Biogeosciences Discuss., 11, C3362–C3369, 2014
www.biogeosciences-discuss.net/11/C3362/2014/

Biogeosciences
Discussions
Open Access

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.

M. Martin Calvo et al.

m.martin-calvo@imperial.ac.uk

Received and published: 9 July 2014

We have carried out a major revision of the manuscript in response to the helpful comments of the anonymous referee. In doing so, we have taken the opportunity to improve the manuscript's readability in several respects. All of the review comments that are critical of our manuscript are reproduced below between quotation marks, followed by our responses.

'. . .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).'

Prentice's and Harrison's work on vegetation/fire modelling and palaeodata synthesis represents a key part of the background for this paper. However, in response to this comment, we have added references to other studies, including the pioneer analysis by Seiler and Crutzen (1980).

'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.'

Tropical savannas do show an increase in the fraction of their NPP that is consumed by fire at high CO2. These ecosystems are characterized by a mixture of C4 grasses with C3 trees. Even though the grasses burn readily, there is always some combustion (and mortality) of trees, and this contributes disproportionately to the fire-related carbon emissions from savannas. LPX explicitly considers the metabolic differences between C4 and C3 plants, and hence it models the differential influence CO2 has on their performance. Under low (LGM) CO2 concentration, the trees become less competitive relative to the C4 grasses (which cope well with low CO2, as the reviewer points out) and therefore they become less abundant.

'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.'

In response to this suggestion we have added a new table (Table 2) that includes the percentage of area burnt by biome, the biomass burned per unit area, and the percentage of annual net primary production consumed by fire. (The average fire return interval in the 'model world' is simply the inverse of the fractional burned area.) Two new paragraphs have been added, summarizing the findings from this Table.

'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.'

LPX is one of the most advanced global models of the climate-vegetation-fire interaction. Nonetheless, there are problems, which will have to be addressed in future model development. One of the most important problems with LPX is that it fails to simulate sufficient burnt area in closed forest, while greatly overestimating burnt area in non-forest vegetation. This issue was already discussed in Prentice et al. (2011). However, the general spatial patterns of fire are remarkably well simulated by LPX, as shown by Kelley et al. (2013), despite these quantitative biases. So we consider it appropriate to deal with the quantitative mismatch by means of a correction based on the ratio of modelled to GFED fractional burnt areas within each biome. Nonetheless, obviously this approach introduces a big approximation. We have now addressed this issue as a caveat in the revised Discussion. Note that these ratios depend on the uncertainties of both model and data, as well as differences in biome distribution between them (biomes calculated from LPX simulations were applied in both cases). We have made some changes in the choice of regions used to determine the corrections, and of biomes for which a realistic correction seems to be possible. In the new results, as shown e.g. in Figure 3, it is extremely clear that the "raw" simulation under LGM conditions is dominated by emissions from non-forest biomes whose emissions are greatly over-estimated under present conditions (however this bias is assessed). After correc-

tion of the bias, the result is an unambiguous reduction of around a third in total global emissions, a result that is consistent across climate models, and broadly consistent with the charcoal record.

'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.'

We checked our calculations and there was indeed an error, which we have corrected. The corrected emissions are now closer to those derived from GFED. But there are some remaining differences between the GFED emissions on a biome basis and the calculated (CRU-based) emissions after correction, for several reasons. First, our estimated corrections were derived only from those regions in which the biome in question occupies a large area. (We made this restriction so as to minimize the impact of small mismatches between the modelled distributions of biomes and those assumed by GFED; such mismatches are unimportant for continents where a biome is widely distributed, but can cause difficulties for smaller areas.) Second, for the same reason, we did not apply any correction to biomes that occupy very limited areas worldwide. Third, the PI simulation treats all areas as natural vegetation, whereas the calculation of conversion factors was performed on the basis of natural vegetation areas only. And fourth, the ratios were calculated using present (1997-2011) simulations and not the PI detrended simulation, in order to get more realistic ratios by comparing similar data. So while the correction brings total modelled emissions (and emissions from major biomes) closer to the GFED values, as is visually clear from the revised Figure 3, it does not and would not be expected to render them identical. We hope that the revised text shows the methodology applied more clearly. We define tropical forests broadly to include tropical dry forests, which do indeed burn naturally. Tropical dry forests are as a substantial source of fire-related CO2 emissions today. We now make this clear and have provided references. The correction factors were based on the total area of all tropical forests (with deforestation fires excluded).

'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.'

This comment draws attention to a significant, and essentially unavoidable, uncertainty in our results. There simply is no quantitative information on PI burning rates that is of comparable precision to the contemporary satellite data. We were therefore obliged to assume broad similarity between PI and contemporary rates. Marlon et al. (2008), and others, have found evidence for effects of land-use change including a recent widespread decline in biomass burning, attributed to the intensification of land use. But given that our interest was to compare 'natural' fire regimes under LGM and recent CO2 concentrations and climates, and that the 'signal' of change (observed and modelled) between LGM and late Holocene times is large, we expect that the comparison is valid to first order. We have added this caveat to the 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.'

Charcoal records do not give a fully quantitative measure of burnt area (Power et al. 2008) because fires in different biomes produce different amounts of charcoal. However, in our modelling approach, we apply published emission factors for different biomes to quantify the total carbon release associated with burning of each plant

functional type (this is internal to LPX). We assume that this should be systematically related to the amount of charcoal produced.

'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'

Unfortunately there is no observational information on long-term changes in surface litter pools. Models produce this quantity, but most of the studies looking at carbon pool differences between LGM and PI concentrate on the total pools, not separating the surface litter pools from the total. Ciais et al. (2011) cited an array of publications comparing the total pools. In the text, we have added the results of Prentice et al (2011b) for surface litter pools.

'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)'

The graphs have now been recreated with better resolution and readability.

'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)'

There are no obvious options for combining classes, apart from the one mentioned by the reviewer; this change alone, however, would not simplify matters much. Instead of combining classes, therefore, we have tried to optimize the clarity of the Figures, taking special care to optimize the distinctness of the colours representing the classes. We note also that some potential readers are likely to want to see at least this level of resolution of biomes – it is already very coarse from the perspective of most palynologists, who are accustomed to making much finer distinctions among vegetation types based on their data!

'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'?'

For pre-industrial simulations, most of the tropical forests are concentrated in the southern tropics. They released greater amounts of CO2 than is the case today due to past deforestation having reduced the area of forests available to burn. We have now included numbers in the text that show that there are indeed variations among the climate models' estimates of biomass burning. Nonetheless these are smaller than the effect of artificially imposing PI CO2 on the LGM climate.

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

Now rephrased.

'2574-21: NO2 -> N2O'

Done.

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

We have changed this wording.

'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?'

We realize that a number of points were expressed too briefly in the Discussion Paper, and need to be clarified. We have therefore added explanations of a number of points, as mentioned above. In particular our revised text addresses the issue as to the robustness of our conclusions with respect to important methodological questions, most importantly the use of correction factors.

'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.'

We have fully addressed this issue – including correcting a significant error in our original application of the conversion factors. See above for details.

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