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To the Editors of „Earth System Dynamics“

Dear Editors,

we herewith submit a revised version of our manuscript " Deforestation in Amazonia impacts riverine carbon dynamics" authored by Fanny Langerwisch, Ariane Walz, Anja Rammig, Britta Tietjen, Kirsten Thonicke and Wolfgang Cramer.

The first reviewer mainly pointed at the unclear presentation of the results in our figures. We revised the figures and made the results more clear. This we for instance achieved by changing the (rather confusing) log10 representation in the maps (Figure 2 and Figure 4) to relative changes in %. Additionally we added more information of the accompanying manuscript (‘Climate change increases riverine carbon outgassing while export to the ocean remains uncertain’, ESD-2015-36) to make the differences and similarities clearer.

The main concerns of the other reviewer have been with respect to the model reliability. This has already been discussed in the other manuscript. Our Replies and according changes refer to ESD-2015-36. We changed the main purpose of the other manuscript and also added more information about validation and reliability of our model.

All changes we conducted are listed in the revision letter and marked in the text with a yellow background. We feel that we now present convincing arguments that our work is indeed a solid model development that its results deserve publication in ESD.

I look forward to hearing from you.

Sincerely yours,
Fanny Langerwisch

“Deforestation in Amazonia impacts riverine carbon dynamics” by F. Langerwisch et al.

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 #1 *(Received and published: 20th November 2015)*

Carbon gets transported in water and hence exported from terrestrial ecosystems in particulate organic, dissolved organic or inorganic form. The current manuscript estimates these fluxes and their changes due to climate change and deforestation for the Amazonian basin from preindustrial times until 1950. It further extends the climate change impact until 2100.

This is an extension of another paper in discussion: Langerwisch et al., Climate change increases riverine carbon outgassing while export to the ocean remains uncertain, Earth Syst. Dynam. Discuss., 6, 1445–1497, 2015. It looks very much to me that the current paper is an extension of Langerwisch et al. (2015). I would have appreciated if the authors also cite it like that. For example the natural vegetation climate change runs of the current paper seem to be exactly the model runs from the other study. I see no harm in this. Being more frank about it, would have open up the avenue to include more results of the other paper in this study. For example, I would have appreciated that the results of Fig 7 of the other paper would be also in Fig 6 of the current paper.

> **Reply**: Thank you for these comments. The current paper is indeed a follow up paper of Langerwisch et al. (2015), which consist of the model description and a first application under solely climate change effects. The current manuscript estimates the combined effects of climate change and deforestation by conducting simulations from 1901 to 2099. We will make the differences and similarities between the two papers clearer in the current manuscript and will incorporate relevant results from Langerwisch et al. 2015 to improve Fig. 6 for comparison.
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> **Changes**: We explained at the end of the introduction that this manuscript is an extension of the other paper and shortly described the results of Langerwisch et al. (2015). We added the information of Figure 7 of the other paper to Figure 3 (formerly Figure 4) to show also the magnitude of change caused by climate change only.

The manuscript left the impression that the effects of climate change (CC) and deforestation (Defor) are not well disentangled. I know that it was tried and it might only be the presentation. Why Defor and CCDefor is the E-metric and CC only is the D-metric? It is very confusing. Defor would have also been cleaner if climate were not changed. Then one could have done, CC, Defor, the combined, and the combination effect.

The metrics are confusing, as mentioned. Why do I need the logarithm? There exist logarithmic axes and colour scales. And the authors have also their problems with it: for example they talk about 5% and then use the strange 10ˆ-0.02. Just use logarithmic scales then the text becomes also more natural.

> **Reply**: Our aim was to understand how and where the changes in climate and deforestation affect the riverine carbon dynamics the most. Therefor we calculated either the differences caused by deforestation only (future status under natural vegetation vs. future status under deforestation) or the effects of both drivers (past status under natural vegetation vs. future status under

deforestation). The effects of climate change only has been published in Langerwisch et al. (2015). For better comparability we will add the data for climate change only to Figure 4.

We used the logarithm to depict the small and larger changes in a comparable manner. For instance showing the rather small effects of climate change (e.g. Figure 3) in contrast to the larger changes caused by deforestation. We also used that in the text, mentioning an increase of about 5% (equaling a value in the figures of $10^{0.02}$) or a decrease of about 5% (equaling a value in the figures of $10^{-0.02}$). We think this is an appropriate way to present the changes which is similar to the option suggested by the reviewer.

