

Interactive comment on “Soil carbon response to land-use change: Evaluation of a global vegetation model using meta-data” by Sylvia S. Nyawira et al.

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Response to T. Pugh of the paper entitled "Soil carbon response to land-use

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change: Evaluation of a global vegetation model using observational meta-analyses"

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Below are the reviewers suggestions (***bold italic font***) and our responses to each point (normal font). In some of our responses, we have cited text from the revised manuscript (*italic font*).

Nyawira et al. develop a framework to evaluate the response of soil carbon stocks in the DGVM JSBACH to land-use change, using meta-analyses of observations of soil carbon stock change. They find that the baseline model is unable to reproduce the observations for most transition types tested, but that inclusion of crop harvest and exclusion of fires on pasture land notably improves the fit of the model response.

The analysis has been carefully executed and the manuscript is well written. It provides a useful framework for evaluation of soil carbon response to land-use change, which is generally poorly evaluated in DGVMs used to provide land-use emission estimates, despite constituting a substantial part of the overall emission. I am happy to recommend publication, subject to addressing the following minor comments.

Thank you for your comments. We are happy that you find our results worth of publication in Biogeosciences.

Section 2.2 -In order to understand the differences between the various land-use types considered, more information about the different PFT types is required. In particular, how do C3/C4 grasses, C3/C4 pastures and C3/C4 crops differ from one another? I suggest to add a table listing the differences in any important

C2

PFT parameters, or other parameters which may be important to the land-use type (e.g. different soil decomposition rates?).

We have added two sentences in this section. In paragraph one we explain the difference in terms of photosynthesis: *“The PFTs differ with respect to their phenology, albedo and photosynthetic parameters; photosynthesis is based on Farquhar et al. (1980) for C3 plants and Collatz et al. (1992) for C4 plants.”*

In paragraph two we added a sentence to explain that there is no distinction of the decomposition rates among the different PFTs, but only between woody versus green litter. *“Non-woody litter has the same decomposition rates for all the PFTs, while the decomposition of woody litters depends on the woody diameter”.*

Pg. 4, line 21 - I wasn't quite sure here if the forest PFTs had been extended to cover areas not presently covered by forest, or not. Can the authors clarify?

In the idealized simulations we extended the vegetation type, e.g., forest, to cover the entire globe in our simulations. However, our criteria for the grid cells selections based on climate or historical LUC removes regions where the forest do not exist. We have clarified this in section 2.3.

Pg. 5, line 12 - 50% seems a high proportion of crop NPP to be allocated belowground. This will have a substantial influence on the size of the flux lost to harvest and should therefore be discussed in relation to published literature in the discussion section.

We are sorry this was a mis-communication from our part. There is no 50% aboveground and 50% belowground allocation of NPP in the model. The choice of the 50% harvest is based on root to shoot ratio and not only accounts for root biomass, but also for other unharvestable plant parts. We have also added a discussion on the uncertainties associated with this parameter choice. We have the sentence describing the

C3

aboveground and belowground allocation and added the following sentence;

The choice to transfer 50% to the litter is approximated from the average root to shoot ratio of several crop types (Extended data Fig. 2 in Gray et al., 2014). The 50% accounts for root biomass, unharvestable parts of the stem biomass being left in the field and a potential return of carbon to soil in the form of manure.

Pg. 5, line 27 - I think these are grid-cells where just the relevant transition type has taken place (based on Fig. S2), and not where any LUC has taken place at all? This isn't clear to me from the text. Also, on first reading I thought climate and LUC criterion were being applied simultaneously, and it only later became clear that they were being applied separately.

We have re-written the sentence to make it clearer that it is where the relevant transition has taken place. In addition, we have made it clear that the two criteria are independent.

Pg. 6, line 4 - Should beta have units of length? Also, please define the units of d_0 (presumably cm).

We have removed the scaling section from the manuscript.

