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To the Editors of „Earth System Dynamics“

Dear Editors,

we herewith submit a revised version of our manuscript "Climate change increases riverine carbon outgassing while export to the ocean remains uncertain" authored by Fanny Langerwisch, Ariane Walz, Anja Rammig, Britta Tietjen, Kirsten Thonicke and Wolfgang Cramer.

Both reviewers agreed that developing a framework to assess coupled system of terrestrial and riverine parts, especially in the Amazon basin is of great importance. However, they were concerned that a manuscript that aims to explain the spatially and temporarily very diverse and complex processes in the Amazon basin cannot be assessed with such a simplified approach. They also criticized the too aggregated validation of the results.

We changed the focus of the manuscript in a way that we now aim to show a first attempt to understand the importance of land-river coupling for carbon assessments. We use the Amazon basin as one example region. We want to better estimate the effects of climate change on large scale carbon fluxes especially in tightly coupled systems as the Amazon basin, which is often only assessed from either the terrestrial or the riverine perspective.

In our manuscript we now focus less on how the Amazon basin might change exactly but more on (a) what is needed to integrate the land and the riverine compartments, (b) what the general trends export and patterns of carbon are and (c) how much carbon assessments might change when the inundation and the connected coupling of land and river are included.

We see from the reviewers' comments that it would be helpful to change the title of our manuscript to better show our aim of developing a framework, using the Amazon basin as one example, rather than understanding and reproducing all processes in the Amazon basin. We'd like to change the title to 'Coupling vegetation and river modelling to assess climate change impacts on carbon fluxes: first applications from the Amazon basin'

All changes we conducted are listed in the revision letter and marked in the text with a yellow background. We added a more detailed validation and we discussed possible reasons for model uncertainties in more detail. We feel that we now present convincing arguments that our work is indeed a solid model development that deserves publication in ESD.

I look forward to hearing from you.

Sincerely yours,
Fanny Langerwisch

“Climate change increases riverine carbon outgassing while export to the ocean remains uncertain” 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: 12 October 2015)

In their MS, Langerwisch et al. present the river carbon model RivCM which they apply to simulate changes in fluvial C exports and CO₂ evasion from the Amazon River system in the 21st century. RivCM simulates soil and litter C exports to headwater streams and from inundated floodplain forests to the adjacent river network, fluvial transport of organic C, decomposition of POC to DOC in transit, respiration of DOC and POC to CO₂ in transit, and the evasion of CO₂ to the atmosphere. RivCM runs at a monthly time step at a spatial resolution of 0.5°x0.5°. It is fed by the litter fall and river discharge simulated by LPJmL at daily time-step and aggregated to monthly time-step of RivCM. The seasonally changing extend of inundated areas is simulated based on the monthly discharge and the inundation model from Langerwisch et al. (2013). Mobilization of C from inundated soils to rivers and transformation of C in transit are simulated based on constant or temperature dependent rates which are taken from the literature and/or (re-)calibrated. The model is calibrated and validated using average annual DOC, POC, and IC concentrations and fluxes at the outlet of the Amazon Basin and literature values of CO₂ evasion from the total river network. Seasonality and spatial variation within the Amazon are ignored in the calibration and validation, although the difference between black water and white water/clear water rivers are highlighted in the methodology and simulation results for different sub basins are presented and discussed in the MS. For present day conditions, even after calibration, simulation results for CO₂ evasion and fluvial C exports to the coast show substantial discrepancies to observed values taken from the literature. For CO₂ evasion, simulated values are only ¼ to 1/5 of the fluxes reported by Richey et al. (2002). Compared to the more recent study of Abril et al. (2014), their simulated CO₂ evasion is even 96.7% lower. Nevertheless, the authors conclude from their future simulation for the 21st century that CO₂ evasion from the water surface will increase by 30%. Their underestimation of recent CO₂ evasion might hint at ignoring important source of CO₂ evasion from the water surface area, like CO₂ from soil respiration entering the rivers via groundwater or CO₂ from the root respiration of floating vegetation or emergent vegetation in the inundation zone. The simulated increase in CO₂ evasion would thus only refer to the proportion of CO₂ evasion fueled by leaf litter on inundated floodplains. The main conclusion of the MS that CO₂ evasion will substantially increase by on average 30 % due to climate change cannot be supported by a model that is performing so weakly for present day conditions. However, the model by Langerwisch et al. represents some pioneering effort into the right direction: the implementation of fluvial C displacement and CO₂ evasion from inland waters into the simulation of the terrestrial C budgets. If the limitation of the presented model were discussed more thoughtfully and if the still weak model performance was presented and discussed in a more transparent way, the MS could become a very interesting and valuable paper for the scientific readership. I suggest the MS to be considered for publication after some major revision. In the following, I will first give some major comments. In the general comments on the text, in particular in the method section, I will still have some more technical comments that need at least to be discussed in the MS.