Changes: We added the data showing how much climate change alone affects the carbon pools to Figure 3 (formerly Figure 4) from the other manuscript (Langerwisch et al., 2015). Therewith we want to stress the effects of the combination of climate change and deforestation. We did not add further measures of uncertainty to it (which is caused by the chosen SRES and GCMs), because we roughly want to show which carbon pools is affected the most and in which direction the changes occurs. For reasons of clarity we changed the presentation of the changes from the log10 to the factor of change in Figure 2 (formerly Figure 3) and Figure 4 (formerly Figure 5).

I think that the regions R1-R3 are not really exploited in the manuscript and can be removed. They are only showing up in Fig 6, and are also of limited interest there.

Reply: Thanks for this suggestion. We kept all regions for better comparability to Langerwisch et al. (2015). The regions differ in the intensity of climate change and deforestation effects. We don't exploit them in detail but still think it's interesting to see more regional effects. For instance looking at Figure 6 one can clearly see the differences between R1, which is mainly climate change affected, and R3, which is heavily impacted by deforestation.

There are two issues that really disturbed me reading the manuscript:
1. the figures are incomprehensible and
2. there is no insight into the relevance of the research.

Reply: We thank the reviewer for raising these points, we added a detailed response to these two points in the next paragraphs and hope this will improve the clarity of the figures.

While the **figures** look appealing at first, there are plenty of problems:
1. There are hardly any labels on the figures. What is plotted in Fig 6, for example. All axes need labels. And the colour bars.
2. The text in the figures is much too small. I had to go to 200% on my screen to be able to read Figs 1-5 and to 300% for Fig 6. It was impossible on paper.
3. The colour schemes are beyond me:
   a) In Fig 2, the colour scheme is not centred, i.e. the green fraction is smaller than the red fraction.
   b) In Figs 3 and 5, the colour bar has sections with colours that span a large section (e.g. yellow) and colours that span a very little section (e.g. orange). This merges all values from about 0.15 to 0.4 (yellow) and from 0.45 to 0.55 (orange). A well-known problem with for example the rainbow colour bar.

4. Fig 6 is unreadable. Text too small, no labels, I cannot separate the lines. I have to enlarge the figure to 300%-400% to be able to distinguish anything. But not more because then the figure gets blurry.
5. I would have loved to see both land use change scenarios in Fig 2 instead of the bar charts for R1-3.
6. Fig 4 should include not only CCDefor but also CC and Defor only. It should also include errors, e.g. on the values given.
7. The green and red borders in Fig 5 are indistinguishable. Think about something else for the distinction.

Reply to all figure remarks: Thanks for the constructive comments.

ad 1. We will add more information to the figures (which is until now only to be found in the caption). We will add labels on the color bars and the axes.

Changes: We added some more information to the figures, e.g. labels on axes and legends and in caption.

ad 2. We will increase the font size to enhance the readability.

Changes: We increased the font size and changed the relative change to percent of change.

ad 3a. Figure 2 shows the input and therewith only the data already published by Soares-Filho et al. (2006). Rather than including more information we will remove this figure, which will provide additional space for enlarging the other figures. See also **ad 5**.

Changes: We removed the figure showing the deforestation input (formally Figure 2).

ad 3b. We chose this color scheme to show the general spatial trends in the study area. We are aware that some of the detailed information gets lost this way, but we still believe that this way of displaying the data is adequate.

ad 4. We will rearrange the inset figures and will enlarge the main figures 6A-C. We hope to increase the readability of the figure this way.

Change: We rearrange the inset figures in Figure 5 (formerly Figure 6) and we increased the font size.

ad 5. To see both deforestation scenarios in detail we suggest to consider Soares-Filho et al. (2006). We will remove this figure to get more space to display the results of our study better and refer to Soares-Filho (2006) for further information on the deforestation scenarios. See also our reply **ad 3a**.

ad 6. We thank the reviewer for this suggestion which is indeed a valuable point. However, by including also the CC and Defor signal separately we think the figure would get even harder to read. We will add a remark that the detailed information on CC can be found in Langerwisch et al. (2015). A measure of the errors is depicted as the shaded area. We will add the description of the error range in the figure caption.

Changes: We added the data for the climate-change-only simulations and the corresponding reference. We did not add the error range, i.e. the range of the different model realizations from the 5 GCM and the three SRES scenarios, since this would made the figure less readable.

ad 7. By having green and red cell borders we want to distinguish the cause of the changes, which itself is shown by different cell colors. To show the dominance of CC and Defor for up to 2000 cells is not easy. We already tried different approaches, but think that this is the best option to still distinguish the colors in the cells itself. We also tried different colors for the borders, which superimposed the cell colors too much. Making a colored, a dashed or horizontally/vertically lined overlay would make the cell color un-recognizable. After testing different options for display we decided to use the green/red cell borders. We will add some more information to Figure 5 to improve the readability to give the reader some guidance.

