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Interactive comment on "Controls on fire activity over the Holocene" *by* S. Kloster et al.

Anonymous Referee #1

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In their manuscript "Controls on fire activity over the Holocene," Kloster et al. describe paleo-fire simulations made using a 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.

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.

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.

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.

Minor comments:

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?).

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

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.

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.

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

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).

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.

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

Interactive comment on Clim. Past Discuss., 10, 4257, 2014.