Reply: We thank the reviewer for this very constructive and helpful review. We agree that the issue of understanding the possible changes in the interaction between terrestrial and riverine system part is not easy to tackle if we cannot sufficiently reproduce the current patterns of CO₂ evasion. However, we believe that our attempt to do so can provide a good template to understand these kinds of coupled systems. We will change

the manuscript in a way to more stress the general idea of assessing this, rather than focusing on the detailed evaluation of the spatially and temporally very complex Amazon Basin.

Changes: We shifted the focus of the manuscript towards a more general approach to understand these terrestrial-riverine coupled systems and changed the beginning of the abstract, first paragraph of introduction, parts of the discussion and the beginning of the summary accordingly.

Comment 1: Spatial and temporal resolution

The model works at a monthly time-step and at a spatial resolution of $0.5^\circ \times 0.5^\circ$. If I get it right, for each monthly time-step, the decomposition and respiration of organic C and CO_2 evasion to the atmosphere are calculated for the water stored in each cell. Here, I have some doubts if the combination of spatial and temporal resolution is appropriate: Did you make sure that the water residence time in the river channels within each cell is longer than one month? Or would there be a reason why that would not be necessary? If so, please explain in the MS.

Reply: Thank you for this remark. We checked that the modelled residence time (taking into account cell size and flow velocity) is shorter than one month - in the lowlands within one month the water passes through about 13 cells. But the waterbody in a given month within a specific grid cell is moving downstream, taking the carbon with it. Since the waterbody and the carbon it contains still remain in the basin (which holds true for all cells not directly close to the river mouth) we assume that for basin-wide estimates it is negligible if the water moved some cells further within one month. We will clarify the description in the methods section (P1454 L 21).

Changes: We added more information on that to the methods section (2.1.2 'Input data and RivCM model initialization') and we discussed the consequences of the resolution in the discussion (section 4.1).

Comment 2: Sources of riverine C

The model concept only considers soil and litter C on floodplains and litter fall onto headwater streams as sources of river C. The authors should at least discuss C inputs from upland soils, like the CO_2 stemming from soil respiration and entering the stream network via emergent groundwater (Johnson et al. 2008) and CO_2 from floating vegetation or root respiration in inundated areas (Abril et al. 2014). The latter have been discussed in the discussion section, but neglecting these C sources should be mentioned earlier, in the introduction and method sections.

For some river systems, floodplains might be a way more important source of organic C than upland soils. To ignore these inputs would, however, be problematic for black water rivers. In the model, the authors assume a reduced mobilization from backwater floodplains forests compared to Várzea system (by 35%). Thus, black water rivers would have lower organic C loads than white water rivers with a similar floodplain extend, also because decomposition of POC to DOC is reduced by 90% in black water system in RivCM. One of the main characteristics of black water rivers like the Rio Negro is the abundance of tropical podzols, i.e. strongly weathered soils in which organic C is more easily flushed through the soil profile due the lack of clay minerals and carbonates on which DOC could be adsorbed. While in the catchments of white and clear water rivers, groundwater has very low concentrations in DOC ($<1 \text{ mg C/L}$), DOC concentrations in groundwater under podzols in the Rio Negro basin have been reported to be very high ($>30 \text{ mg C/L}$) (McClain et al., 1997).

Reply: Thank you for this comment. Yes, we neglected the other carbon sources despite terrigenous organic carbon. We already mentioned it in the manuscript (P1452 L25) that we are neglecting the autochthonous sources. We will add a more proper explanation of the reasons and also add some information about other possible carbon sources that are

not included in the methods section (P1452 L26) and an additional paragraph on the model performance in the discussion (P1471 L3, before 4.1).

Change: We added a paragraph on this in the methods section (paragraph 2.1.2) and mentioned it in the section on model performance in the discussion section (paragraph 4.1).

