Reviewer #2

Interactive comment on "ORCHILEAK: A new model branch to simulate carbon transfers along the terrestrial-aquatic continuum of the Amazon basin" by Ronny Lauerwald et al.

Anonymous Referee #2

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Overall the authors present compelling work to address a major deficiency in earth system models. The authors demonstrate successful simulation of CO2 and DOC lateral transport and CO2 evasion in the Amazon using ORCHILEAK. Previously, no ESM models existed that simulate the lateral transport of CO2 and DOC from surface water sources. The ORCHILEAK enables scientists to attribute DOC and evasion sources. Additionally, the model has the ability to quantify the CO2 evation in relation to terrestrial net primary production. The authors partition these processes into flooded and non-flooded lands with regard to soil carbon and DOC throughfall and subsequent lateral transport and evasion. Below I provide general and specific comments for the authors to consider to improve the current form of the manuscript.

The authors clearly describe the work that was instrumental to their contribution, and present sufficient references to support their model advancement. The authors provide a succinct abstract summarizing the modeling advancement, results, and potential future application. The authors explicitly show where code area available to replicate this project and provide this in the supplement with good instructions. Including a table of all forcing and evaluation datasets and sources would aid efforts of anyone attempting to build upon the authors work.

We agree and added a table of forcing files necessary to run ORCHILEAK over the Amazon basin (now Table 1) as well as a table of evaluation datasets used in this study (now Table 2) in the main text, method section.

The equations and formulae are correctly defined,
but providing all the equations within the appendix hinders the reader's ability to distinguish which equations are novel to the manuscript. The manuscript would benefit by including a table describing each variable in the appendix, not in a different file in the supplement, or by including the equations in line throughout the document with variable description.

We agree. We now include the equations in the text and have inserted the table with the variable explanations in the appendix (Table A.1).

1. The general methods are clearly outlined, however ambiguity exists in the temporal resolution of the model implementation. Specifically, many temporal resolutions are mentioned: i.e. 6 min, 30 min, and daily. Clarification on how these different components of the model interact would enhance clarity. Adding a flow chart or including this within Figure 3 would add clarity.

We followed the advice of Reviewer 2 and included the information on different temporal resolutions into Figure 3. A colour code, explained with a legend, now indicates the temporal resolution of each flux.

We also added a more clear description of the use of the different time steps at the end of subsection 2.2.1:
"Like the cycling of water and C in vegetation and soils, the allochthonous inputs of DOC from S_{can} and S_{soil} into the inland water network (F_{RO}, F_{DR}, $F_{soil2flood}$, $F_{soil2river}$, see Fig 3) are computed at a temporal resolution of 30 minutes and at the spatial resolution of the grid cell. The lateral transfer between the S_{fast}, S_{slow}, S_{river} and S_{flood} and the transformation of C within those storage reservoirs are only simulated at a daily time step and at the spatial resolution of the basin. Therefore, to simulate the lateral transfers, the allochthonous DOC and CO_2 inputs are first aggregated over 48 30-minute time steps until one full day is over. The fluxes from the water column back into the soil column ($F_{flood2soil}$,$F_{up2swamp}$ in Fig. 3) are simulated at the daily time-step of the routing module, but are used as inputs in the soil carbon module, which runs at a 30 minute temporal resolution. This is achieved by downscaling the daily fluxes uniformly over the 48 30 minute time-steps of the following day of simulation. The evasion of CO_2 from river and floodplain water surface ($F_{river2atm}$, $F_{flood2atm}$) is also simulated at the daily time-step of the routing module, but to approximate the continuous interplay of CO_2 inputs and CO_2 evasion controlling the water-air gradient in CO_2 partial pressures (pCO_2) a much shorter time-step of 6 minutes is used, and the CO_2 inputs to the water column are thus uniformly distributed over the 240 6-minutes time-step contained in each day."

2. There is limited discussion on calibration. After reading the manuscript, the majority of the parameters seem to be taken from the literature. Please describe the calibration process referenced in Sections 2 and 3.

The majority of parameters are indeed taken from the literature. The calibration process mainly focussed on the change in the flooding scheme, and the necessary recalibration of discharge afterwards. Most parameters controlling the flux of DOC from vegetation and soils to the water column and the decomposition of DOC within the water column were taken from the literature or are based on assumptions. Nevertheless, we secured that our choice of model parameters led to a good agreement with observed values. In the revised manuscript, we now report sensitivity analyses on the most important parameters, showing the sensitivity of the model results to a change in parameter values. Generally, we now include performance measures in all graphs on simulated discharges, inundation, DOC concentrations and fluxes as well as CO_2 evasion rates, which are more objective.
For this purpose, we restructured parts of the results and discussion section (see our response to comment #3 of reviewer #1) to better present the results of our calibration and sensitivity analyses. In section 2.3, we also now describe more clearly the general calibration procedure.

