer rainfall and reducing insolation, reduces fire chance? Can you please develop on this? Especially exploring Appendix A figure. I think a worth exploring aspect here is what are the the P-PET thresholds for each forest type to create a higher fire-risk.

Please also note the supplement to this comment:
https://www.biogeosciences-discuss.net/bg-2019-260/bg-2019-260-RC1-supplement.pdf

Interactive comment on Biogeosciences Discuss., https://doi.org/10.5194/bg-2019-260, 2019.