Comment 3: Calibration and validation

The authors calibrated and validated the fluvial DOC, POC, TOC fluxes only for the outlet of the Amazon river, and still the calibrated DOC and POC exports deviate substantially from observed values (Table 4). Similarly, CO₂ evasion is only calibrated and validated for the whole basin. This is strongly inconsistent with the methodological distinctions made for black water, clear water, and white water rivers. How shall one know how effective the correction factors for black water rivers are?! In addition, spatial differences in the simulated change in water-atmosphere CO₂ evasion are highlighted in the results section and in the abstract. However, without any calibration and validation for sub basins (at least one sub basin of each kind: white water, black water and clear water), the simulated spatial patterns of change within the Amazon basin stand on a very weak basis. It would be important to see how the model performs for black water rivers like the Rio Negro.

I strongly suggest that the authors make a validation of TOC, DOC and POC exports for the major sub basins. As a source for observed data, they could use the CAMREX data collected by Richey and colleagues during the 80's. The export fluxes per sub basin are summarized in (Richey et al. 1990). On a related subject, the literature value of TOC flux at Obidos listed in table 4, the 36 Tg C yr⁻¹, which is cited there as Richey et al., 2002, was first published in Richey et al., 1990.

For spatial patterns in water-atmosphere CO₂ evasion, the authors could compare their simulation to the map of CO₂ evasion in (Rasera et al. 2013). In table 4, I really would like to see a validation of the simulated river discharge, i.e. simulated vs. observed annual discharge. From table 4, I can see that simulated fluxes of TOC and DOC are overestimated while the concentrations are underestimated. Does that indicate that river discharge is substantially overestimated? Please, clarify.

Reply: We thank the reviewer for this very constructive comment. Yes, effects of the sub-basin corrections and calibrations can only be adequately shown in a more detailed validation of the sub-basin results. We will conduct further validations and will add this to the manuscript in the results section (P1467 before 3.1) and in an additional paragraph in the discussion section (P1471 before 4.1).

Changes: We added more detailed validation (with the suggested sources) results to Table 4, we added a paragraph on that to the results section (3.1) and discuss it in more detail in the discussion section (4.1).

General comments:

Abstract:

Comment - P1447, L11-12: I do not agree that RivCM successfully reproduces observed C fluxes. Here in the abstract, the authors should be more honest about how good the performance of RivCM really is, in particular the fact that river CO₂ evasion is underestimated by a factor >4. Here, the authors should give percentages for over/underestimation of CO₂ evasion and fluvial TOC exports as listed in table 4. Then, they should name potential reasons why CO₂ evasion is underestimated (neglecting important sources). It is important to highlight these limitations as the

main result of the study is that CO₂ evasion from rivers will increase by 30% due to climate change.

Reply: We will add additional information on the model performance and on possible reasons for the under- and overestimations and rewrite our concluding sentence in the abstract.

Changes: We rephrased the sentence in the abstract, and we discuss the possible reasons for over- and underestimations in more detail in the discussion.

Section 2:

Comment - P1452,L22-25: If I get it right, here, IC represents only free, dissolved CO₂, and does not include carbonate alkalinity (DIC present as HCO₃ and CO₂, which is counterbalanced by base cations). Please, define your use of IC here.

Reply: Other dissolve inorganic carbon species are included; we calculated the fraction of HCO₃⁻ and CO₂ in the water depending on the river type specific pH. For the output we only focused on the amount of carbon in all (in-)organic carbon species. We will clarify this in the manuscript (P1452 L25).

Changes: We added some more information on that to the methods (paragraph 2.1.2 ‘Outgassing’ section, about P1462L8).

Comment - P1454,L17-18: Why have these classes been chosen?

Reply: These classes have been chosen according to the location and the spatial extent of the area they cover, i.e. the smallest class covering headwater cells and the largest class only covering the main stem. We will add some clarifying information to the manuscript (P1454 L18).

Changes: We added some clarifying information (Section 2.1.2 – ‘Input data and RivCM model initialization’)

Comment - P1454,L20-21: Is water retention on floodplains taken into account in the simulation of discharge?

Reply: The retention of water in the floodplains depends on the floodplain area, which is calculated by the model, and on the profile of the cross section of the river, which we cannot estimate. Therefore water retention is not included in the simulated discharge. It would only delay the transport of water but not the amount of routed water and we think that the general patterns in the riverine carbon would not change drastically if we would include the retention.

Changes: We added information on that to the discussion section (4.1).

Comment - P1456, L7: Is that due to the albedo and insolation?

Reply: We are not sure to which sentence this questions refers. L6-8 states: ‘Since LPJmL does not account for inundation, which changes respiration, the respiration of litter in (partly) water-saturated soils is calculated within RivCM.’ The rate of respiration in partly water saturated soils (e.g. under oxygen shortage) differs from the rate in air-saturated soils. This does not have connections to the albedo nor the insolation. If we get the correct sentence to which the reviewer is referring to we can clarify the influence of albedo and insulation.