3. The authors mention this briefly on page 37 L16-21 and on 38 L2-4 how the lack of representation of POC transport might shift in stream DOC and CO2 production downstream. Please expand upon how the lack of representation impacts the current model evaluation including the impact on aggregate downstream DOC, POC, and CO2 evasion. How would this impact the evaluation results presented in Figure 12 and 13? Does this mean the current form of the the model over/under-compensates for the lack of mobile POC? Which parameters would be impacted?

We agree that this is an important point. We expanded the discussion on the absence of representation of POC transport in section 3.4, which now reads:
"…
One of the major future steps would be the implementation of particulate organic C (POC) fluxes in ORCHILEAK. Of the TOC fluxes at Obidos, the most downstream sampling location on the Amazon main stem, POC contributes less than one fourth of the total flux (Moreira-Turcq et al., 2003; Ward et al., 2015), and was reported to further decrease to only about 10% downstream to the river mouth (Ward et al., 2015). The decomposition of this POC, which is mainly derived from floodplain litter, has been

reported to contribute substantially to the in-river CO_2 production in the lower part of the Amazon (Ward et al., 2013). Our simulation results also highlighted the substantial contribution of submerged leaf litter to the CO_2 evasion. However, in our simulation, POC is not transported downstream with the water flow, i.e. it is assumed to decompose locally, and only the DOC and dissolved CO_2 produced from this decomposition are transferred laterally. The representation of POC transport would induce a downstream shift in the simulated DOC and CO_2 production from POC. The lack of this representation might have induced a bias in the simulated longitudinal pattern of DOC concentrations, pCO_2 and CO_2 evasion with an overestimation of upstream values compared to downstream values. With the limited availability of evaluation data and the rather simplified representation of POC and DOC decomposition in the model, it is impossible to conclude whether the lack of representation of POC transport explains part of the discrepancy between observed and simulated DOC concentrations (Fig. 14), or whether a too low DOC decomposition rate compensates for the bias. Mayorga et al. (2005) found that there must be a small, rapidly cycling pool of young organic matter from terrestrial vegetation close to the river that sustains a high CO_2 concentrations of a young ^{14}C age, while the majority of the transported POC is substantially older. The actual effect of POC transport shifting CO_2 evasion downstream is thus likely rather limited. Nevertheless, a more complete representation of fluvial POC and DOC exports would be highly beneficial to constrain dynamic boundary conditions for an ocean biogeochemical model of the Amazon plume. The application of ORCHILEAK to rivers with substantial soil erosion driven POC exports will require the implementation of soil erosion and sediment transport modules (Naipal et al., 2015, 2016).

…."

4. Figure 14 displays the performance of modeled DOC concentration relative to observations. Removing the data from Rio Negro from this plot reveals that the model only produces ranges of DOC from 3-5 mg C/L while observations are double that range [1-7 mg C/L]. How do you reconcile the low variability of the model relative to the observations? This seems in contrast to the results of the simulation data presented in Table 4.

We agree that the variability in simulated DOC concentrations is significantly lower than the observed variation in DOC concentrations. Only in case of the Rio Negro, we see flushing effects with high DOC concentrations at high discharges.

We see indeed high variation in DOC concentration in the overland flow (or "surface runoff", as it is termed in our study) (Table 6). Two distinct phenomena have to be taken into account when evaluating their effect.

First, the streams and rivers are fed by the sum of both, surface runoff and drainage, and drainage shows a comparatively low variability in DOC concentrations. For instance for sampling point RO3 (simulation b, Table 4 [now Table 6]), we simulate DOC concentration ranges (5^{th} and 95^{th} percentile) of 1.8 to 37.7 mg C L-1 for the surface runoff and of 2.2 to 4.8 mg C L-1 for drainage. However, according to our simulations, surface runoff contributes only 4.9% to total runoff (see Table 4), and thus DOC concentrations in total runoff (5^{th} and 95^{th} percentile) range only from 1.9 to 4.9 mg C L^{-1}. In addition, we find that in our simulations the variability in DOC concentrations is highest at low total runoff, and decrease with increasing total runoff, see the following graph (Fig. R.1). When at this location, the total runoff rate is higher than on average (2.62 mm/day) (these events make in sum about 90% of the total runoff over the simulation period), the DOC concentration ranges (5^{th} and 95^{th} percentile) only from 2.9 to 4.3.