Changes: We remove the colored cell borders. Instead we plotted inset maps, which demonstrate in which areas either the climate change or the deforestation lead to the changes in the carbon pools. The inset maps are only small, to avoid an overlay with the main figures, since they are supposed to only show general large-scale patterns. Additionally we changed the color bar labels and increased the font size. We are convinced that this presentation improves the readability.

I am also missing insights about the relevance of the study; some people would probably say that a research question is missing. If there is deforestation than there is less new carbon input and hence carbon export decreases. This is quite logical. So is the flux important? The numbers of POC and TOC in Fig 4 are a factor of 1000 less than the pools. So it looks like a small flux to me. What is wrong with my view?

Reply: We thank the reviewer for pointing to this important issue. We will improve the respective text in the introduction to explain the context and relevance of our main research objective better, so that the reader knows how to put our results into the wider context.

Changes: We added a more detailed information on the relevance of our research to the introduction and the discussion section 4.4.

There is less C input into the ocean. Is this important for the ocean? Do the fish depend on it? Does the carbon cycle care?

Reply: The reduced input of organic material to the ocean will negatively affect the respiration and production off the coast, since these depend on the imported organic matter (Cooley and Yager, 2006; Körtzinger, 2003; Subramaniam et al., 2008). To clarify this we will add more information on the effect of changes in the amount of organic material discharged to the ocean for the ocean's regional characteristics and the consequences in the discussion section.

Changes: We added some more details how the changed input of organic matter might influence the conditions in the Atlantic Ocean to the discussion section 4.4.

What is the influence for the Amazonian rainforest? I guess nutrients are transferred by inundation. How much is it related to POC and DOC and how much to IC. It might be that nutrients are transferred abiotic and are hence rather like IC and not so very influenced by deforestation.

Reply: The effect of deforestation is mainly a reduction of transported organic material, which acts as a nutrient supply for down-stream inundation forests. To clarify this we will add more information on the consequences of deforestation to the forest in the introduction and the discussion section (4.3).

Changes: Additionally to the already mentioned consequences, ranging from changing nutrient input (which might affect the vegetation productivity) and altered input of fruits and other food sources form animals (which might affect the production and habitat for several animal species) we added some more information on the in this section (Discussion Section 4.3).

These are all questions that might be asked and interesting for the community given that it is going to be published in Earth System Dynamics.

Reply: Thank you for raising these issues it will certainly help us to improve the manuscript in this respect and provide some guidance to the reader on why our study is an important contribution. We hope to sufficiently improve the manuscript with the above-mentioned replies.

I am also desperately looking for an explanation what happens after 2050. Why is POC and CO2 suddenly decreasing? The two scenarios were similar up to 2050. Then land use change stops. Why should it then suddenly decrease so strongly? There must be something else happening which should be revealed to the reader.

Reply: Thanks for this remark. We tried to explain and discuss this in the discussion (section 4.2) but apparently this was not sufficient. We will add a more detailed explanation to this paragraph. In summary the reasons are (besides the already mentioned complete removal of the vegetation) the following: During inundation the cells are partly or completely covered with water, which leads to the export of organic material. After the gradual decrease of forest cover (and therewith input of organic material) before 2050, there is a depletion of the remaining organic material in the following years. This can explain the harsh decrease of POC and outgassed carbon after we kept the deforested area constant.

Changes: We added more information on that to the section 4.2 'Shortcomings of the deforestation scenarios and implementation of crops in LPJmL'.

Minor remarks are:

1. Why extrapolating land use to the past? Why not taking historical land use maps such as of Pongratz et al.? I would have done no land use change at all before 2000 so that the references in the denominators in the metrics are always the same.

Reply: Thanks for this remark. Pongratz and colleagues did an important step towards understanding global land use change for the last centuries. However, the classification of land-use types is different and cannot be easily combined with the future LUC scenarios. So, there are conceptual limitations to combine these two data sets. Additionally, the large transformation of forests in the Amazon basin to agricultural land began in the late 1950 with the establishment of the Belém-Brasilia Highway and later of the Transamazon Highway (in the 1970). For Amazonia this is considered as the beginning of the 'modern period of Amazon clearing' (Fearnside and Hall-Beyer, 2007).