Comment - P1456, L12: Do you have a reference for this?

Reply: We will provide a reference, which includes the model description of LPJ.

Changes: We added the reference Sitch et al 2003.

Comment - P1457,L14-22: Is the soil C pool in the inundated areas updated with inputs from the litter layer in RivCM?

Reply: Yes, the soil carbon pool is filled by the litter which is provided by LPJmL.

Changes: We added details on that.

Comment - P1458, L11-14: Could you please describe in one sentence how MaxInunArea was calculated in Langerwisch et al., 2013?

Reply: We will add a short description of this calculation (P1458 L14).

Changes: We added a short description in the Methods section (2.1.2 – ‘Size of monthly inundated area’)

Comment - P1459, L5-9: Do you generally assume the river area to be 25% of the maximum inundable area? The estimates of Richey et al., 2002 refer to the central Amazon basin, which is characterized by very extensive floodplain areas. The relations between river surface area and maximum inundable area are likely not transferable to the rest of the Amazon Basin. Maybe you can check with the publication of (Lauerwald et al. 2015), which provide a 0.5° degree map of river surface areas (excluding Strahler orders 1 and 2) in their supplemental material.

Reply: Yes, we assume that 25% of the floodable area is permanently inundated by the river, based on the work of Richey et al.. We are aware that their estimate has been calculated only for a central rectangle of 1.77 million km², which covers the main stem and the surrounding areas. But even though it covers the main stem, it also covers areas more distant. We will check the mentioned publications and adapt/clarify our manuscript accordingly.

Changes: We discussed this issue in the discussion part 4.1.

Comment - P1460, Eqs 13+14, Table 3: The factors mobil<litc> and mobil<soilc> are taken from Irmiler 1982, and obviously derived for a black water system. Before, for the amount of litter and soil C, and later, for the decomposition of POC, the authors highlight the differences between Várzea and Igapó floodplains, and introduced correction factors for the latter. Why should the mobilization rate be the same for both systems?

Reply: For the decomposition we assume that this reaction will happen on a slower rate, because the plant's material is less easily degradable (P1461, L2,3). For the mobilization we assume that the structure of the terrigenous material is not of high importance. It would be different if we assume that at black water rivers twigs are mobilized, while at white water rivers only leaves are mobilized. But there is no reason to make this difference. We think that the physical conditions of moving water to mobilize terrigenous material are the same on black and white water rivers. That's why we have the same mobilization rate on black and white water rivers. We will add some clarifying explanation to the manuscript in the methods section (P1458 L4).

Changes: We added an explanation to the methods section 2.1.2 (‘Mobilization’, at about P1458L4).

Comment - P1461, Eqs 20-24: Are the respiration rates the same for DOC and POC, and for black water and other rivers? In Eq. 17, the decomposition of POC from black water rivers are reduced by 90% relative to other river systems. Why should the respiration rate be the same? Similarly, it was written before that the decomposition from coarse to finer POC and further to DOC would increase the rates of heterotrophic respiration (P1453, L14-22). Why is that not represented here in these equations?

Reply: The respiration rates are the same for DOC and POC and in black and white water rivers. The reason for having different decomposition rates at black and white water rivers is, that leaves at black water rivers tend to be more sclerophyllous and therefore less easily degradable. For the respiration of already degraded organic material we assume only minor differences. As soon as the leaves and twigs are degraded to small particles we assume that they react similarly. We will add some more clarifying information to the methods sections (P1461 L16) and to the discussion.

Changes: We added some clarifying explanation to the methods section 2.1.2. 'Respiration' (at about P1641L17).

Comment - P1462, L7, Table 3: What does $ctoco_2$ represent exactly? Is it the proportion of CO_2 on DIC, similar to dissociation constants which are not represented due to the lack of pH values? Please, clarify.

Reply: $ctoco_2$ represents the ratio of the atomic mass of carbon (12.001 g mol⁻¹) in the CO_2 molecule (44.01 g mol⁻¹). Because we only calculate the actual flux and pools of carbon, we have to use the factor to calculate the outgassed CO_2 .

Changes: We added some information as a footnote to Table 2.

Comment - Table 4: I think the value of Neu et al., 2011 refers to the CO_2 evasion flux per water surface area, not per total surface area! It would be nice to have a simulated vs. Observed river discharge.

