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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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Reviewer #2 (Remarks to the Author): Biogeosciences Discuss., https://doi.org/10.5194/bg-2019-260-RC2, 2019 Author(s) 2019. Interactive comment on "Fire risk modulation by long-term dynamics in land cover and dominantforest type in Eastern and Central Europe" by Angelica Feurdean et al. Patrick Bartlein (Referee) bartlein@uoregon.edu Received and published: 31 October 2019 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 presentation 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. R: We present these scatter plots in SI 2. We have conduced additional statistical analysis for GAMs (please see our response below).

Specific comments: Line 85: "mediating the fire regime"

R: "in mediating fire regime" corrected to "mediating the fire regime". Line 90.

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

R: We have deleted the word novel.

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

R: Indeed diverge was used here to reflect spatial variability. This sentence now reads "Biomass burning was highest during the early Holocene and lowest during the mid Holocene in all three ecoregions, but was more spatially variable over the past 3-4 ka BP." Lines 90-94

Line 92: "highest" (for parallelism).

R: Stronger predictor was replaced with highest predictor. Line 93.

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.)

R: Done, the sentence now reads: "In temperate forests, biomass burning was high at $\sim$ 45% tree cover and decreased to a minimum between 60 to 70% tree cover." Lines 93-94.

Line 94: How does one define "abruptly?" By abrupt we mean changes in characteristics that occur very fast with respect to the scale of observations, here represented by the relationship between two variables. In Fig. 3, one can see that in the BNE ecoregion, the decline in biomass burning is not gradual but very fast once the tree cover become higher than 70%.

Line 111: "have"

R: Done, vegetation properties has... corrected to vegetation properties have. Lines 122.

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

R: Done "While high tree cover may reduce fire hazard, fire.... . Line 132.

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

R: Corrected to: "Widespread plantations of highly flammable trees (e.g. Pinus) produced by modern forestry may further increase..." Line 134

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

R: Done, this sentence now reads: "Yet, present-day ecosystems and fire regimes carry the legacies of past anthropogenic impact and climates" Line 140.

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

R: Done, Hyphenated. Line 143.

Line 149: For parallelism, either: "evidence of fire (something, "occurrence?" "frequency?"), land-cover composition, and climate" or "evidence of changes in fire, land-cover composition, and climate."

R: Done, this sentence now reads: "This study utilises independent evidence of changes in fire, land cover composition and climate with a statistical modelling approach..." Lines 149-150.

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).

R: We have introduced a paragraph addressing the issue of limitation of charcoal records. This reads: "Sedimentary charcoal is the most common proxy to determine relative changes in biomass burning (Adolf et al., 2018). However, due to the absence of calibration data sets and a poor understanding of the influences of non-fire-related processes on biomass burning, it is not possible to quantify the absolute burned area from charcoal records. We therefore interpret the charcoal signal as relative trends in biomass burning (see Marlon et al., 2016 for advances and future recommendations in the field of proxy-based fire reconstruction). Lines 160-165.

We are aware of the differences in charcoal deposition between lakes and bogs and we have acknowledged this in the methods section. In the same way, differences are probably less important than between a lake with or without inflow or outflow. Statistical treatment of the charcoal records has reduced, to some extent, the biases connected to both analytical method and depositional environment. However, to thoroughly address this subject with the current dataset would be a new study in itself and it is beyond the scope of this paper. We therefore cannot carry out the reviewer's suggestion. However, we have included a paragraph addressing the difference between charcoal records derived from peat and lakes: "Regarding the depositional environment, bogs provide a more local representation of past fire occurrence than lakes, because they are characterised by limited charcoal transport and post-fire transport or erosion (Conedera et al., 2009; Mooney and Tinner, 2011; Remy et al., 2018). In addition, peatlands are susceptible to burning, which may introduce hiatuses in the depositional environment. ". Lines 165-169.

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 longtailed distribution) sizes if all fires were included?

R: We have clarified this, it now reads "The average fire size is ca. 10 ha in eastern Europe, between 5 and 10 ha in southern Europe and <5 ha in northern and central Europe". Lines 176-179.

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

R: Completed "Charcoal concentrations were transformed into charcoal accumulation rates or influx (CHAR). . . " Lines 191-192.

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.)

