Interactive comment on “Comparing modelled fire dynamics with charcoal records for the Holocene” by T. Brücher et al.

Reply to Anonymous Referee #2

Dear Referee,

thanks for your critical and helpful comments on the paper to improve it further.

Please find our specific changes to your comments:

General comments:

Scale issue

We appreciate the concern about scale mismatches expressed here, but for this study it would be inappropriate to compare grid cells to data points. Although paleofire records are point estimates, syntheses of multiple records from regional to global scales have consistently shown correlations with climate data on millennial scales (e.g., Marlon et al. 2008; Power et al. 2012; Marlon et al. 2013 [western US] - could also add [Han, Y.M., Marlon, J.R., Cao, J.J., Jin, Z.D., An, Z.S. Synchronous variations in soot, char and climate in Asia. (2012) *Global Biogeochemical Cycles* 26(4)]. The objective of this study is therefore to determine whether such broad-scale correlations have a mechanistic basis that can be examined with a global model. Extracting individual grid cells from the model simulations to compare with the point data would thus be highly unlikely to produce meaningful results. The inherent randomness of ignitions on the landscape, the diversity of topography and other fine-scale influences on fire that often result in large differences when comparing a small number of nearby records [Gavin DG, Hu FS, Lertzman K, & Corbett P (2006) Weak climatic control of stand-scale fire history during the late Holocene. Ecology 87:1722-1732] is what we are attempting to "filter out" in a sense by conducting our analyses at continental to global scales in this study.

From the Earth System modeling perspective, grid points within Global Climate Models (GCM) do not resolve regional heterogeneity. E.g. the regional topography could not be resolved, the full area in a grid cell is on the same height and also temperature and precipitation (amount, intensity) is the same over the complete domain. Given these facts, someone should not trust single grid-box output, and therefore we do not recommend comparing time series of specific single grid cells with point measurements. While the charcoal is transported via the atmosphere it is not a-priori given, which grid box (or even boxes) of the model should be taken to compare with one single charcoal site. A huge effort would be necessary, to compute ejection height and atmospheric transport which is beyond the limitations of the climate model used in this study and the scope of this work.

The mean time series out of all grid cells is not area-weighted, what is typically done in the climate modeling community. This is due to the fact, that within the processing of regional or global Z-scores out of charcoal records no weighting function is applied to calculate the average: each charcoal site is transformed to Z-scores and all Z-scores are linear averaged. By computing the Z-scores of each grid cell, the information of the cell area gets lost, as each time series is scaled by there amplitude and afterwards all time series are averaged linear.

Refer to study by Molinari et al., 2013

Thank you for bringing our attention to the paper by Molinari et al., 2013. We will refer to it in the revised paper.

"Another regional model study by Molinari et al (2013) compared charcoal records in Europe with an Earth System Model that simulated dynamic vegetation using lpj-guess and two independent land-cover scenarios. The climate simulations and analysis of land cover change suggests biomass burning across Europe was primarily explained by vegetation, precipitation and temperature-related parameters during the early Holocene. Charcoal-based observations of increased fire activity during the mid-late Holocene were primarily driven by changes in anthropogenic land-cover, and secondarily by changes in vegetation and temperature (Molinari et al 2013). As our study does not count for anthropogenic changes, we can conclude, that the simulated climate seems to be off and therefore the simulated burned trend of burned area does not fit to the reconstructions."

CO2 fertilization

You are right, that we have to reframe the sentences dealing with CO2 fertilization of the biosphere, as the effect is minor, but exists.
Doing a back on the envelope calculation and referring to the paper by Arora et al. (2013, Journal of Climate, DOI: 10.1175/JCLI-D-12-00494.1) the carbon concentration feedback parameter for Land (β_L) in our model is about 1.46 Pg C ppm^{-1}. Having an increase of about 12 ppm and a land carbon stock of roughly 4000 Pg, the additional carbon stored in the biomass is about 17.52 Pg, which is less than 0.5%. The carbon is partly stored in the soil, so the effect of 'additional' carbon emitted by fire is low. This effect will be different for different regions, as the fuel is different (grass or woody type).
To quantify the effect of fertilization on the emissions due to fire, a setup of varies model simulations would be necessary to do a factor separation.

Specific comments:

Page 6436, lines 4-5
Does this sentence imply that there are no biogeophysical feed backs between land and atmosphere in CLIMBA? How might changes in burned area affect climate locally? I suppose that the very coarse CLIMBER-2 grid is too coarse to realize any biogeophysical feedback as a result of changes in burned area, but this issue should be commented on, either here or in the discussion section.

The biogeophysical effects linked to changes in vegetation cover (albedo, roughness, transpiration) are accounted in land surface scheme of CLIMBER-2 by internal vegetation model VECODE. Biogeophysical feedbacks as a result of changes in burned area (e.g. changes in albedo) are not considered. The coupling of the two models (CLIMBER and JSBACH) closes the carbon cycle, but there is no biogeophysical feedback between models. But as the overall

change in simulated burned area is rather small, the biogeophysical effect on grid point scale (even within JSBACH) is low. We will add the following sentence:

"This coupling scheme does not allow to account for biogephysical feedbacks linked to changes in the burned area, as the CLIMBER and JSBACH models are only coupled via the carbon cycle. The biogeophysical effects due to changed vegetation distribution are solely simulated by the land surface model of CLIMBER-2 (Brovkin et al., 2002)."

