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Discussions

Interactive comment on "Climate vs. carbon dioxide controls on biomass burning: a model analysis of the glacial-interglacial contrast" *by* M. Martin Calvo et al.

Anonymous Referee #1

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M. Martin Calvo and colleagues present an interesting and compact paper with a clear message: decreased biomass burning rates in the LGM compared to the PI was largely due to ecophysiological effects (lower CO2 concentrations) instead of being due to a different climate. Another take-home message is that also in the future, enhanced CO2 concentrations will lead to more fuel and thus more fires in certain areas.

The paper is well written and easy to follow. I do have some issues with the methodology, some of them being fundamental. In addition, there is a lot of referencing to earlier work from Prentice and Harrison and in some occasions it might be appropriate to credit work from other scholars, for example in the opening sentence of the introduction (Seiler and Crutzen 1980 instead of Prentice et al 2011 for example).

The two main issues I have with the paper:

1) In South America and Africa the amount of flammable area (grassland / shrubland / savanna) is substantially higher in the LGM than in the PI (Figure 2) so one would expect higher emissions from these areas, although a drier climate would offset some of this increase. Figure 3 tells us that emissions from these sources are very small though and it is highlighted in the text (2578-16) that savanna fire emissions are most reduced by changing CO2 concentrations. That is strange given that most savannas consist of C4 grasses which should cope better with lower CO2 concentrations than other biomes. I realize this comment is somewhat speculative but there is very little text, data, or figures to support the claims made in the paper at this level. This should be addressed otherwise the paper is too much of a "we ran a model, changed some input datasets, and this is what came out" paper which does not justify the work done. One possibility would be a table with for each biome the total area, average fire return interval, and fuel load/consumption for both LGM and PI to gain more insights in what has actually happened in more detail than what is provided now.

2) the correction factors indicate that something is fundamentally wrong with the fire module in LPX. The authors acknowledge LPX has some issues and everyone realizes that reproducing fire patterns is difficult, but conversion factors up to almost 400 (Table 1) are worrying and to me it is difficult to justify using this model to investigate the even more complicated fire - climate -vegetation interactions.

Moreover, I think the conversion factors are not calculated or applied in the right manner; the middle panel in Figure 3 represents emissions after the correction factors have been applied and should thus give relatively similar results as global models based on satellite data do. This is not the case. For example, LPX estimates that tropical savannas are only responsible for 20% or so of emissions while in global fire assessments it is the major emissions category. And tropical forest fires are the main source of carbon losses in LPX after applying conversion factors even though the authors mention they have excluded deforestation fires; tropical fires in natural state rarely burn.

One final comment about calibrating the model: the co-authors have published in the recent past about PI burning rates being higher than current rates. In this paper, however, the PI outcomes are calibrated to mimic current rates. This requires some discussion.

Other: Savannas fires don't leave much charcoal so they are poorly constrained in the charcoal database, this should be acknowledged as it impacts the comparison and confidence we put in the runs with LGM climate and CO2 concentrations give the preferred results.

Figure 1 - since this paper focuses on fires, it would be nice to compare the carbon pools that are most relevant for fires (e.g., surface litter pools) with other literature if that is possible

Figure 2 - it may be due to the conversion to pdf but the graphs require some work (for example, the legend is difficult to read)

Figure 2 + 3 - difficult to distinguish the colors because there are a lot of classes, please consider combining a number of classes (tundra and shrub-tundra for example)

Figure 4: The ST region is much higher than in satellite-based assessments (please discuss why) and the variability is rather large for the different climate models (also apparent in Figure 5). Isn't that at odds with the message that "it is all CO2'?

2577-15: "depending on the model" -> maybe rephrase so that it is clear that you mean climate model

2574-21: NO2 -> N2O

2578-12: this sentence seems to indicate that savannas are a forest biome which is not the case

In summary, based on my reading and interpretation two major issues have to be addressed, that is why I recommend major revisions. I realize some of my worries (for

example the very large correction factors that have to be applied) are beyond the scope of the paper but they should be acknowledged much more clearly and potential reasons should be discussed. Most importantly, do they impact the main conclusions? My concerns regarding the potential mistakes with calculating or applying the conversion factors, which give a different distribution of fire activity than the reference dataset, need to be addressed. In case this comment is based on a misunderstanding from my side please use the discussion section here and I would be happy to re-evaluate my decision.

Interactive comment on Biogeosciences Discuss., 11, 2569, 2014.