Reply: Neu et al published data on the outgassed carbon per m² and year. Since we estimated the water covered area, we can compare this value with our output. There is a comparison of observed with simulated discharge in Langerwisch et al. 2013. In this publication the discharge of 44 observation sites has been compared to simulated data showing the LPJmL can reproduce the observed discharge patterns. We will add a sentence referring to the discharge evaluation in the previous publication.

Changes: We mentioned the discharge validation publication in the results section (3.1)

Comment - From table 4 it is obvious that the simulated riverine CO_2 evasion is underestimated by a factor of 4-10, likely because some sources of CO_2 evasion are neglected (see my major comment 2). The calculation of CO_2 evasion is, however, based on the oversaturated concentrations reported by Richey et al., 2002. That also means that the fraction of free dissolved CO_2 laterally exported to the coast and not evading to the atmosphere from the river would be overestimated.

Is the simulated concentration of free dissolved CO_2 listed in table 4 that reported by Richey et al., 2002 and used to force the riverine CO_2 evasion in this model? Please, clarify. At least the concentration value after Richey et al., 2002 (can be calculated from the seasonal p CO_2 values that were extracted here for this study) should be listed here in that table. It would also be nice to have the fluvial export flux of IC listed in that table to see which proportion of CO_2 produced in the river water column is exported laterally to the coast and which proportion is evading vertically to the atmosphere.

Reply: The concentration of inorganic carbon listed in Table 4 has been taken from Cole and Caraco (2001) and Neu et al. (2011). We will check again the references and will add the requested information to table 4.

Changes: We added more data to compare our simulated inorganic carbon concentration to Table 4 and checked the data from Richey et al. (2002).

Comment - P1464, L17-25: The coupling between the land and river model, does it go in both directions, i.e. are outputs of RivCM used as input for LPJmL? In the cells for which inundation can occur, are litter and soil C storage and decomposition/respiration only simulated in RivCM? Are these cells ignored in LPJmL when calculating net-exchange of CO₂ between atmosphere and land vegetation/soils?

Reply: The coupling is one-directional. RivCM uses the LPJ output as input, but the processes calculated, are only affecting the carbon pools and fluxes in RivCM. The carbon stored in litter and soil is calculated in LPJmL, but the reduction of litter due to the mobilization is not fed back to LPJmL. In the net-exchange the decomposition fluxes in LPJmL and RivCM are combined, but there is not double-accounting, because the carbon transported and respired in the river is not respired on land anymore. We will add some clarifying information to the methods section (P1455 L2)

Changes: We added this clarification to the method section 2.1.2 ('Data input from LPJmL to RivCM', at about P1455 L2).

Comment - P1464, L26 - P1465, L4: In setting 2, is there still the litter fall onto the permanently inundated surface areas of head water streams included?

Reply: In this experiment there is no input of terrigenous organic material into the river (see P1465 L1). We will write it more clear in this paragraph.

Changes: We added this information to the method section 2.4 ('Modelling protocol and simulation experiments', at about P1465 L2).

Comment - P1466, L12-15: Again, if the authors want to present simulated differences between sub-basins, they should calibrate/validate their model on sub-basin level (see major comment 3).

Reply: Since it is our overall intention to understand the general trend in the carbon fluxes and pools in the whole Amazon basin we did not focus much on the sub-basin validation. We will additionally conduct some sub-basin validation and add the results to the manuscript (see also Comment 3 on calibration and validation).

Changes: We added more data to validate our results. See also comment 3.

3 Results

Comment - Table 6: It would be nice to have the fluxes of riverine outgassing reported in this table, not just their proportion on the total CO₂ flux to the atmosphere. The focus of the MS is on riverine CO₂ evasion and thus those numbers should be given directly, in particular as the proportion of riverine CO₂ evasion is very small. From table 4 it is evident that riverine CO₂ evasion is substantially underestimated for present day conditions. So I guess the proportion of riverine outgassing on total CO₂ evasion is underestimated as well. This should already be discussed here in the results section. The authors should make clear that, though their model is not able to reproduce the observed riverine CO₂ evasion for present day conditions (they are off by a factor of >4!!!), they assume that the simulated relative changes in riverine CO₂ evasion would be representative. The authors should discuss how this could be justified.

Reply: We will add the numbers to Table 4 and also discuss the consequences of the mismatch of observed and simulated outgassing in more detail (in the results and the discussion sections).

Changes: We added the absolute values of outgassed CO₂ from the terrestrial and the riverine part to table 6. We also discussed in more detail possible reasons for the underestimation of the outgassing compared to observations (discussion section 4.1).

