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Biogeosciences Discussions

# *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 comments:

This paper takes on the important issue of disentangling the relative roles of changes in climate and land-cover (both natural and anthropogenic) on bimass burning. The study employs independent data sources (climate-model simulations, pollen-based land-cover reconstructions and sedimentary charcoal-derived estimates of biomass burning) in a statistical modeling approach (generalized additive models, or GAMs) using data from Central and Eastern Europe over the Holocene. Overall, the results are likely sound, but there is need for some clarification and improvement in the pre-



sentation of the results, and I think there is also room for some more exploratory data analysis. In particular, it would be interesting to see scatter plots of the data presented in Fig. 2, and labelled scatter plots to support the relationships shown in Fig. 3. The application of GAMs in this analysis is completely appropriate, but as in any statistical analysis, the results will be more powerful if well supported by exploratory and diagnostic analyses.

Specific comments:

Line 85: "mediating the fire regime"

Line 88: I'm not sure whether GAMs are still considered "novel".

Line 90: Replace "diverged more markedly" with "was more spatially variable"? (I don't understand the notion of "divergence" of biomass burning.)

Line 92: "highest" (for parallelism).

Line 92: "decreased strongly" How does one define "strongly?" I would simply say something like "decreases to a minimum between 60% to 70%. . ." (The support for these statements is Fig. 3A, and the text on lines 360-366, right? You're talking about the form of the relationships, not the strength.)

Line 94: How does one define "abruptly?"

Line 111: "have"

Line 130: "higher tree cover" Higher than what? Maybe just "high tree cover"?

Line 133: Something missing. ". . .produced by modern forestry. . .?"

Line 139: Not just anthropogenic impact. Past climates too.

Line 141: Hyphenate "centennial-to-millennial" (when used as an adjectival modifier of, e.g. "data sets").

Line 149: For parallelism, either: "evidence of fire (something, "occurrence?" "fre-

quency?"), land-cover composition, and climate" or "evidence of changes in fire, land-cover composition, and climate."

Line 155: We are all pretty sure (we being the authors, reviewers, and readers here) that charcoal influx is a measure of fire, but we (again in the broad sense) have not yet completed the calibration between biomass burning and charcoal influx (except perhaps at the global scale, and somewhat tenuously, Harrison et al., 2018 Earth Sys. Dyanm. 9:663-677). It would be good to include a few sentences, for non-paleo readers in particular, on what the sedimentary charcoal record can and cannot say about biomass burning.

Line 156: peatland vs. lakes. There is a big difference in the way that lakes and bogs accumulate charcoal, given the propensity of bogs to potentially burn, which makes their records "lossy." This is an area of ongoing research, and the potential impact of the two different kinds of records should be discussed later in the paper (i.e. in Section 4).

Line 165: I don't quite understand this. Are the average fire sizes calculated only for fires less than 10 ha? What are the average (or better, median, fire-size has a long-tailed distribution) sizes if all fires were included?

Line 179: "charcoal accumulation rates (or "influx")"

Line 188: "To reduce the influence of high-resolution charcoal records. . ." I think there's a problem here. The description of the treatment of the charcoal data leaves out the "prebinning" or "presmoothing" step, which is designed to reduce the influence of those high-resolution records. If the bootstrapping was done by resampling individual charcoal observations (i.e. samples or influx values), then that does not ameliorate the influence of high-resolution sites, because their charcoal observations, being more plentiful, will be included in the bootstrap samples more frequently. If the bootstrapping was done by site (the usual approach), then that still does not reduce the influence of high-resolution sites, because again the high-resolution sites will flood the bootstrap



samples with their observations. (Bootstrapping-by-site was developed to include the effect of uneven spatial distribution of sites on the smoothed curves (Marlon et al. 2008). I'm guessing the presampling step was included (because it is part of the usual "analysis flow" implemented by the R package), but just not described.

Line 192: "we then calculated the mean and 90% confidence intervals of the aggregated records" In practice (at least when the locfit() function as implemented by the R paleofire package is used to fit composite curves, as described by Blarquez et al., 2014), the confidence intervals are defined by the fifth and ninety-fifth percentile values of the bootstrap replicates of the composite-curve values. The locfit() function also provides confidence-interval values, as described by Loader (1999, sec. 2.3.3), for an individual composite curve. Which confidence intervals are being described here? (I'm guessing, from the width of the intervals, the former.)

