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

Anonymous Referee #1

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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). Mobiliza-

tion 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.

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.

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 or-

ganic 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).

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 an DOC are overestimated while the concentrations are underestimated. Does that indicate that river discharge is substantially overestimated? Please, clarify.

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 CO_2 evasion is underestimated by a factor >4. Here, the authors should give percentages for over/underestimation of CO_2 evasion and fluvial TOC exports as listed in table 4. Then, they should name potential reasons why CO_2 evasion is underestimated (neglecting important sources). It is important to highlight these limitations as the main result of the study is that CO_2 evasion from rivers will increase by 30% due to climate change.

Section 2:

P1452,L22-25: If I get it right, here, IC represents only free, dissolved CO_2, and does not include carbonate alkalinity (DIC present as HCO_3 and CO_3^{2-}, which is counterbalanced by base cations). Please, define your use of IC here.

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

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

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

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

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

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

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.

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?

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?

P1462, L7, Table 3: What does ctoco2 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.

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.

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.

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?

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

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).

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.

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.

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.

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?

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.

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.

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

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?

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