Comment - Table 6: Please, write the units in the column headings. Why is TOC discharge and CO₂ evasion reported in different units? Please, use annual fluxes and the same units for each flux.

Reply: We will write the units in the column headings. The reason behind having CO₂ outgassed to the atmosphere in 1012 g month⁻¹ is, that parts of the outgassed carbon will be taken up by the plant again relatively quickly (within a month), while the carbon discharged to the ocean (in 1012 g yr⁻¹) is definitely extracted from the system. To avoid confusion of the reader we now show both in the same units.

Changes: As suggested we added the units to the table column headings and converted the monthly fluxes to annual fluxes. We also added the absolute values for the export.

4 Discussion

Comment - P1471, L4-8: Here, the authors should make clear that they did not do any calibration/validation at sub-basin level. For the spatial differences they just trust their simulation without having validated the effects of spatial drivers, in particular the spatial distribution of black water systems vs. white and clear water systems.

Reply: Also as a result from suggestions made in General comment 3 and comment on P1466, L12-15 we will conduct a sub-basin validation of our model results. To estimate large-scale basin wide changes it is helpful to be able to reproduce carbon pools and fluxes also on a sub-basin level. We will add this analysis and a discussion of its results to the manuscript (in the results sections P1467 before 3.1, and in the discussion section P1471 L3).

Changes: We added a further validation and discussed possible causes of the mismatch between some simulation data with observations in the results and the discussion section.

Comment - P1471, L9-14: Were the rising atmospheric pCO₂ taken into account in the calculation of CO₂ evasion? Were the oversaturated CO₂ concentrations, which were taken from Richey et al to force the CO₂ evasion for present day conditions, adjusted for future simulations?

Reply: Yes, the rising CO₂ concentrations have been taken into account, depending on the CO₂ from the SRES emission scenarios. For the future conditions we applied the same oversaturation factors as we applied for the present day condition. The increased partial pressure of CO₂ also increases the CO₂ concentration in the water. We assume that the course of this oversaturation, being a very high respiration in the water and only a comparably slow mixing and outgassing of the CO₂, will be the same even under elevated pCO₂.

Comment - P1472, L9-29: The authors should also discuss the effect of river damming and POC burial in sediments (in reservoirs, floodplain lakes, on floodplains). These are not included in the model and might cause an overestimation of fluvial POC exports.

Reply: This is right; we neglected this aspect in the manuscript, although we are aware of it. We will add some sentences in the discussion reflecting this point.

Changes: We added information on that to the discussion section 4.1.

Comment - P1473, L11-23: The CO₂ evasion from the river stems from soil and litter C that is laterally displaced and respired in transit. The authors should clearly point out what is so different about this CO₂ evasion compared to soil and litter C directly respired in/on upland soils. Isn't the effect of the rivers that soil and litter C are just respired further downstream? If an ESM model ignores inland waters and fluvial C transport, would it over- or underestimate the net-exchange between the atmosphere and land (including inland waters)? From table 6, it looks like the simulated overall CO₂ flux from land to atmosphere does not change significantly if RivCM is coupled to LPJmL or not. Here, the authors need to bring some more convincing arguments why this land-river coupling would be important.

Reply: The main effect of the mobilization of terrigenous organic material is indeed that carbon is removed from one region and transported somewhere else. Therefore it is no longer available on site and the basin-wide carbon assessments should take into account that carbon (either as organic carbon or inorganic carbon) is transported and finally discharged to the ocean. Including this export leads to a more realistic estimation of carbon fluxes, and a model ignoring this constant drain of carbon from the Amazon basin, will therefore overestimate the general ability of Amazonia to sequester carbon. To make this clearer we will have a more thorough discussion on that in the discussion section (P1473 L23).

Changes: We added the above mentioned arguments to the discussion section 4.2.

Comment - P1473, L22-25: The model substantially underestimates CO₂ evasion from the rivers. Thus, you cannot draw these conclusions here.

Reply: Here we will make the limitations of our approach clearer, by showing that we can only estimate possible relative changes (e.g. a small to high increase or decrease) instead of absolute numbers of change. We will add some clarifying sentences to the results (P1470 L9) and discussion (P1473 L14) sections.

Changes: We added clarifying information to the discussion section 4.2.