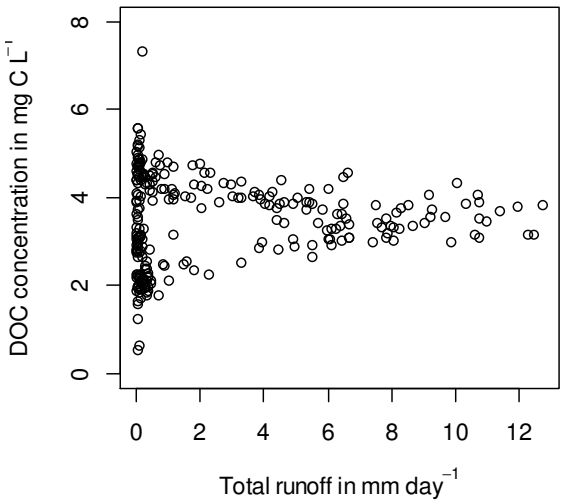

Figure R.1: Simulated DOC concentration vs. total runoff (from monthly values) for sampling location RO3, simulation b (see Table 4, which is now Table 6).

The second aspect that we have to take into account is the proportion of labile vs. refractory DOC. Note that in our simulation, labile DOC has a half-life of only 2 days. This implies that the majority of labile DOC is already decomposed before it reaches a downstream river sampling location. From table 3 (now table 5), we see that the labile proportion of DOC in the surface runoff is quite important, but also highly variable. In the drainage, the labile proportion is on the contrary generally not significant in our simulations (Table 3, now table 5). Thus, for RO3, where total runoff is dominated by drainage, the variation in labile proportions is not that important. For sampling location RO1 (see Table 4, now table 6), however, surface runoff contributes to about one third of total runoff according to our simulation. Simulated DOC concentrations vary (5^{th} and 95^{th} percentile) over 9.8 to 29.6 mg C L^{-1} in the surface runoff and from 5.4 to 13.3 mg C L^{-1} in the total runoff. The simulated concentrations of refractory DOC in total runoff vary only from 4.8 to 9.0 mg C L^{-1}. In our simulations, there is a tendency for a higher proportion of labile DOC at high total DOC concentrations and at high total runoff (see Fig. R.2 a and b). This pattern is in agreement with what has been observed in the field (McLaughlin and Kaplan, 2013). Thus, although we see a strong positive trend for higher total DOC concentrations at higher total runoff (Fig. R.2c), this trend is much weaker for the refractory DOC (Fig. R.2d). We also see that refractory DOC concentrations vary more strongly at low total runoff. When at this location, the total runoff is higher than on average (the average is 4.05 mm/day, the days with higher runoff contribute about 79% of the total runoff over the simulation period) the refractory DOC concentration ranges (5^{th} and 95^{th} percentile) only from 5.5 to 8.5 mg C L^{-1}.

