

We thank the two reviewers for the detailed and helpful comments on our manuscript. We hope to have addressed all raised issues in our response and in the revised manuscript.

In their manuscript "Controls on fire activity over the Holocene," Kloster et al. describe paleo-fire simulations made using global fire model driven by changes in climate and vegetation. They compare simulated area burned to inferences from paleofire records (from charcoal data), and assess the relative importance of different forcing variables on past burning among several large regions. Overall the analysis is well-conceived, the data appear to be of high quality, and the paper is nicely written and easy to follow. I would like to see a more thoughtful interpretation of the results and have a number of other minor suggestions, but otherwise recommend the manuscript for publication.

Major comment:

My main criticism of the paper is that it presents little interpretation of the results. Presently there is no "Discussion" or similar section, and the only interpretations/implications are given in a rather light "Conclusions" section. I think the paper would be most improved by adding a more in-depth discussion of its findings. I encourage the authors to think critically about the aspects of their study that they find most compelling and focus on these, but also offer three suggestions that stand out to me here.

R1.1 - First, there is no discussion of the extent to which effects of different forcing variables on simulated burning depends on past variability in those variables versus sensitivity of the fire regime (real or simulated) to them. This distinction is very important—as an extreme example, note that either a constant Holocene climate or complete insensitivity of fire regime to climate would lead to the conclusion that climate variability was unimportant to past fire regime change, but the implications are obviously quite different. At minimum this distinction needs to be assessed thoughtfully, and I would think that doing so would lead to fruitful ground for further discussion (e.g. implications for future change). Note also that the below suggestion (see "Minor comments") to present the forcing data in a more interpretable form (i.e., not as unitless ratios) would probably be helpful here.

We extended the fire model description and present more in detail the sensitivity of the fire model to the single forcing variables in the Method section of the revised manuscript: "Soil moisture, aboveground biomass, and wind speed control the burned area in the fire model. A high soil moisture lowers the fire occurrence probability and the overall fire spread. The model assumes that above a moisture of 0.35 a fire gets extinguished. A high aboveground biomass assures a high fire occurrence probability. The fire model scales the fire probability constrained by fuel availability linearly between a lower aboveground biomass amount of 200 gC/m² and an upper amount of 1000 gC/m². A high wind speed increases the fire spread and the burned area. An increase in wind speed from 15 to 20 km/hour, for example, results in an increase in the fire spread rate of 25% based on observations (Arora and Boer, 2005)."

In the Result section we included more detailed information on the strength of the single changes impacting burned area and extended the discussion in the Conclusion section (changes are highlighted in red in the revised manuscript).

R1.2 - Second, I find the discussion of charcoal- vs. simulation-based results (last paragraph in the paper) insufficient. Certainly it is true that discrepancies indicate that one or both data sources are "wrong", but this is not a very insightful conclusion. I think the authors have a responsibility to make a more critical evaluation, at least of the simulated burned area, if not the charcoal data as well (which is perhaps not their expertise). As a particular example, simple "uncertainties" do not sufficiently explain the completely opposite trends of charcoal vs. simulation data in Europe, which the authors mention specifically. Another obvious point of discussion here is the distinction between error in simulated burning due to deficiencies in the fire model, the climate model, and the forcing data used to drive the latter. As experts in the fire modeling community, I believe the authors should be able to weigh in insightfully here. Overall, I agree with the statement (last line of the manuscript) that combining fire models and charcoal data could help reduce uncertainty in both. But this study is one of the first to take such an approach, so the authors need to be sure to set a good example of how such data-model insights can be gained.

We extended the discussion on charcoal versus simulation based results in the Result section (in the revised manuscript you can find these changes highlighted in red). However, we also stress that this discussion has been made already in detail in Bruecher et al., 2014, in which the same simulations were analysed and for example the opposite trends of charcoal vs. simulation data in Europe is

already discussed in greater detail. We added:

“Bruecher et al., 2014 compared in detail simulated burned area and charcoal data reported as z-scores. For the same regions as presented here Bruecher et al., 2014 found rank correlation between simulated burned area and charcoal data reported as z-scores between 0.32 and 0.66, with the highest correlation found for North America, which is also the region with the most charcoal data available (up to 83 charcoal sites).”