R: We have clarified this and the paragraph now reads: "The standardisation procedure included a min-max rescaling of CHAR values, followed by a Box-Cox transformation to homogenise within-record variance, and a Z-score transformation using a base period from 12 to 0.15 ka BP. This base period includes the entire dataset, but excludes the effect of recent human impact on fire activity during the post-industrial period. For compositing charcoal records by ecoregions, transformed charcoal records from each ecoregion were pre-binned in 100-year bins to reduce the influence of high-resolution records on the composite charcoal record. Pre-binned charcoal time series were smoothed with a LOWESS smoother with a 500- year window half width. Confidence intervals values (95%) were calculated by bootstrap resampling the binned charcoal series and calculation of the mean for each bin 1000 times (default settings). For numerical processing of the CHAR series we used the R paleofire package version 4.0 (Blarquez et al., 2014). CHAR composite anomalies (100-year time interval) relative to the Holocene average of the entire CEE region and the three ecoregions, represent regional trends in biomass burning; where zero Z-score values correspond to the mean charcoal influx over the Holocene; positive Z-score values represent greater-than-mean charcoal influx over the Holocene; and negative Z-score values lower-than-mean charcoal influx over the Holocene. " Lines 194-210.

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

R: Done.

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

R: The two sentences were united in a single one which now reads: "Throughout the text, we use the term 'grassland cover' to denote both natural and human-modified grasslands (pastures), and 'arable land cover' to denote arable and disturbed land, because it is not always possible to distinguish between natural and managed grasslands or between arable and other forms of disturbed open land cover based on pollen analysis (Fyfe et al., 2015)". Lines 228-232.

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?

R: Yes present-day ecoregions, our sentence now reads: "We then generated composite estimates of land cover classes grouped by present-day ecoregions by spatially aggregating the averages of pollen records within the corresponding ecoregion". Lines 243-244.

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.

R: We have added a new sentence stating that: "The model does not account for changes in the length of individual months or seasons due to variations in Earth's orbit (e.g. Bartlein and Shafer, 2019)" Lines 257-258.

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.

R: We used the near-surface air temperature. We have modified the text accordingly. Line 247.

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

R: Done. Line 250.

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. R: You are right. It should not be ratios. We used anomalies for the temperature. The text has been modified accordingly and now reads: “The bias correction was calculated with respect to the last 30 years of the TraCE-21ka simulation (representing pre-industrial conditions) as anomalies of the surface temperature and ratios of precipitation. The temperature anomalies were subsequently added, and precipitation ratios multiplied, to the CRU data in order to obtain the bias-corrected climate. ” Lines 253-258.

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 modeldevelopment, not climatic controls of fire.)

R: We used PET as we preferred a drought indicator that purely depends on climate and not on soil and vegetation. P-E would, of course, also have been a sensible choice. In our opinion both indicators are similarly adequate and would yield similar overall conclusions. We considered PET to be more parsimonious as simulating E depends on more assumptions. In Daniau et al. (2012), it is not clear for us how E was calculated. The author list indicates that the approach from the biome and STASH models from Prentice et al. and Sykes et al. from the 90s was used. In this approach, E depends on soil water, and soil water holding capacity is needed as input, in the models mentioned above often using 150 mm across the globe (plant-available water, not total volumentric content). We also considered this approach, but finally decided to use the simpler approach using PET. Using the balance of PET and P is also a common approach (e.g. Hickler et al. 2009). In addition, because PET is generally more sensitive to changes in climate than AET, it also yields a larger signal-to-noise ratio when subtracted from

precipitation. As a result, using PET it is easier to identify periods that are particularly wet or dry in different regions compared with using AET. Finally, P-E has the caveat that it becomes zero under water stress, but it cannot show the severity of water stress, i.e., it cannot indicate the quantity of water that lacks to satisfy atmospheric demand (E cannot be higher than P if changes in soil water storage are negligible compared to changes in P, which is common). We think that discussing these issues would be beyond the scope of the manuscript. However, thanks for pointing out that Thonicke et al. (2001) was not a good reference here. Line 236 to 240 was re-formulated as: "We used the boreal summer (June, July, August, hereafter “JJA”) surface temperature (JJA T) and precipitation minus potential evapotranspiration (JJA P-PET), as a proxy of peak summer dryness, which is a main driver of fire risk. PET was calculated using the Thornthwaite model (Thornthwaite, 1948), which requires the surface temperature and average day length of each month as input variables."

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

R: How we calculate the average day length is already stated in the manuscript: “the average day length for each month going back to 12 ka BP was calculated using the Earth's orbital parameter scheme in CCSM3.” Lines 268-270.