Page 6437, lines 5-9
If anthropogenic emissions of CO2 as a result of land cover change are not considered in the current study, this sentence can be removed, as it describes something that was not part of the study and is a potential source of confusion, as it strays from the main message of the manuscript.

In our opinion the sentence has to be reformulated, and not skipped. The information will help to understand why the transient simulation ends with a lower than observed value for atmospheric CO2.

"As the focus of this study is on natural vegetation and natural fire occurrence, anthropogenic land use emissions (e.g., Pongratz et al., 2009; Ruddiman, 2003) are neglected, which would lead to higher atmospheric CO_2 concentrations at pre-industrial (PI) times (about 18 ppm by a land use emission scenario based on Hyde (Goldewijk et al., 2001))."

Page 6438, lines 21-22
As commented above, because the charcoal data are not uniformly representative of a region, model-data mismatch could be a result of data processing. This statement should be modified to acknowledge this fact.

We partly disagree. On the one hand you are absolutely right, the input data and the spatial scale between charcoal records and model data are different. On the other hand, the data processing– converting model output to Z-scores – is done identically for model and data. So, differences in the time series related to different spatial coverage can be acknowledged, but not because of differences in data-processing. To make this clear, we add:

"To be more specific, only differences in the spatial data coverage and model deficiencies could lead to differences."

Page 6439, lines 8-10.
This sentence is awkward and confusing and might better be broken into two. Please revise.

Text will be clarified in the revised version.

Page 6440, line 2
Inappropriate Germanized usage of the word "until", revise.

Text will be clarified in the revised version.

Page 6440, line 13-34

The sentence starting with "Regions. . ." is awkward. Revise.

Text will be clarified in the revised version.

Page 6440, line 26 Replace "wide spread" with "widespread"

Text will be changed in the revised version.

Page 6444, lines 16-18
The sentence beginning with "Since..." is poorly worded, please revise.

Text will be clarified in the revised version.

Also, it is not clear how changes in vegetation fraction are estimated given the authors own admission that both the climate (and probably the land model) are not sufficiently detailed in spatial resolution to really say much about this region. I suggest not trying to draw these kind of conclusions based on the limitations in the model setup the authors describe.

For this specific region the model does not agree with reconstructed trends, this is related to model limitations rather than data uncertainties. Since model-data trends do not agree, we highlight this region for further investigations.

We hypothesize the climate driver for biomass burning in the above case may not occur within the current model, capturing the wrong simulated trend). We agree that this output provides limited information on drivers of changes in the charcoal-based records of biomass burning, as they both do not match. It would be incorrect to assume variability in this region is driven by precipitation, as suggested by the simulation..

To make it more clear, we add the following:

"Since the vegetated fraction does not increase significantly, the higher values of Z-score burned area are likely linked to changes in precipitation, which seems to be off in the simulation."

Page 6444, lines 27-29
These two sentences should be revised to avoid colloquial language and delete extraneous words, e.g., ". . .as the a member...".

Text will be clarified in the revised version.

Page 6445-6446, Paragraph beginning on p. 6445, line 12
This paragraph is confusing and it is not always clear what quantities are being discussed. For example, "JSBACH simulates an increase [in what?] in all regions. . .", or ". . .the highest number [of what?] is found. . .". The discussion switches been "increase" and "decrease" frequently and I had to read the paragraph several times and refer back to the figures to understand what was being described. I should not have had to do this.

The phrase starting with "the Z-score transformed values show an opposite trend. . ." doesn't make any sense at all. Please revise this entire paragraph to improve clarity and emphasize the main message the authors want to convey.

Text will be clarified in the revised version.

Page 6446, lines 16-17
Again, please improve the clarity of this awkwardly worded sentence.

Text will be clarified in the revised version.

Page 6447, lines 6-8
The sentence starting with "Close to the overall. . ." does not make sense. I think the authors are trying to compare total burned area with Z-scores, but I am not sure. If a comparison between charcoal Z-scores and burned area is intended, I doubt the validity of any implied causal link, because changes in Z-score do not have to be caused by changes in burned area, fire return interval being rather more important, so this is not really be an appropriate comparison. The authors acknowledge this point, however obliquely, on page 6448, lines 16-20.

The intention is to put some numbers on the increase of burned area. As charcoal Z-scores do not tell anything about it, it could be of interest for the reader. We modify the sentence to avoid confusion.

Text will be clarified in the revised version.

Page 6448, line 2
Remove the comma in "shows, that"

Text will be changed in the revised version.

Page 6448, lines 16-20
Please improve the wording of the very awkward and long sentence starting with "So, neither. . ."

Text will be clarified in the revised version.

Page 6456, Figure 3
All of the panels in this composite figure are too small to make a meaningful interpretation of the results. Please replot in larger size or split into separate figures or both.

The figures where designed for the final manuscript layout and not for the discussion template. Having the figure 3 on a single page all details are readable. Therefore, there is no need to replot the figures of fig. 3.