Line 195: Hyphenate "land-cover" when used as an adjectival modifier.

Line 211: Delete "this is" and run-on from the previous sentence.

Line 218: "We then generated composite estimates of land-cover classes grouped by ecoregion. . ." Presumably, the present-day ecoregions are used to do this, but haven't ecoregions moved around in the past?

Line 224: The TraCE-21ka data are reported using a fixed (modern) 365-day (or "noleap") calendar, and consequently should be adjusted to reflect the impact that changes in Earth's orbit have on the length of individual months or seasons (Bartlein and Shafer, 2019, Geosci. Model Dev. 12:3889-3913). The "calendar effect" on transient climate simulations can, for example, influence the amplitude of temperature variations over time on the order of several degrees, and change the timing of, for example, Holocene "thermal maxima" on the order of thousands of years, depending on the region and month of the year. Our paper appeared only recently, and so it would not be reasonable to expect that any analyses reported here should be redone, but it would be useful ask how big the impact might be. I looked at the monthly time series of

near-surface air temperature for a region corresponding to the data-dense region here (42.67 to 64.94 N and 7.5 to 41.25 E), for both "raw" and calendar-adjusted data. As it happens, there is little effect on June and July temperatures, but calendar-adjusted temperature for August is 1-2 deg. C higher in the early Holocene than the unadjusted data, and remains so until around 7 ka. When combined with the June and July temperature curves, the calendar effect is likely to not be significant. However, the overall "shape" of the monthly time-series curves differ, and so in future work it might be better to not aggregate the monthly time series into seasonal averages.

Line 229: "surface temperature" Was this actually "near-surface air temperature" TREFHT, temperature at the reference height (2 m) in the CCM3 variable-naming scheme, or was it land-surface temperature, also known as "skin temperature," TS? If TREFHT, then using the CRU TS 3.1 data as a present-day reference is ok, but if TS, then the amplitude of its seasonal cycle will be larger than that of temperature in the CRU data set.

Line 232: State the version number ("CRU TS 3.1").

Line 233: "as ratios of the surface temperature . . . from CRU". "Bias correction" using ratios is appropriate for precipitation, but is a little unconventional for temperature, where the biases are usually taken to be additive, not multiplicative.

Line 237: "P-PET" Why not P-E, which has been used before to index moisture availability (e.g. Daniau et al. 2012)? Potential evapotranspiration is energy limited (i.e. by net radiation in the Penman-Montieth approach, and via a temperature index in the Thornthwaite approach), whereas evapotranspiration is governed by both energy and moisture, so why is potential evapotranspiration preferred here? Moreover, I don't think Thonicke et al. (2001) an appropriate motivational citation. (That paper is about model development, not climatic controls of fire.)

Line 242: "average day length for each month . . . was calculated" Please explain.



Line 242: "The resulting climate fields. . . were interpolated" Typically, this would be done by interpolating the long-term mean differences (paleo minus present) on the GCM grid onto the 0.5-deg CRU grid, and applying them to the "observed" CRU values. Is this what was done?

Line 245: "Similar to vegetation and fire reconstructions. . ." If you followed the Blarquez et al. (2014) procedure, then the charcoal composite curves aren't exactly "loess" curves in the classical sense, where a variable-width smoothing window "span" is used (as opposed to a fixed-width window). In any case, what was sampling frequency of the three data sets?

Line 251: Reorganize the sentence. (The predictor is the sum of smoothed functions of land cover and climate.)

Line 253: Reorganize the sentences. (You used the mgcv package to fit models with thin-plate spline predictors and a Gaussian-family error distribution.)

Line 256: "Akaike Information Criterion (AIC) weights. . ." Cite: Wagenmakers EJ & Farrell S. (2004), AIC model selection using Akaike weights. Psychonomic Bulletin & Review 11: 192-196.

Line 258: "evidence within each data set" This is a fancy way of saying "goodness of fit".

Line 259: Replace "scores" with "values" ("Scores" come up in other contexts in fitting GAMs.)

Line 264: Delete "analysis".

Line 266: "see Pollen-based. . ." Refer to the section number.

Line 267: "GAMs on JJA climate" Jargon. ("GAMs using JJA climate" would be ok.)

Line 273: "over all . . . and within the three ecoregions."

Lines 274-290: Straighten out tense throughout. (“simulation indicates” vs. “Biomass burned showed” etc.)

