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# *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.

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We have carefully read and evaluated the helpful comments exposed by the anonymous reviewer 2 one by one, which has let us improve the contents of this study so they are clearer. The reviewer's comments are presented below between quotation marks, followed by our responses.

'The Abstract states that modelled global fire CO2 flux is 70-80% lower at the LGM than during the PI period, and raising LGM CO2 to PI levels increases fire fluxes by 4-10x! This is caused by a CO2 change from 185 ppm to 280 ppm (i.e. a 51% increase). However, oddly, the results as reported in the paper itself do not seem to agree with



these headline responses. For example, Figure 4 shows an increase in global fire fluxes of 1.8-3.8x due to PI as opposed to LGM CO2 at the LGM. Furthermore, LGM fluxes are 33-54% lower than PI. It is very odd this does not match the text in Abstract. However, I am most concerned about the fact that the major effect occurs only after corrections to the model biome-level outputs have been applied. Figure 3 shows that the uncorrected model output shows no reduction in the CO2 fire flux between PI and LGM, and the effect of PI CO2 on the LGM flux is to increase it by 19-50%. The dominant (uncorrected) flux comes from the 'Dry grass/shrub' biome, and the correction reduces this by 84%. At the same time, the correction increases the tropical forest flux by _4x, making this biome dominate the LGM-PI difference. Tropical forest fire fluxes are negligible at the LGM with LGM CO2, but increase to PI levels with PI CO2. The correction then increases them to 4x these values, and so the effect of PI CO2 at the LGM is almost entirely due to this correction. My concern is therefore that if the baseline CO2 fire flux from tropical forests is incorrect by a factor of 4, how can we have confidence that the anomalous behaviour of this flux under changed productivity is reasonable? The authors need to very carefully explain the reasons for the incorrect baseline, and why a simple single correction factor is justified before this work can be published. Alternatively, they need to improve the model so that the correction factors are not necessary.'

The statement in the original Abstract (that PI CO2 caused an increase in emissions by four to ten times) was unfortunately incorrect – this unrealistically large figure arose due to an earlier coding error that we had corrected, but we inadvertently left it in the Abstract. The Abstract now gives correct numbers. The reviewer is right to observe that the correction procedure is a necessary step in our analysis. However, as the new version of Figure 3 makes abundantly clear, the model greatly over-estimates the fire-related emission from (above all) dry grass and shrub, temperate parklands and deserts. As these biomes are modelled to occupy large areas at the LGM, this over-estimation clearly matters. Inspection of Figure 3 shows that a reduction of the emissions from these biome alone (commensurate with the large difference between

GFED and CRU-simulated emissions) would make the crucial change from LGM fires apparently emitting more carbon than present fires, to the reverse prediction, which is consistent with the charcoal record. It is also true that the large magnitude of the increase in fire emission due to the change in CO2 concentration (under LGM climate) partly depends on the correction factor applied to tropical forests. But the key point is simply that with PI CO2 the model produces large areas of tropical forest, which did not actually exist at the LGM, and which the model does not produce when LGM CO2 is applied. It was already known, for possible reasons discussed by Prentice et al. (2011a), that LPX underestimates fire fluxes from forested regions while overestimating them for grasslands. The model nonetheless represents the state of the art in global vegetation-fire modelling, and performs dramatically better than LPJ in predicting present fire regimes (Kelley et al., 2012). Recent improvements in the simulation of fires by LPX in Australia (Kelley et al., 2014: a new reference now added) have not yet been tested for global application but they do confirm that the biases in the version used here are consistent and systematic. LPX moreover has shown a good ability to calculate carbon and water fluxes, as well as spatial patterns of vegetation and fire regimes (Kelley et al. 2012, Prentice et al. 2011a). Confidence in the model's predictions of burnt area after correction rests on these considerations, plus the fact that the spatial patterns of burnt area are well simulated even if there are large biases in the amounts (Prentice et al., 2011a). With correction for known biases, we think that our results represent the best that can be achieved for such a study at the present time.

'Some other points: p3.2 It is stated that burnt area in the most relevant aspect of biomass burning for the carbon cycle, and Prentice et al. (2011a) is cited as evidence. I read this article, but it does not really say this. In fact, it shows that globally there is no reliable relationship.'

This is a very good point: we realize now that we made a mis-statement. For the atmosphere it is primarily carbon emission, rather than burnt area, that matters and the relationship between them is not simple because of the large differences among



biomes in the amount of combustible material. We have modified the Introduction to make the distinction and logic clearer.

'p.3.4 What about the role of wind speed? '

We mention this now.