Pg. 6, line 13 - How do you sample simulated soil C changes over the ages? Do you take a simple mean over the age range in the observations, or do you weight the mean by the number of observations in each age range? I would argue the second is much better, if you have the data to do it.

We have added two sentences to clarify that the second approach is indeed taken.

“For this we use the age represented by each site in the meta-analyses to select the transient years in the simulations to include in averaging the soil C response. We average the soil carbon response over these years and spatially for the selected regions.

C4

This average represents the simulated soil C response over the different ages represent in the meta-analyses.

Section 3.2, para. 1 - I think you can be a bit more assertive here in saying that the reason for the results from the crop to grass simulations is fire. That seems to be very clearly demonstrated at the end of the paragraph, and I'm not sure why the section beginning "we suspect" (line 11) is included.

We have re-written the sentence.

Pg. 8, line 14 - Should Fig. S2 be cited here?

We have included the figure citation.

Pg. 9, line 24 - What is meant specifically by "forest floor"? Surface litter?

Correct. We have changed forest floor to surface litter.

Pg. 9, line 27 - Is the larger NPP for forests than pastures in accordance with the literature? Would be good to discuss this briefly with some references.

We added a discussion on this in section 4.2.3.

"For most of the considered regions in the tropics, the larger simulated NPP for forests compared to pastures is consistent with other observations (Smith et al 2012). Murty et al. (2002) associated the observed increase in soil C following conversion of forest to pasture with low initial content of soil C, application of fertiliser and careful management that avoided overgrazing. Table 3 shows low previous land use soil C for forest to pasture compared to forest to crop in the meta-analyses. However, this is not the case

C5

for the simulated soil C in the considered regions.

End of section 4.1.4 - Absolutely agree with this sentiment, but shouldn't we then be aiming for a more stringent test than getting within the very large standard deviation that results from this small-scale heterogeneity?

We agree with the reviewers sentiment. However, in our model-data comparison we do not use the standard deviation as a measure of agreement between the simulated results and the meta-analyses. In this part we discuss that the model may not capture spatial variability in soil C changes due to other missing processes, which the meta-analyses may capture.

Pg. 11, line 23 - What exactly is meant by "top soil"?

We have clarified that the top soil refers to the upper 30cm.

Section 4.3 - I agree with the general statement regarding absolute estimates, but the way this section is written seems to imply that JSBACH was successful in capturing the observations in this evaluation, which I feel would be stretching it a bit for several of the transition types, especially grass to crop (based on Fig. 2)

The reviewer is right that our phrasing was misleading. We have re-written the section as follows:

"Even though DGVMs provide land-use-related absolute soil C changes, our comparison focused on relative changes. This is the preferred variable in the meta-analyses because spatial heterogeneity partly cancels in relative terms when two sites in close proximity are compared to each other, as done in chronosequences. Only relative changes allow for deriving robust carbon response functions (Poeplau et al 2011). In the jsb_drvn_harv simulation, the equilibrium changes indicate a decrease in soil C of

C6

about 11 kgC m^{-2} and 3 kgC m^{-2} for forest to crop and grass to crop, respectively, in the temperate region. The decrease for forest to crop in the tropics is about 9 kgC m^{-2} (Fig. 1b). The reverse LUCs result in soil C increase of about the same magnitude. Because DGVMs are unaffected by small-scale spatial heterogeneity, their estimates of absolute changes are expected to be more robust than those of meta-analyses and therefore better representative for global carbon responses. After successful evaluation against relative changes, DGVMs can therefore be used to assess large-scale soil C changes in the absolute terms that are relevant for carbon budget estimates.”

Table 4 - I'm not clear on the logic of having this table in addition to Table 3. It would seem more helpful to add the *obs_drvn* and *jsbach_drvn_harv* data to Table 3 (appropriately adjusted for 30 cm depth), to allow them to also easily be assessed against the observations.

We have removed the model scaling section following concerns raised by another reviewer and included the meta-analyses soil C densities in Table 4 (now Table 3).