Additionally, the model LPJmL, which provides vegetation information to RivCM, requires historical land-cover information to correctly capture transient carbon dynamics. LPJmL is initialized with climate and land-use data and starts to simulate vegetation dynamics from bare ground. Unlike LPJmL, other vegetation models initialize their vegetation using land-cover maps of a particular year. We cannot do that with LPJmL.

We will add a more thorough explanation why we used the backward trends for our work in the methods section 2.2 – '*Climate change and deforestation data sets*'.

Changes: I the methods section 2.2.1 we added more information why we used the backward trend for the deforestation scenarios of Soares-Filho et al. (2006).

2. Longer and shorter to what in Table 1?

Reply: Table 1 describes the characteristics of the three sub-regions. In Langerwisch et al. (2013) the effects of climate change on the inundation patterns in these regions have been estimated. The statements in Table 1 on 'inundation length' and 'inundated area' refer to differences of past and projected inundation - longer or shorter inundation in comparison to the reference period (Langerwisch et al., 2013). We included this information to show how much of the terrestrial-riverine system is already affected by climate change only. We will add some clarification on that to Table 1.

Changes: We added more clear column headings and mentioned the reference period we used as in Langerwisch et al. (2013).

3. I would remove R1 to R3 from Table 2.

Reply: We'd like to keep them because of the above mention reasons that in Figure 6 for instance one can clearly see how large regional differences are, between areas that are mainly influenced by climate change or deforestation. Having spatially explicit climate change and deforestation scenarios enables us to also see spatial difference within the Amazon basin instead of only focusing on basin-wide averaged values. We hope to improve the presentation of our results in a better structured way.

4. Why are the proportions not adding up to 100% in Table 3?

Reply: We only show the fraction of affected area. The sum of the climate change dominated and the deforestation effects dominated cells equals the sum of all dominated cells, which might be well below 100%. To avoid confusion, we will change it to 100% of affected area and then proportion of these 100% to either climate or deforestation dominance in Table 3.

Changes: For clarification we changed the table and added a column-set for the total fraction of significant change and changed the values for dominance to the proportions of the totally changed fraction (making sum up to 100%).

5. I was wondering if the arrow in Fig 1 that shows the CO2 feedback of LPJmL to the climate models is true? It is not written in the text.

Reply: There is no feedback from the vegetation model LPJmL to the atmosphere. Both, LPJmL and RivCM, use the atmospheric CO_2 concentration based on the SRES scenarios and the climate provided by the General Circulation Models. There is no arrow going from LPJmL to the climate models.

As an aside, the present study also cites that "The ability of the coupled model LPJmL–RivCM to reproduce current conditions in riverine carbon concentration and export [...] has been shown and discussed by Langerwisch et al. (2015)." This is summarised in Table 4 of the other paper. The ability seems to be reasonable for the concentrations but rather weak for export. I would see the model system therefore rather as a tool to study sensitivities rather than projections.

Reply: Thanks for this comment. As this was also mentioned in the reviews for the other manuscript (Langerwisch et al., 2015) we will shift the main conclusion of the manuscript to show that RivCM is more a general tool to assess land-river-interactions, rather to reproduce regional characteristics in detail. We will also update respective sections in this manuscript.

Changes: We added more information on that to the methods section 2.1.2. 'RivCM – a riverine carbon model'.

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"Deforestation in Amazonia impacts riverine carbon dynamics" by F. Langerwisch et al.

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 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.
> **Changes**: We added a more detailed validation to the other manuscript and changed the aim of the manuscript to 'development of a terrestrial-riverine modelling approach'.

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 resuspension 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.

Changes: We added more information about the not-included processes and our arguments on that to the other manuscript (section 2.1.2 and 4.1, see comment 2 of review 1)

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 (CO_2, HCO^{3-} or CO_3^{2-}), 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 CO_2 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).

Changes: We changed the according paragraph in the other manuscript, which describes the model details.

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 CO_2 is incorporated in the model. Depending on the river type we 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 CO_2, which is dissolved in the water, we calculated the ration of different carbon forms in the water (CO_2, HCO^{3-} or CO_3^{2-}). We adapted the CO2 saturation concentration accordingly. We did not include the local changes in the water's pH by the solution of CO_2, 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).

Changes: We added some more information on that to the other manuscript, which describes the model details.

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).
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> **Changes**: We added the results of a more detailed validation using observation data from the HiBAm and the CAMREX projects to the other manuscript (Table 4, a paragraph in the results section (3.1) and discussion section (4.1)).

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 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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