Comment - P1474, L6-13: Is there any significant seasonality for DOC and POC concentrations at Obidos? I also do not fully understand this argument. If the simulated discharge is arriving too early or too late (because the water retention on floodplains was not well simulated?) at Obidos, wouldn't the POC and DOC transported in the discharge also be earlier or later? After the simulated monthly values have been aggregated to an annual flux, would that still make a difference?

Reply: Our message in this paragraph is that our overestimation of the carbon concentration leads to an overestimation of the export. LPJmL is able to reproduce the discharge of water, so the only reason for overestimating the carbon discharge is the too high concentration of organic carbon (POC is +44%, DOC -28%) in the water. A different hydrograph (too early or too late high or low water peak) would lead to different results, because the discharged carbon in a certain month is calculated with the respective water and carbon amounts in the cell. We will add some clarifying sentences in the discussion section (P1474 L16).

Changes: We rephrased the sentence regarding the effect of water discharge and carbon concentration in the discussion (4.2)

“Climate change increases riverine carbon outgassing while export to the ocean remains uncertain” 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: 12 October 2015)

This manuscript takes on the rather daunting task of coupling a large scale dynamic vegetation model with a highly aggregated river carbon model to address the potential changes in river carbon fluxes under different climate change scenarios. The plus/minus to doing this are:

Plus. It is very useful to think about developing overall system models, coupling the multiple key sectors. It forces critical thinking, and the mobilization of information from multiple sources. Not an easy task!

Minus. That said, at what point is the aggregation so great and assumptions so broad that there is a little confidence in the output?

Reply: Thank you very much for your comments. With our manuscript we try to assess the importance of including the inundation and associated carbon export by the river to a vegetation model. We will write more clearly that we don't intent to fully understand the temporarily and spatially complex carbon fluxes in this coupled land-river-system. Rather we aim to establish a concept to estimate the effect of coupling land with river in a mostly from a terrestrial or riverine perspective investigated system.

Changes: We added and rephrased parts in the abstract, the introduction (first part) and in the discussion.

BROAD ISSUES

Comment - 1. The model development discussion is very generic, and shows little understanding of the Amazon itself, at multiple levels. - It starts with the space and time scales of the model, set to 0.5o and monthly. In a month, a parcel of water travels from high in the Andes to the Atlantic. A 50 x 50 km cell covers rather a lot of territory, relative to the scale of stream and river channels. - It seems that all rate terms including in-river are computed within LPJmL, which is purely terrestrial. It would be useful –essential- to evaluate these relative to in-river measurements (literature). – I started to go through the model setup topic-by-topic, and tracking each to output, but don't have enough time to complete that.

Reply: Yes, our approach on assessing the terrestrial-riverine coupling on monthly time-steps and on a spatial resolution of 0.5x0.5° is rather coarse. We are aware that there are certain limitations to the model approach. But we aim to assess general large scale carbon patterns and changes and accept that on a smaller scale the model is not able to reproduce the local patterns very well. With our work we try to understand how much the basin-wide carbon balance depends on the interaction with the river and how much it could change in the future. To clarify our objective we will add a paragraph on that in the introduction (and further in the discussion) and will try to make this clearer throughout the manuscript.

Changes: We added some clarification on our general goal to establish a model that capture the interconnection between the terrestrial and the riverine part to the Introduction

Comment - 2. The analysis breaks the Amazon up into several sectors (northern, western, etc). Calibration/validation is done very generically for the “export” values cited (which correspond to the station of Óbidos, though not mentioned). That station represents the highly damped integration of so many very different water sources (Madeira vs Negro, for example) and timing

that it doesn't represent a robust point of calibration, if the intent is to represent the response of different regions (see below).

Reply: We thank the reviewer for this very constructive comment. We will conduct further validations on the sub-basin level and will add this to the manuscript in the results section (P1467 before 3.1) and in an additional paragraph in the discussion section (P1471 before 4.1).

Changes: We added the results of a more detailed validation using observation data from the HiBAm and the CAMREX projects to Table 4, we added a paragraph on that to the results section (3.1) and discuss it in more detail in the discussion section (4.1).

Comment - 3. It is not at all clear how the values of the different primary pools are established – POC, DOC, (D)IC, other than to say “mobilization.” Processes for each are very different. Is IC total DIC or pCO₂? DIC includes a significant component of weathering, which is never mentioned. Floodplain autochthonous production is not a negligible component of the river system C cycle.

Reply: The mobilization only includes the export from organic material from the land to the river. All carbon pools in the model are based on the terrigenous carbon and atmospheric carbon. Weathering or other sources of inorganic carbon are not included. We also neglected the autochthonous production of organic carbon, as we mentioned in the methods section (P1452 L 26). We will add some more information why we excluded some processes (such as in-river production or weathering) in the methods section. Additionally we will discuss in more detail how the results would change by including the neglected processes.

