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Earth System Dynamics Discussions

Interactive comment on "Climate change increases riverine carbon outgassing while export to the ocean remains uncertain" *by* F. Langerwisch et al.

F. Langerwisch et al.

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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: 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 form 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 $\frac{1}{4}$ 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.

Comment 1: Spatial and temporal resolution The model works at a monthly time-step and at a spatial resolution of 0.5°x0.5°. If I get it right, for each monthly time-step, the decomposition and respiration of organic C and CO2 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).

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 CO2 stemming from soil respiration and entering the stream network via emergent groundwater (Johnson et al. 2008) and CO2 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 mg C/L), DOC concentrations in groundwater under podzols in the Rio Negro basin have been reported to be very high (>30 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).

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 patters 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).

General comments:

Abstract: 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.

Section 2: 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).

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).

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.

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.

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

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

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.

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).

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 km^2, 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.

P1460, Eqs 13+14, Table 3: The factors mobil<litc> and mobil<soilc> are taken from Irmler 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).

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.

P1462, L7, Table 3: What does ctoco2 represent exactly? Is it the proportion of CO2 on DIC, similar to dissociation constants which are not represented due to the lack of pH values? Please, clarify.

Reply: ctoco2 represents the ratio of the atomic mass of carbon (12.001 g mol-1) in the CO2 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 CO2.

Table 4: I think the value of Neu et al., 2011 refers to the CO2 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 m^2 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.

From table 4 it is obvious that the simulated riverine CO2 evasion is underestimated by a factor of 4-10, likely because some sources of CO2 evasion are neglected (see my major comment 2). The calculation of CO2 evasion is, however, based on the oversaturated concentrations reported by Richey et al., 2002. That also means that the fraction of free dissolved CO2 laterally exported to the coast and not evading to the atmosphere from the river would be overestimated. Is the simulated concentration of free dissolved CO2 listed in table 4 that reported by Richey et al., 2002 and used to force the riverine CO2 evasion in this model? Please, clarify. At least the concentration value after Richey et al., 2002 (can be calculated from the seasonal pCO2 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 CO2 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.

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)

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.

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).

3 Results 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).

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.

4 Discussion 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).

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.

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.

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 a more thorough discussion on that in the discussion section (P1473 L23)

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.

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)

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