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?

R: Yes, this is exactly how it was done. The bias-correction procedure using CRU is already stated earlier in the text (albeit wrong, but now corrected due to your comment). Lines 258-264. To emphasise that the bias-corrected fields are also used in the interpolation the specific site, we now write: “The resulting (bias-corrected) climate fields were subsequently interpolated to the same locations as the charcoal records using a bilinear interpolation.” Lines 270-272.

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?

R: We used a 100-year time interval for charcoal and climate (please see lines 207 and 268, respectively) and a 200-year time interval for pollen (line 214). To generate composite estimates of JJA climate and land cover classes in Fig. 2 we fitted a 500-year loess smoother (please see lines 528-529). Line 251: Reorganize the sentence. (The predictor is the sum of smoothed functions of land cover and climate.)

R: Done, it now reads “The predictor is the sum of smoothed functions of land cover and climate (Hastie and Tibshirani, 1990).” Lines 274-275.

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

R: Done, it now reads “We used the mgcv package to fit models with thin-plate spline predictors and a Gaussian-family error distribution to automatically determine the optimal level of smoothing for each term in the model and automatic term selection (Hastie and Tibshirani, 1990).” Lines 275-278.

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.

R: Reference included.

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

R: Sentence changed accordingly. “AIC weights are a normalized indicator of support for each model given the goodness of fit while penalising more complex models”. Lines

280-282.

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

R: Scores replaced with values. Line 282.

Line 264: Delete “analysis”. R: Deleted.

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

R: Done. Line 298.

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

R: Done. This sentence now reads: ‘’However, we also constructed GAMs using JJA climate for the 12-8 ka BP period to investigate the relationship between climate and fire without any significant human impact”. Lines 289-292.

Line 273: “over all . . . and within the three ecoregions.”

R: Done. This sentence now reads “The amount of biomass burning was highest during the early Holocene (between $\sim$ 10.5 and 8 ka BP) over all of Central and Eastern Europe and within the three ecoregions.”

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

R: Done. Line 298.

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

R: Done. This sentence now reads: “The reduction in biomass burning accompanied a decrease in JJA temperature and an increase in summer moisture availability (around 8 ka BP) in all ecoregions. . .” Lines 301-303.

Line 281: “but less evident” Something missing.

R: Rephrased: “We found differences in trends in biomass burning among ecoregions over the past 3 ka BP. Biomass burning increased markedly at 3 ka BP in the BNE ecoregion, but this increase is less evident in the CON ecoregion”. Lines 303-305.

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.

R: Thank you for this suggestion. We have introduced the following paragraph: “When considering GAMs fitted with only climate predictor variables over the full time series (12-0 ka BP), the proportion of the deviance of biomass burning in the three ecoregions averages 48% (Table 2). There is a marginal increase in biomass burning with increasing temperature in the BNE, whereas it shows first a decrease, then an increase with increasing temperature in the CON and ATL ecoregions, which is difficult to interpret. Furthermore, the response of biomass burning to P-PET is more unpredictable in all ecoregions (Supplement SI2). We therefore search into the drivers of biomass burning on the pre- and post-8 ka BP time periods separately”. Lines 324-330.

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. A) 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. B) 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? C) 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.

R: We have introduced a new table (Table 2) into the main text that shows the deviance values and R-Squared values.

Thank you for spotting this, we have used 119 points for full datasets (12-0 ka period), 39 points for 12-8 ka period and 80 points for 8-0 ka period. This is corrected in our new SI2.

We have now included plots of residuals as a function of time. These plots show that there is still a fairly high amount of temporal autocorrelation in the residuals, particularly for the null models. As an experiment we constructed a separate series of models that were identical to these but which also include a smoother for time itself. These models removed some of the temporal autocorrelation in the residuals, but not all. This is not surprising given that the response variable itself is an estimate produced from a smoother fitted using time as a predictor; is expected to be substantially more temporally autocorrelated than the original source data from which it was interpolated. More importantly, the models fit with an additional smoother for time produced model selection results that were not materially different from those without. As a result, we are presenting the original models for the sake of simplicity.

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

R: Done: Table 1 now reads “Model selection results for generalized additive models of the effects of land cover and climate on biomass burned for the period 0-8 ka. ”

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

R: R: Done.

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 labeled scatter plots be convincing? R: Please see the full version of the code in SI2 and paleofire.org.