Line 278: Reword “The reduction in biomass burned accompanied a decrease in JJA temperature and an increase in summer moisture availability. . .” (for parallelism).

Line 281: “but less evident” Something missing.

Line 293: This section could use a summary paragraph that summarizes the results and motivates the further statistical analysis which looks at the combined effects of climate and land cover. Alternatively, that could go into a lead paragraph in the following section, which otherwise gets right into model selection.

Line 298: I see an appendix A (but not A1), but that does not report any deviance values. In the supplement, I can see that the average of the deviance explained by the first three models is 71.7%, so perhaps the value in the text is just badly rounded. I really think you need a summary table in the main text, that summarizes the goodness of fit and complexity of each model. The reader should not have to conduct an additional analysis to figure out what’s going on. In addition, it is not clear just what data is going into the analyses. In the supplement, it looks like there are 80 observations in the “full” data sets (i.e. 12 ka to 0 ka), 39 in the 12 ka to 8 ka subset, and 80 again in the 8 ka to 0 ka subset. Does this imply that temporally spacing of the input data varies over time?

Also, the GAMs are being fit to data that already have be smoothed, so to what extent does that influence the interpretability of the deviance explained? At a minimum, a time-series of the residual values would be interesting.

Line 302: “Table 1” The table should indicate that the results pertain only to a subset (8 to 0 ka) of the data.

Line 303: Replace “scores” with “values”.

Line 309: “Fig. 3A” How were these marginal plots constructed? (The supplement



seems to conveniently omit the code. . .) Personally, I dislike plots that are unsupported by actual data (unlike the partial residual plots in the supplement). Would labelled scatter plots be convincing?

Line 324: “higher-than-present”.

Line 327: “recovered?” That’s kind of a Holocene-centric view of the world.

Line 328: “land-cover change and human imprint” (for parallelism).

Line 330: “land-cover models” Replace with “models that include land cover as predictors”.

Line 332: “mid-to-low latitudes”

Line 333: “climate reconstructions are fragmentary and mostly qualitative” I think some of your coauthors would disagree with that assertion.

Line 336: “Simulated” Does this refer just to the TraCE-21ka simulations, or to simulations in general?

Line 343: “This could be partly explained by. . .” And also (largely I think) by climate-model resolution.

Line 350: “GAM models” Expanding the acronym yields “generalized additive model models,” so just say “GAMs.”

Line 350: “While the GAM models use biomass burned as the response variable. . .” They use composite curves of charcoal influx, which are thought to represent biomass burning.

Line 355: “lowering” You mean “decrease in tree cover” as opposed to canopy height or something, right?

Line 361: Figure 4 needs more explanation, and might be out of place. What are the individual points? Should the figure preceed Fig. 3 (and be explained earlier)? It

seems to simply show data, and not the results of any model fitting.

Line 367: “regional divergence between biomass burned and percent tree cover” Do you mean divergence in the relationship between the two, or simply regional variations in the relative levels of each?

Line 369: “Broadleaf cover. . .” But Table 1 shows the lowest AIC for a model with total tree cover in the CON region.

Line 391: “While past ignition is assumed to increase with population density. . ..” Citations?

Line 392: “and associated fuel limitation” It’s not just fuel limitation that reduces burning in arable lands. It’s never been good policy to burn crops, except as an element of warfare.

Line 436: “summer conditions”?

The attached figure shows area-weighted averages of TraCE-21ka near-surface air temperature (TREFHT) for ice-free land grid points over the region 42.67 to 64.94 N and 7.5 to 41.25 E. The gray and black curves show individual annual values and 30 yr (window half-width) locally weighted means of the distributed data, while the overprinted pink and red curves show data that has been “calendar adjusted.” See Bartlein and Shafer (2019, Geosci. Model Dev. 12:3889-3913, for discussion, in particular Sec. 3.4).

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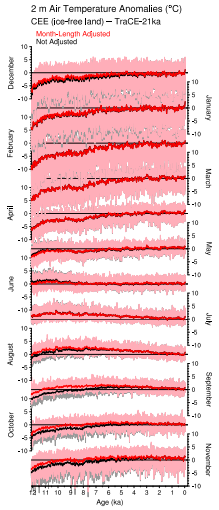


**Fig. 1.** Calendar-adjusted TraCE-21ka data