and

„Europe is the only analysed region for which the simulated burned area and the charcoal data show opposite trends. One reason for this discrepancy might be the missing anthropogenic fire control in our simulations. Molinari et al., 2013 showed in a modeling study that increased fire activity during the mid-late Holocene were primarily driven by changes in anthropogenic land cover, which we do not account for in our simulation.“

R1.3 - Finally, it is surprising that there is little discussion of implications to modern/future change. I think it is fine that the study focuses on pre-industrial changes, but clearly one of the key motivations for any paleo- analysis is to learn something relevant to the present Earth system state and potential future trajectory. Explicitly comparing simulated pre-industrial burning to modern (e.g. GFED database) seems entirely appropriate and within the scope of this paper, and could lead to some interesting insights about recent change (or at least about model performance, recognizing caveats about human activity, etc.). In any case, the relative importance of different forcing variables and the trajectory of past fire activity certainly have implications for future fire regimes in scenarios of global environmental change. The impact of the paper would be greatly improved if these were explored thoughtfully in the discussion.

We added a paragraph in the Conclusion section on the implications of our results for predicting future global fire activity (in the revised manuscript you can find these changes highlighted in red):
”Consequently, estimates on future fire activity can not only be based on e.g. temperature and precipitation trends derived from climate projections but require a more integrative approach. This could be based on process based fire models that are evaluated against observations including charcoal data or more complex causal functional relationships derived from observations that will greatly benefit from a further extension of the charcoal database. Future fire activity, however, will in many parts of the world be strongly anthropogenically disturbed, which limits the applicability of relationships derived from past fire activity to future climate conditions. Changes in land use, urban settlement, human ignition and fire suppression will all impact fire activity and will in many places of the world most likely dominate the overall change in fire activity (Andela and van der Werf, 2014, Kloster et al., 2012). Nevertheless, understanding the climate control on fire activity is essential for any future management plan that aims for a sustainable future.“

Minor comments:

R1.4 - P4260,L25–P4261,L6: The methodology is not entirely clear. E.g. what is the temporal resolution of CLIMBER-2 (I understand it's not annual, but is it... 50-yr?); it sounds like the 50-yr base climate recycled over and over in sequence, but I'm not entirely sure; I don't exactly understand how and why the “data presented here are smoothed...”. To be clear, I am not concerned that the methodology is flawed, it just isn't explained clearly enough here. Finally, even if the method is mostly described by Brucher et al. 2014, some additional detail would be helpful, e.g. what variables are used to force the CLIMBER-2 model (solar, volcanic, and CO₂, as in the PMIP3 simulations?).

We extended our method sections, which hopefully makes the description now clearer:

“The simulations are setup similar to Bruecher et al., 2014. The base climate is represented by 50 years extracted from a MPI-ESM CMIP5 simulation, representative for the climate of the early industrial period (1850--1899). CLIMBER-2 simulated climate anomalies are added to this 50 year spanning annual varying base climate. The resulting climate is used as forcing for JSBACH. This approach is required as CLIMBER-2 does not simulate year-to-year climate variability, which is however critical to simulate land and vegetation dynamics in JSBACH. Unlike in Bruecher et al. 2014 we choose not a year randomly out of the 50 year base climate, but applied a constant base climate cycle, i.e. every 50 years cycle followed the same sequence. As a result the data presented here does not have any year-to-year variability when smoothed over 50 years or a multitude thereof.”

R1.5 - P4262,L6-8: Again, I do not understand what was done (seems related to comment above).

We extended the description of the factor experiments (see also comment R2.2): “This experiment serves as reference for the factor experiments. In the factor experiments one single forcing factor is varying over time. The others are prescribed continuously over the simulation period as a constant 50 year cycle, representative for 8K conditions (7999 to 7950 cal yr BP) and are taken from the output of the reference experiment FMW.”

R1.6 - P4262, L15-17: Fig. 1 shows only simulated data, so it is not suitable for illustrating whether the model does a good job to “capture major burning regions...” For this, a comparison to GFED or another observation-based fire map would be required. As noted above, I do think the authors should consider making such a comparison, even though there are caveats as they note.

Following the reviewers suggestion we included the burned area as reported in the GFED4 database into Figure 1 to facilitate comparison between our simulation and present day satellite based observed burned area.