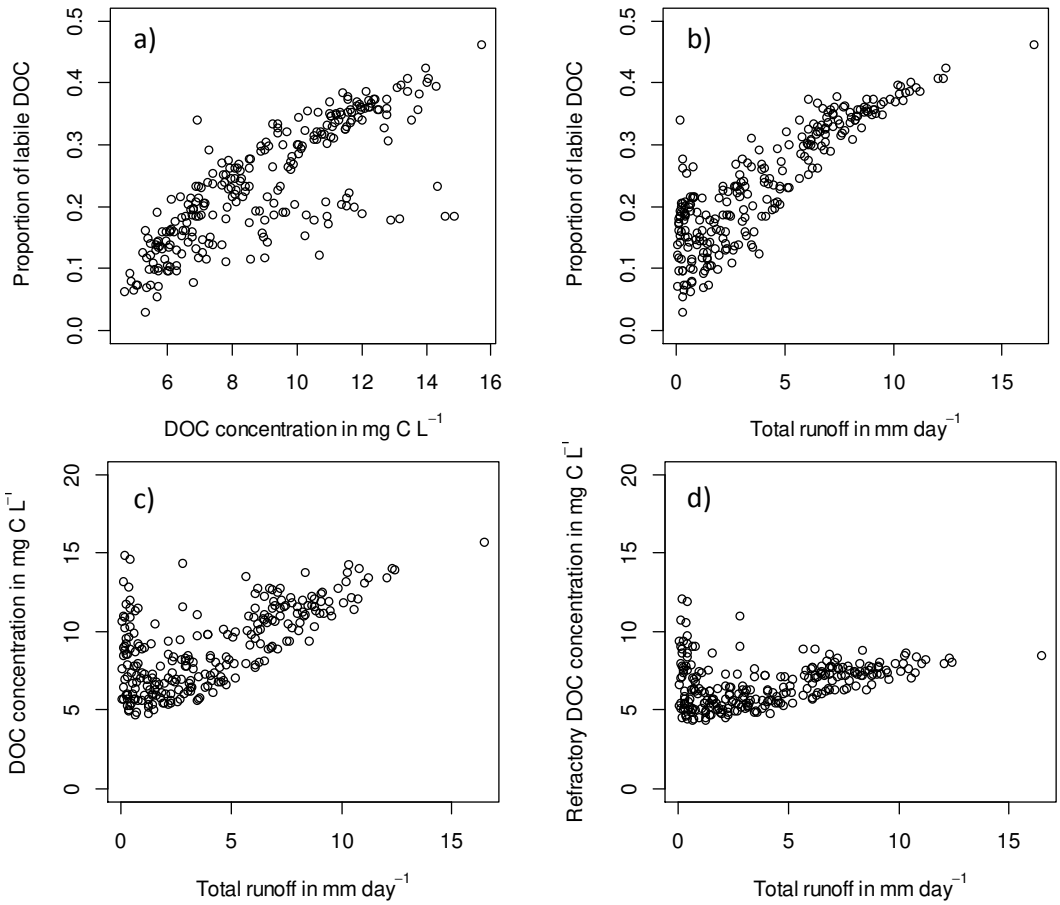

Figure R.2: DOC dynamics at location RO1 (see Table 6 (which was originally 4))

In summary, the simulated DOC concentrations in total runoff vary less than in surface runoff. The variations are much lower for refractory DOC which is transported over longer distances and which represents the bulk of DOC passing to downstream river sampling locations. The variations are in addition even lower during episodes of high total runoff that contribute most to the bulk of river discharge. For these three reasons, the simulated DOC variations in the river are much lower than in surface runoff and in the headwaters.

The higher variability in observed vs. modelled DOC concentrations in the river (Figure 14) is likely due to a number of processes not included in ORCHILEAK, which control short-term fluctuations in DOC production and consumption. Note, for instance, that algae blooms can contribute to short-term peaks in DOC concentrations (Moreira-Turcq et al., 2003), and algae derived organic matter can also exert a priming effect which increases the decomposition rates of allochthonous organic carbon in the river (Ward et al., 2016).

5. The authors note how CO2 evasion is comparable to data from Richey et al., 2002, but also disclose that the inundated fraction is greatly underestimated for the central Amazon. The central Amazon shows that the model has the highest evasion rates there [Fig. 15]. The concluding remarks on Page 35, Lines 10-17 should explicitly address how the match in CO2 evasion and mismatch in inundation are related. Specifically, how does the underestimation of inundated extent impact the assertion that 51% of CO2 evasion is attributed to the floodplain?

We agree and have modified the revised manuscript to include this important point. We have added a short paragraph in the new section 3.4 of the revised manuscript:
"The fact that we simulate a total CO_2 evasion similar to the one reported by Richey et al. (2002) is somewhat surprising taken that that our mean water surface area is substantially lower (see section 3.1). In other words, we simulate a higher CO_2 evasion rate per water surface area than estimated by Richey et al. (2002). These authors used relatively lower gas exchange velocities k_{600} of 1.2 to 2.3 m day^{-1} to calculate CO_2 evasion from rivers, while we applied a significantly higher value of 3.5 m day^{-1}, following more recent observations (Alin et al., 2011; Rasera et al., 2013). Note that in our physically based model approach, the total CO_2 evasion is not so sensitive to the gas exchange velocity, but rather to the simulated CO_2 sources. Reducing or increasing the gas exchange velocities $k_{river,600}$ and $k_{swamp,600}$ by 50% lead to a change in simulated total CO_2 evasion of only -4% and 1%, respectively. On the contrary, in a data driven approach to calculate CO_2 from observed river pCO_2 values, the calculated CO_2 evasion will change linearly with changes in the gas exchange velocity. Rasera et al. (2013) finds higher gas exchange rates than Richey et al. (2002) and thus suggests that the total CO_2 evasion must be considerably higher. As the results summarized in Fig. 16 suggest, our CO_2 evasion rates per water surface area are comparable to those of Rasera et al. (2013). Assuming that we underestimate the average flooded area, we conclude that we likely underestimated the CO_2 inputs from flooded soils and vegetation and the CO_2 evasion from the water surface to the atmosphere. In the future, improved floodplain forgings and simulations at higher spatial resolution might help to overcome these underestimations."

Specific comments: Figure 3 caption refers to Table 1, I believe the authors intend to reference Table S1.

Reviewer #2 is right. It should be table S1, which has now become table A1 in the appendix. This has been corrected in the figure caption.