Earth Syst. Dynam. Discuss., 6, C1192–C1198, 2016
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Earth System Dynamics Discussions
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ESDD
6, C1192–C1198, 2016

Interactive comment on "Deforestation in Amazonia impacts riverine carbon dynamics" *by* F. Langerwisch et al.

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Received and published: 9 February 2016

We thank the reviewer for the time she/he took and for the very helpful comments provided, which will help us to improve the manuscript!

Anonymous Referee #2 (Received and published: 12th January 2016) The authors claim to have identified that deforestation in Amazonia impacts riverine carbon dynamics. This, of course, is unexpected and demands a sophisticated model to study. The work is based on the river carbon model RivCM and was published in the same journal and reviewed. I agree with the critical points of the reviewers there. The model provides output for CO2-outgassing and lateral C-fluxes on a monthly basis.

The model represents a setup where PCO2 increases with increasing T and CO2-atmo

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or lateral C-export. So the result that in the future CO2-outgassing is increasing due increase in T, or PCO2-atmo or lateral C-export is surprising to some. A bulk evaluation is given in Table 4 of the previous publication, where the authors explain RivCM, with a view aggregated data, showing that the model cannot capture the measurements and estimates for outgassing. The model simulates 1/6 of outgassing at present days. This is not too close to measurements and detailed extrapolative analysis in the literature.

Reply: Part of the model error arises from validating it at the entire basin scale. To address this issue more adequately, because similar points have been raised by reviewers of Langerwisch et al. 2015, we will add some sub-basin validation in the revision of the other manuscript (Langerwisch et al. 2015). We ask the reviewer to see also our author's responses to Referee #1 Comment 3 and Comment on P1466, L12-15 and to Referee #2 Comment 2 for the manuscript Langerwisch et al. (2015). We will also re-phrase our main aim 'development of a model that simulates coupled terrestrial-riverine system', instead of stating that this model is able to reproduce the Amazonian characteristics in detail.

The setup of the analysis opens some questions. How can the authors justify to ignore some important processes or factors influencing the water PCO2, without showing that this is legitimate?

Reply: The processes the reviewer might have in mind, which we did not include, are production of aquatic organic material (aquatic photosynthesis), sedimentation and re-suspension of material. We will add some more information about the processes we neglected and why we think that they are negligible in the other manuscript Langerwisch et al. (2015) in the methods section (2.1.2). The reasons for not including these processes are the following: In most limnic systems the addition of organic material via aquatic photosynthesis plays a major role for the organic carbon pool (Lampert and Sommer, 1999; Schwoerbel and Brendelberger, 2005). In Amazonia, however, the input of allochthonous material produced in the floodplain forests is more relevant than the production of organic matter within the river (Cole and Caraco, 2001; Junk, 1985; Junk and Wantzen, 2004). The aquatic photosynthesis rate in large parts of the Amazon River network is comparably low and submerged plants rarely occur (Junk and Piedade, 1997) since the white water rivers contain large amounts of sediments and are thus turbid and the black water rivers contain only little nutrients (Benner et al., 1995; Richey et al., 1990; Sioli, 1957). Sedimentation and resuspension of organic and inorganic material is also occurring (Junk and Piedade, 1997; Tockner et al., 2000). This especially has impacts on the river bed structure (Allison et al., 1995; Junk and Piedade, 1997). The sedimentation of organic material is comparably small with only 50 g C yr−1 per square meter of water area (Melack et al., 2009). These processes act on the small to medium scale (Junk and Piedade, 1997; Yarnell et al., 2006). On the spatial resolution of 0.5° × 0.5° (latitude/longitude) both processes of sedimentation and resuspension are balanced for organic carbon and have therefore not been explicitly calculated in the model.

The model is not including the carbonate system. Why? Basics from every hydrochemical text book are ignored. I would like to learn how this is justified.

Reply: We have included the carbonate system in the model. In which form CO2 occurs in the water (CO2, HCO3- or CO32-), depends on the pH, which is different for black and white/clear water rivers. This is taken into account in the model, although it is not explicitly mentioned in the model description. We will add this to the other manuscript (section 2.1.2 - Outgassing). The model output is in C only, but in the routines the solution of CO2 is calculated. We thank the reviewer for pointing out that this part was not clearly described although the model considers these aspects. We will add some clarification on that in the other manuscript (Langerwisch et al., 2015).

In addition, it seems that in an advanced setup the pH-control on PCO2 can be ignored. It would be helpful to learn how?

Reply: We are not sure if we understood the reviewers comment correctly. The pH-control of solved CO2 is incorporated in the model. Depending ont he river type we

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initialized the pH of the water (black pH 4.5, white/clear pH 7). After respiration of organic material in the water and the production of CO2, which is dissolved in the water, we calculated the ration of different carbon forms in the water (CO2, HCO3- or CO32-). We adapted the CO2 saturation concentration accordingly. We did not include the local changes in the water's pH by the solution of CO2, because the overall pH of the river mainly depends on the geological area they drain (Moreira-Turcq et al., 2003; Sioli, 1954). We will add more information on that in the methods section of Langerwisch et al. (2015).

Export of DOC and POC into the river system is not evaluated in a spatial explicit manner, despite the many published and available monitoring data from Brazil (e.g. Ore-Hybam). But evaluation and justification for above points was skipped or aggregated, so it is not possible to assess the subroutines, subcatchments (e.g., white versus black river?) and if neglecting relevant factors is justified.

Reply: Thanks for this comment. We will include some sub-basin validation of POC, DOC and DIC in the other manuscript, as we already mentioned in the author's response to Langerwisch et al. (2015).

However, the statements are very precise: "The results suggest that, following deforestation, riverine particulate and dissolved organic carbon will strongly decrease by up to 90%"; "Mainly due to the higher atmospheric CO2 it leads to an increase in riverine inorganic carbon by up to 20%", and so on.

Reply: We understand the reviewers concern that such detailed result description are in contradiction to the in his/her opinion rather general model concept. By providing an improved explanation of the model and captured processes (see our replies to the comments above), and referencing the detailed validation in Langerwisch et al. (2015), we now provide the justification for such precise findings. Thus, we are sure to sufficiently describe the evidence that, e.g. DOC will decrease by up to 90% compared to a reference period are justified. The purpose of the manuscript is to show how drastically

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deforestation in the terrestrial part can influence not only riverine carbon dynamics but also even carbon in the adjacent ocean.

I personally would not support a publication either of the model or the results from the model as it is presented here. I am not sure why the manuscript was submitted with an obviously unfinished version of a river-carbon model, ignoring a lot of the recent insights into CO2-evasion and the carbon dynamics in rivers, as well as a thorough evaluation of subroutines with the available data?

Reply: We hope to have adequately addressed the reviewers concern by better explaining some model functionalities related to the above-mentioned comments. We think by adding this information to this manuscript as well as the revision of Langerwisch et al. 2015, the context, the model concept and selected model functionalities are now better understandable. In general we think that our model, although not perfect, provides a reasonable tool to assess the effects of climate and land use change on such a tightly coupled land-river system and also opens-up some avenues for further research. We hope to have found convincing arguments showing that the model is indeed a solid model development that deserves publication in ESD.

General reply: The review mostly refers to the model reliability, which has also been discussed in the two reviews of the first manuscript (Langerwisch et al., 2015). Most of the mentioned points are already considered in the revision of this manuscript. We hope that the revised version of Langerwisch et al. (2015) will clear most of the criticized points.

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