P4263,L19-21: At least one point about wind speed bears further discussion: Is the lack of effect due to little change in simulated wind speed over the Holocene, or insensitivity of the fire model to wind speed? This is similar to the overall comment above about sensitivity vs. variability contributing to the importance of forcing variables, but exacerbated in this case by the fact that the wind data are not presented at all.

The model is actually sensitive to wind speed. We discuss this in more detail in the Method section in the revised manuscript: “A high wind speed increases the fire spread and the burned area. An increase in wind speed from 15 to 20 km/hour, for example, results in an increase in the fire spread rate of 25% based on observations (Arora and Boer, 2005).” (see also comment R1.1). The simulated wind speed is however, not strongly changing over the Holocene for the analysed regions. We added this to the Result section: “All regions, have in common that changes in wind speed between 8000 and 200 cal yr BP do not significantly impact the fire activity, as the simulated wind speed changes over the Holocene are very small (less than 0.1% for the regions analysed). Therefore, the wind speed control on fire activity will not be further discussed for this study.”

R1.7 - P4263,L22-27: Based on Fig. 2a, the increase in the FMW experiment is <10%, but cited here as 11%—please double-check.

We corrected this. The numbers were taken from Table1, based on the non-smoothed data and therefore not directly comparable to Fig. 2.

R1.8 - P4265,L5-13: Can you confirm that the appearance of interactions is not due to representing the simulated area burned as a % change relative to 8000 BP? If the different experiments have different absolute values of area burned, then the % change numbers will not add up, even if no interactions are occurring. Regardless, an alternative standardization for the simulated data might be preferable, as it is a bit odd to compare the simulated data as ratios (% change) to differences (z-scores) in charcoal data in Fig. 2. (And in any case, the details/rationale for the standardization used need to be described in the Methods and/or figure caption—they are not currently).

All experiments start from the same control. As such the percentages (relative changes) do add up. We describe this now in more detail in the revised manuscript. "Results are presented relative to the 8K state (7900--7999), which is identical for all simulations." Z-scores are, however, no differences but normalized changes. This is also explicitly stated in the manuscript: "Z-scores are a standardized measure frequently used by the palaeofire community to compare aggregated values of past fire activity. They are, however, no quantitative measure and therefore cannot be related to absolute changes (Power et al., 2010)."

R1.9 - P4265,L18 and subsequent: Similar to the previous comment, the representation of the forcings as relative % change in Fig. 2 hampers comparison of the role of different forcing variables on simulated area burned across regions. E.g. Temperature is a key control in N. America, but appears to have a minor effect in Aust. Monsoon region, but it is difficult to judge this difference since both temperature series are represented as % change relative to an unknown absolute value. Again I would recommend showing the actual forcing data, or at least using a difference (vs. ratio) so that anomalies are given in familiar and comparable units.

We added a paragraph to clarify how the results are presented in this analysis and refer to Bruecher et al., 2014 for the absolute changes: "Results are presented relative to the 8K state (7900--7999), which is identical for all simulations. Absolute changes for burned area and a number of external forcing factors (precipitation, surface temperature, gross primary productivity, biomass carbon, soil carbon) are presented Figure 2 in the supplement of Bruecher et al., 2104." The reference to the absolute changes is now added to the caption of Figure 2.

R.1.10 Fig. 2: Yellow lines (charcoal data) not defined. Also, I believe citation should be Marlon et al. 2013, not 2009 (as in the text).

We corrected the reference and defined the yellow line in the revised manuscript (see also comment R2.9).

This short paper follows one from Brücher et al. (2014) where the same authors studies how burned area and fire emissions have varied during the Holocene. The focus of this paper is to disentangle what drives the variations found in their earlier work focusing on the effect of fuel availability, moisture, and wind speed. Given that climate variations over the Holocene were substantial enough to impact fires this is an interesting research area and after a substantial revision this paper would be a welcome addition to the literature.

My main critique is that after reading the paper I still have many questions about the findings and implications. This is partly due to the paper being so short. Splitting and expanding the Conclusions section into a longer discussion and shorter conclusion section would be helpful. Things that require additional discussion include:

R2.1 - How representative is the comparison between charcoal and models? In Australia for example the charcoal records are mostly in the SE while most fires burn in the N. For the comparison the authors could sample only those grid cells where charcoal records were derived from for example.