'p3.7 How does weather control ignition? Presumably through lightning? If so, please state explicitly.'

Weather does indeed control ignition through lightning. This point has been now been made explicitly.

'p3.10 Independently of what?'

It means that CO2 affects both vegetation type and productivity, independently of burned area. We have clarified this point.

'p3.18 I am not sure this is true. The model would also have to represent the effects of CO2 on fire, and this would not be known from patterns, and including processes mechanistically needs testing.'

We have amended the text to recognize that the model must also have a sound basis for the modelling of CO2 effects.

'p3.20 Well, if the model has been shown to capture the climate signal, it could be used to ascertain the CO2 one. The argument here seems awkward in that models are claimed as useful, while the observations are described as being inadequate.'

It is inescapable that the record of burned area is too short to infer CO2 effects. Again, we have amended the wording to avoid awkwardness.

'p3.28 'and' > 'but' p4.8 'showed' > 'claimed'? '

Both changes made. They make our meaning clearer! Thank you for pointing out these improvements.

'p4.19 But, surely the spatial relationships help as CO2 is not expected to affect these (except C3/C4 contrasts)?'

This is a good point: the problem is in trying to interpret aggregate time series. We have modified this statement accordingly.

'p4.21 'However' with respect to what? Awkward semantics. . .'

Agreed. We have deleted the 'however'.

'p6.25 Is such a coarse time resolution appropriate for fire risk? Also, what about number of wet days? I thought these were part of the fire model forcing?'

The time resolution of the model is daily. It includes a standard weather generation approach that converts monthly data (total precipitation and number of wet days) into daily precipitation events. This conversion is vitally important for fire simulation because fuel dryness depends on the length of runs of consecutive dry days. We have added text to clarify this important point.

'p7.9 I am uncomfortable with using a climate variable to classify the output of a vegetation model!'

It is inelegant, but necessary to deal with a limitation of current DGVMs, when output is to be represented as 'biomes'. The reason lies in the coarse discrimination of plant functional types in current models. In particular, these models generally lack a class of plants specific to arctic and alpine environments. So if it is considered important to differentiate forest from tundra (as is done in all biome classifications) then an additional criterion has to be introduced, to represent the limit of forests. We have used a growing degree-day (GDD) criterion as is typically used in static biogeography models. To give a concrete example, both tree species of Betula characteristic of boreal forest, and dwarf shrub species such as B. nana, are modelled as 'boreal summergreen woody plants'. But vegetation dominated by dwarf species is found beyond the Arctic treeline, whose location is represented well by this GDD threshold. We have added a statement



on this point.

'p7.10 How is this justified? Why not use the average of the climate models to run the vegetation model?'

Our approach is more correct: when dealing with non-linear processes (as represented in the vegetation model) it is generally considered to be desirable to average the results after running the model, rather than running the model with average inputs.

'p7.11 Was this described earlier? Clarify exactly which simulations were performed in one place.'

We have added text earlier in the document so that this is now clear.

'p8.7 There is a great need to clarify exactly why these biases occur. Should we be concerned about them in the context of this study? '

This is a difficult point because we do not know exactly why these biases occur (although the recent work for Australia, published since our preprint, by Kelley et al. 2014, gives some clues). If the causes were obvious, the biases would already have been corrected! In the revised text, the point has been addressed in the following ways: (1) we have referred to Kelley et al.'s new work and commented on the possible causes of the biases; (2) we have taken care to show how our qualitative conclusions arise.

'p8.9 But, if these biases are related to productivity, then surely this compromises the ability of the model to represent effects of changes in productivity on fire emissions?'

The model performs well in terms of simulating global productivity patterns, as documented by Kelley et al. (2013). The non-linear response of fire regimes to underlying variables including NPP however appears to have translated a moderate systematic bias in NPP in fuel-limited regions into a considerable over-estimation of their burnt area. This is illustrative of the difficulty in accurately modelling such a highly non-linear process as fire. Nonetheless, we have no reason to doubt the model's ability to represent the key features of spatial pattern in NPP; after biome correction, it is these spatial patterns (rather than absolute values) that are operative.

'p9.21 Give the absolute fractions at LGM for LGM CO2 and PI CO2 forcings.'

Done.

'p10.3 Please supply more details.'

Done.

'Also, the figures are not all clear, and switch to B&W for no obvious reason. 'CRU' is not described as such in all legends.'

We have improved the Figures generally. However we have restricted the use of colour to discriminate biomes (in a consistent way). We have used black-and-white in other contexts so as to avoid potential confusion between colours used for biomes and colours used for other purposes.

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