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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.

Angelica Feurdean et al.

angelica.feurdean@gmail.com

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REVIEWER COMMENTS: Reviewer #1 (Remarks to the Author):

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 palaoefire 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

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.afsjournal.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 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.

R: Thank you. We have slightly altered the content of this sentence to show that deciduous trees have low flammability, low ignition probability and fire spread, instead of saying they have a negative effect on fire. Some references provided by the R1 refer to the Mediterranean biome, which is characterised by an entirely different suite of species and behaviour related to fire. Our revised sentence now reads: "For example, needleleaf trees with volatile compounds and resins, retention of dead biomass in crown, ladder fuels and slow litter decomposition rates promote fire hazard. Temperate broadleaf deciduous trees (with the exception of drought-adapted oaks) with high leaf moisture content and lower litter accumulation, usually have a low ignition probability and less flammable fuel, although under very dry conditions fire may spread with high intensity once fuel has been ignited (Sturtevant et al., 2009; Rogers et al., 2015)." Please see lines 123-127.

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 spatiotemporal 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.

R: All charcoal records will be available at the paleofire.org. The fully commented code is available as SI2 and deposited at the paleofire.org.

- 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 "datahungry" 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). R: We have removed the word novel from introduction and added a new sentence citing Simpson, 2018 in the Method section. This reads "GAMs have been shown to provide robust statistical analyses of trends in palaeoenvironmental time series (Simpson, 2018)". Please see lines 274-275.

GAM outputs have now been re-plotted with colored ribbons representing standard errors.

- 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/whyaic-appeals-to-ecologists-lowest-instincts/) as AIC somehow increases our feeling of quality, and that might not be true.

R: The use of the lasso in fitting GAMs allows variables with little predictive power to be eliminated from a model, but it does not allow comparison of relative predictive power for models using different predictors. As the goal of the current project is exactly that sort of comparison, we do not see that the lasso is serving the same function as our use of AIC. Regarding McGill's points and the comments following that blog post, they seem to us to be more related to the misuse of AIC than any particular problems with

AIC per se, and the way in which we are using AIC to do model selection here is one of the approaches he specifically says is defensible. To quote his discussion of one example from that post: “You could also use AIC to do variable importance ranking (compare AIC of S∼prod, S∼seas, S∼energ). This is at least close to what Burnham and Anderson suggested in comparing models. You could even throw in S∼area at which point you would basically be doing hypothesis testing vs a null although few would acknowledge this.” The analogous approach (given the structure of our models) is precisely the approach adopted here; comparing models that differ by a single predictor of interest, and contrasting those with a null model. As such, we acknowledge the issues McGill raises but feel that in this case we have used AIC appropriately.

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.

R: We have included a few statistical parameters such as deviance explained and r. squared in a new Table 2 into the main text. The SI 2 contains a fully commented code. A copy of the script will be deposited at the paleofire.org.

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 Eu-

rope. 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.

R: Not all references are from fuel-limited regions, as Scheffer et al., (2012) comes from boreal, whereas Frejaville et al., (2016) from temperate region in Europe. We have refined the paragraph explaining how tree cover can affect biomass burning. This now reads: “Feedback mechanisms linking climate and fuel composition, structure and moisture could explain the shape of the fire-tree cover relationship. Low biomass burning at high tree cover may have been driven by a dense, thick forest cover with reduced understorey, which created cool and moist microclimate, and therefore has low ignition potential and flammability (Kloster et al., 2015). A reduction in tree cover and or forest openness allows the development of more understorey vegetation that provides a favourable fuel mix composed of fine herbs, shrubs and coarse woody debris that facilitates ignition and surface fire spread (Pausas and Paula, 2012; Frejaville et al., 2016). In addition, open forests favor a more dry microclimate close to the ground, as radiation can penetrate deeper into the canopy and boundary layer conductance is augmented close to the ground, which allows wind to dry the understory vegetation and litter more effectively (Ryan, 2002). Lower radiative properties of the land surface at higher tree cover, reduce evaporation and/or enhance cloud formation, which also contribute to a moister local climate (Bosman et al., 2019). At the spatial scale provided here, the pooled percentages of tree cover never declined to values that would have been indicative of fuel-limited conditions, i.e, no negative relationship between biomass burning and low tree cover (Fig. 4). However, fuel limitation as an effective barrier for fire spread may have been the case at individual sites included in this synthesis, particularly those located at low and high elevations.” Lines 390-404.

- Another interesting topic that I'm unsure you discuss in depth (or maybe I overlooked 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.

R: We acknowledged that climate conditions played a role in vegetation dynamics, which in turn feedbacked into biomass burning, i.e, decreased temperature and increased moisture availability promoted forest expansion directly and indirectly increased fuel moisture, thus reducing fire probability. The GAM table (now Table 2) showing pre- and post-8k climate alone is not such a good predictor of biomass burnt. Therefore, land cover, which admit is affected by both climate and human-driven changes, must have played an important role. Please see revised Sections 3.1 and 4.2 .

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. R: We have checked the pdf and it does not contain new comments, but repeats those already listed here.

References: Bosman, P.J., van Heerwaarden, C.C. and Teuling, A.J., 2019. Sensible heating as a potential mechanism for enhanced cloud formation over temperate forest. Quarterly Journal of the Royal Meteorological Society, 145(719), pp.450-468.

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