The representativeness between charcoal data and model has been assessed in the Bruecher et al., 2014. We pick up on this and state in the revised manuscript: "This comparison is done on a regional average even though the charcoal data are very site specific and some regions are only represented by a few charcoal sites, e.g. Sub-Saharan Africa has only 3 sites. The coarse resolution of the climate model, however, does not allow a site specific evaluation as the single site conditions (precipitation, temperature, etc.) can not be explicitly resolved, whereas region specific characteristics are in general expected to be captured."

R2.2 - Conceptually, it is difficult (at least for me) to understand how fuel availability and moisture can be seen separately. Aren't those tightly coupled? This is discussed in the Conclusions section but it would be better in the methods section.

In the revised manuscript we discuss the setup of the factor experiments, which allows us in the model to separate the fuel availability and fuel moisture control on fire activity, in more detail in the method section: "In experiment FMW all parameters controlling fire activity in the model are varying with time, i.e. the full set of forcing is applied and the simulated burned area represents fire activity over the Holocene. This experiment serves as reference for the factor experiments. In the factor experiments one single forcing factor is varying over time. The others are prescribed continuously over the simulation period as a constant 50 year cycle, representative for 8K conditions (7999 to 7950 cal yr BP), and are taken from the output of the reference experiment FMW."

R2.3 - The modeled short-term variability is in general very low (gradual changes) while the charcoal record gives much more fluctuations. This requires discussion. Is it the smoothing? Are not all climatic changes represented in the model? Etc.

We extended the description in the Method section: "Unlike in Bruecher et al. 2013 we choose not a year randomly out of the 50 year base climate, but applied a constant base climate cycle, i.e. every 50 years cycle followed the same sequence. As a result the data presented here does not have any year-to-year variability when smoothed over 50 years or a multitude thereof." (see also comment R1.4).

R2.4- What are the implications of this study? The abstract ends with a statement that the findings are important to project future climate but there is very little about this in the main text. Including this would increase the impact of the paper. Clearly this requires also a balanced discussion between the role of climate and the role of humans.

We extended the discussion of future implications of our study (see also comment R1.3).

Minor comments:

R2.5 - P4260 L21: I assume it is 5 degrees instead of 51 degrees

51 is actually correct.

R2.6 - P4261 L12: "Fuel availability is simulated as a function of aboveground biomass". Since in many savanna ecosystems trees don't burn (and higher tree densities of- ten lead to lower grass fuel loads which do burn) biomass is not the same as fuel availability. Please change or discuss in more detail.

We agree that aboveground biomass is only a crude proxy for the fuel availability. To discuss, however, the limitations of the fire model in detail is beyond the scope of our study. Here we refer the reader to the fire model publications (Arora and Boer, 2005, Kloster et al., 2010, Li et al., 2013), which discuss in detail the weaknesses of the fire model applied in the current study.

R2.7 - P4261 L15: "Human ignitions are not accounted for". I think this is fine for the purposes of this study given the limitations outlined by the author but it does limit the extrapolation to the future which should be discussed, see for example a recent paper by Andela et al (2014) in Nature CC showing that the human factor can already be seen in the satellite record in Africa.

We extended the discussion on the importance of our findings to assess future fire activity, including a discussion on the dominant role anthropogenic factors will play in the future (see also comment R1.3 and R2.4). We did also include the reference to the Andela and van der Werf (2014) study, which shows the dominant impact of landuse for Southern Africa already for present day conditions.

R2.8 - P4263, L10: "This trend fits to with an increase", please rewrite. Also some minor wording issues, for example inline -> in line (several occasions), therefor -> therefore

We corrected this in the revised manuscript.

R2.9 - Figure 2: These figures are too small to interpret easily. In addition a legend would be helpful for quick interpretation (right now the yellow line is not labelled, I assume this is the charcoal index, and in addition this color is difficult to see). It might be good to consider to make these separate figures

The figure will be enhanced in the revised manuscript. In addition, we changed the axis labeling to make the figure more readable and included a legend. The yellow line is now labeled and also details on the smoothing of the data are given in the revised manuscript (see also comment R1.10).