Changes: We added a paragraph on this in the methods section (paragraph 2.1.2) and mentioned it in the section on model performance in the discussion section (paragraph 4.1).

Comment - 4. Carbon flux is, of course, a product of discharge and concentration. Any analysis of carbon flux has to start with hydrology. But we have no idea how well LPJmL does for the Amazon, or how it delivers the hydrology commensurate with the change scenarios. It is thus difficult to have a clue about the carbon part of the argument.

Reply: LPJmL can reproduce the discharge of most of the large river systems very well. This was shown by Gerten et al. (2004, Journal of Hydrology) and Gordon et al. (2004, Ecol. Appl.). But for the Amazon basin the hydrograph was shifted. In 2013 we published a study showing that by adapting the flow velocity from 1.0 m s⁻¹ to about 0.25 m s⁻¹ (in the lowlands) the discharge was much better reproduced than before (Langerwisch et al. 2013, HESS). By applying the modified flow velocity in the current study we are certain that the discharge patterns in the Amazon basin are adequately reproduced, which is indeed a prerequisite to assess riverine carbon fluxes. We will add some more information on that to the methods section (P1452 L8).

Changes: We mentioned the discharge validation publication in the results section (3.1)

Comment - 5. The abstract states that the model “successfully reproduces observed values...” Actually, it doesn't even come close. And even if it did, it wouldn't mean much, at Obidos, given how many different signals are combined there.

Reply: Thanks for this remark. Our aim is to understand changes in the carbon pools and fluxes and therefore we assumed the reproduction of the general trend could be sufficient. We will add a more detailed validation on the sub-basin level and will also discuss more detailed the consequences of such a large scale approach.

Changes: We revised the abstract and removed the statement that the model successfully reproduces observed values. We discuss the reasons for the model-data deviation in more detail We added the results of a more detailed validation to Table 4, we added a

paragraph on that to the results section (3.1) and discuss it in more detail in the discussion section (4.1).

Comment - 6. Examination of river outgassing relative to terrestrial misses the point that the river outgassing is relevant to the carbon nominally sequestered by on land, it is not part of the daily 24 hour production/respiration cycle.

Reply: We assume that by extracting carbon from one site and finally exporting it to the Atlantic Ocean the carbon is no longer available for the short-term 24h production/respiration cycle. In the results section (3.4) on the effect of including the inundation, discussing the results from our experiments (Standard, NoInun, NoRiv), we will discuss this further, as well as in the discussion section 4.2.

Comment - 7. In an effort to be all-inclusive, enough detail to be convincing is lost.

Reply: We will make more clear what the aim of our manuscript is, namely not to be all-inclusive, but rather show general trends and possible changes in the future. We will add clarifying paragraphs in the introduction and the discussion sections.

Changes: See reply on general comment no. 1.

CONCLUSION: Where does this leave us? At an absolute minimum, the thrust of the manuscript has to be changed. Perhaps start by breaking out by major tributary basin (Negro, Madeira etc)

What it is not. A credible examination of Amazon River carbon outgassing and export to the ocean, under current or future climates. The author's justifications of their results aren't valid. While their idea of serving as a linkage between small-scale observations and global estimates is a good one, it does not justify the large errors between their observed and predicted results in outgassed C or exported OC. The model also does a poor job of predicting outgassed CO₂ under current conditions, so it is difficult to rate the significance of the model's predicted increases. (There are grammar issues with this manuscript as well).

What it is/could be. A structure for how to go about developing a modeling framework, for working towards such goals. A useful paper would be to outline the issues involved in doing this. This manuscript could fill a niche in connecting current research on carbon processing in the Amazon with predicted climate change models.

At the end of the day, it depends on what the objectives are, here. I question whether or not such a strategy, with its abstractions and scales, could possibly produce a result that is meaningful to how the Amazon actually functions, under either current or future conditions. If it is to be, much better presentation and justifications are necessary. If the intent is to provide an Amazon module for a global model, perhaps it could get there.

Reply: Thanks again for these very helpful comments. We will make it clearer that our aim is to make a first attempt to understand the importance of land-river coupling for Amazonia. We finally want to better estimate the effects of climate change on large scale carbon fluxes especially in tightly coupled systems as the Amazon basin, which is often only assessed from either the terrestrial or the riverine perspective.