Line 324: “higher-than-present”.

R: Done. “We found that climate, specifically warmer-than-present summer temperatures and higher-than-present moisture content“. Lines 359-361.

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

R: We have slightly altered the sentences, which now reads: “Enhanced biomass burning with increasing temperature and moisture in the early Holocene is expected, as fuel builds up progressively following the cold and dry conditions with limited biomass prevailing during the Lateglacial (Feurdean et al., 2014)”. Lines 362-364

Line 328: “land-cover change and human imprint” (for parallelism) and Line 330: “land-cover models” Replace with “models that include land cover as predictors”.

R: Done. Lines 365-366.

Line 332: “mid-to-low latitudes”

R: Done. Line 371.

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

R: Independent of vegetation, quantitative climate reconstructions are truly rare in this region, and we would like to avoid using pollen as both a way to reconstruct land cover and climate conditions in order to circumvent the risk of circularity. This sentence was now altered to: “Except for pollen-based climate reconstructions, proxy-based climate reconstructions are mostly qualitative and more fragmentary, which hampers their inclusion into the generalized additive models”. Lines 371-373.

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

R: Amended to: “TraCE-21ka simulation and proxy-based climate reconstructions are in general good agreement in indicating warm and dry. . .” Lines 375-388.

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

R: Amended accordingly “This could be explained by climate-model resolution, though an increasing human impact on the proxy-based climate reconstructions such as the effect of water acidification and eutrophication on chironomid taxa and deforestation on pollen and testate amoebae composition could also be responsible (Heiri et al., 2015; Mauri et al., 2017). ”. Lines 282-286. Line 350: “GAM models” Expanding the acronym yields “generalized additive model models,” so just say “GAMs.”

R: Here and elsewhere in the Discussion we have replaced GAM models with 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. R: Sentence amended accordingly, it now reads: “While the GAMs use charcoal influx, which is thought to represent biomass burning as the response variable, we acknowledge that the relationship can go in both directions”. Lines 290-294.

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

R: We mean a reduction in the tree cover not canopy height. Sentence changed to better reflect this: “A reduction in tree cover 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).” Lines 400-404.

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.

R: Indeed Fig 4 should precede Fig. 3 and in the revised version we have swapped places plus added an explanatory text to Fig. 3 in section 3.1. This reads: "Three-dimensional scatter plots of CHAR and percentages of land cover classes show that locations with greater biomass burning tend to be consistently characterised by low broadleaf tree cover in the CON and ATL ecoregions, and by high needleleaf forest cover in the BNE ecoregion (Fig. 3). CHAR also increases with arable and pasture cover, although percentages of the two land-cover classes at which biomass burning increases vary between ecoregions (Fig. 3). " Lines 316-320.

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?

R: We mean that the relationship is divergent. We have amended this sentence to better reflect this: "The GAMs run separately on broadleaf and needleleaf tree cover indicate regional divergence in the relationship between biomass burning and percent tree cover associated with the dominant functional forest type (Figs. 3, 4). " Lines 414-417.

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

R: Thank you, we have corrected this. The sentence now reads: "Broadleaf cover had the most powerful negative effect on biomass burning in BNE and ATL ecoregions, and the second-most negative effect after total tree cover in the CON ecoregion (Fig. 3B; Table 1; Supplement S2) ".

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

R: Refined to: "While ignitions may increase until population reaches intermediate density, human-caused change in land cover from forest to arable land and associated fuel limitation result in a decline in biomass burning (Marlon et al., 2013; Andela et al., 2017). It has also been argued that increasing population density reduces fire frequency and burned area through the impact of land conversion and landscape fragmentation on fuel availability (Knorr et al., 2014). "

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.

R: We refer here to the burning of crop residue and not the crop themselves. We have stated in the next sentence that: "If fire was primarily restricted to burning of agricultural waste, e.g., straw and chaff, to improve soil fertility and clean the land, this should have provided less biomass to burn than wood (Pfeiffer et al., 2013).

Line 436: "summer conditions"?

R: Amended, this sentence now reads: "Although the climate was an important driver of fire hazard during the early Holocene, in particular warmer and drier-than-present summer conditions." Lines 490-493.

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). P.J. Bartlein Interactive comment on Biogeosciences Discuss., https://doi.org/10.5194/bg-2019-260, 2019. Fig. 1. Calendar-adjusted TraCE-21ka data.

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