Referee #1

1. In the presented manuscript, two branches of the land surface model (LSM) ORCHIDEE, the ORCHILEAK branch (dissolved organic carbon (DOC) processes in the soil and leakage to surface runoff) and the ORCHIDEE-MUSLE branch (DOC and CO2 transport in rivers) are merged and enhanced by an explicit lateral river transport scheme for 3 particulate organic carbon (POC) and 3 sediment classes. The resulting ORCHIDEE-Clateral of Zhang et al. targets and is evaluated against the European river network. By comparing the model results to observations of European river discharge, sediment discharge rates, total organic carbon (TOC), DOC and partially POC concentrations, the study aims and provides insights into i) the lateral redistribution of organic carbon through the European river network and its effect on vegetation/terrestrial ecosystem budgets ii) CO_2 fluxes and iii) loss of carbon and sediments to the marine environment.

In general, the manuscript is well written and the content presents a valuable contribution in determining lateral fluxes and sediment/TOC budgets and their effects on ecosystem production with the aid of large scale LSMs. While not being an expert in global land surface modeling (-disclaimer-), I would recommend to publish the manuscript after addressing some major points outlined below.

Thanks a lot for your positive comments, as well as your suggestions and corrections below. Your comments are very helpful to improve our manuscript. We have carefully studied them and revised our manuscript accordingly. Please see our responses to your specific comments below.

General comments

2. My main concerns are centered around the rather low explanatory power of the model with regards to river DOM, TOC (Fig. 4) and particularly POC concentrations (Fig. 6) - the latter being the main development step of the new ORCHIDEE_Clateral branch. I understand that modeling such a complex river network is extremely challenging. Particularly when considering the tradeoffs between computational resources and limited availability of observations (for model input, parametrization and comparison/evaluation), this can lead to such deviations on seasonal timescales, while model results being still

robust and valuable for long term simulations on (annual,) decadal and centennial timescales. Nevertheless, I would suggest to consider the following points:

First, if I am not mistaken, calculating TOC-DOC=POC (according the authors caption in Fig. 4) would likely provide you with a much higher number of river measurements for POC than currently shown in Fig. 6 (assuming here that there is some overlap between TOC and DOC measurements for the river stations). Hence, a more direct comparison and evaluation between the new model development step and observations could (and should) be achieved beyond the river Rhine and Ems comparison demonstrated in Fig. 6.

Thanks for your suggestion. Unfortunately, the GLORICH database does not provide any additional samples for which both TOC and DOC would have been observed. In fact, TOC appears most often in the database as a variable replacing DOC measurements, likely when sampled water was not filtered (Ronny Lauerwald, personal communication, co-author in Hartmann et al., 2019). Furthermore, we think that even if TOC and DOC would have been observed independently (from unfiltered and filtered samples), POC values calculated as difference between both variables would be associated with high uncertainties, and thus not necessarily improving the dataset used for model evaluation.

Hartmann, J., Lauerwald, R., and Moosdorf, N.: GLORICH - Global river chemistry database, in: PANGAEA (Ed.), 2019.

3. **Second, since total mass fluxes are calculated via water volume transport times particulate and dissolved matter concentration, it would be helpful for the reader to show and discuss such comparison to observations in addition to the shown material (i.e. is it improving correlations compared to correlations for concentrations alone or further deteriorating them? - e.g. due to timing and/or the seasonal variability, see next point).**

We have added two supplementary figures to show the comparison between simulated and observed riverine carbon flux rates, in addition to the riverine carbon concentration. As our model captures well the seasonal and interannual variations in riverine water discharge rates, the simulated seasonal and interannual variations are closer to observations for riverine carbon flux rates than they are for riverine carbon concentrations. Please see Figs. 6, S11 and S12 for further details.

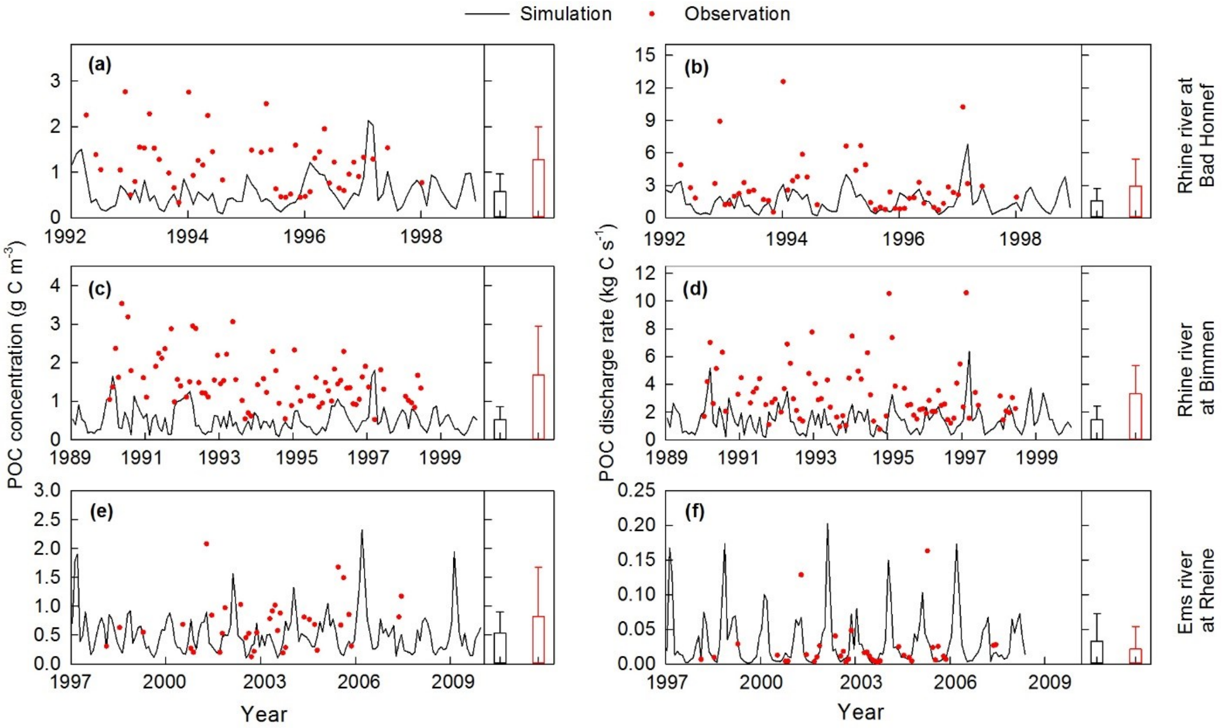


Figure 6 Comparison between observed (instantaneous measurements) and simulated (monthly average values) riverine POC concentrations and POC discharge rates at three gauging sites. The histograms and error bars denote the means and standard deviations of POC concentrations, respectively. Long-term average water discharge rates at Bad Honnef, Bimmen and Rheine during the observation periods are 2023, 2100 and 80 m^3 s^{-1}, respectively.

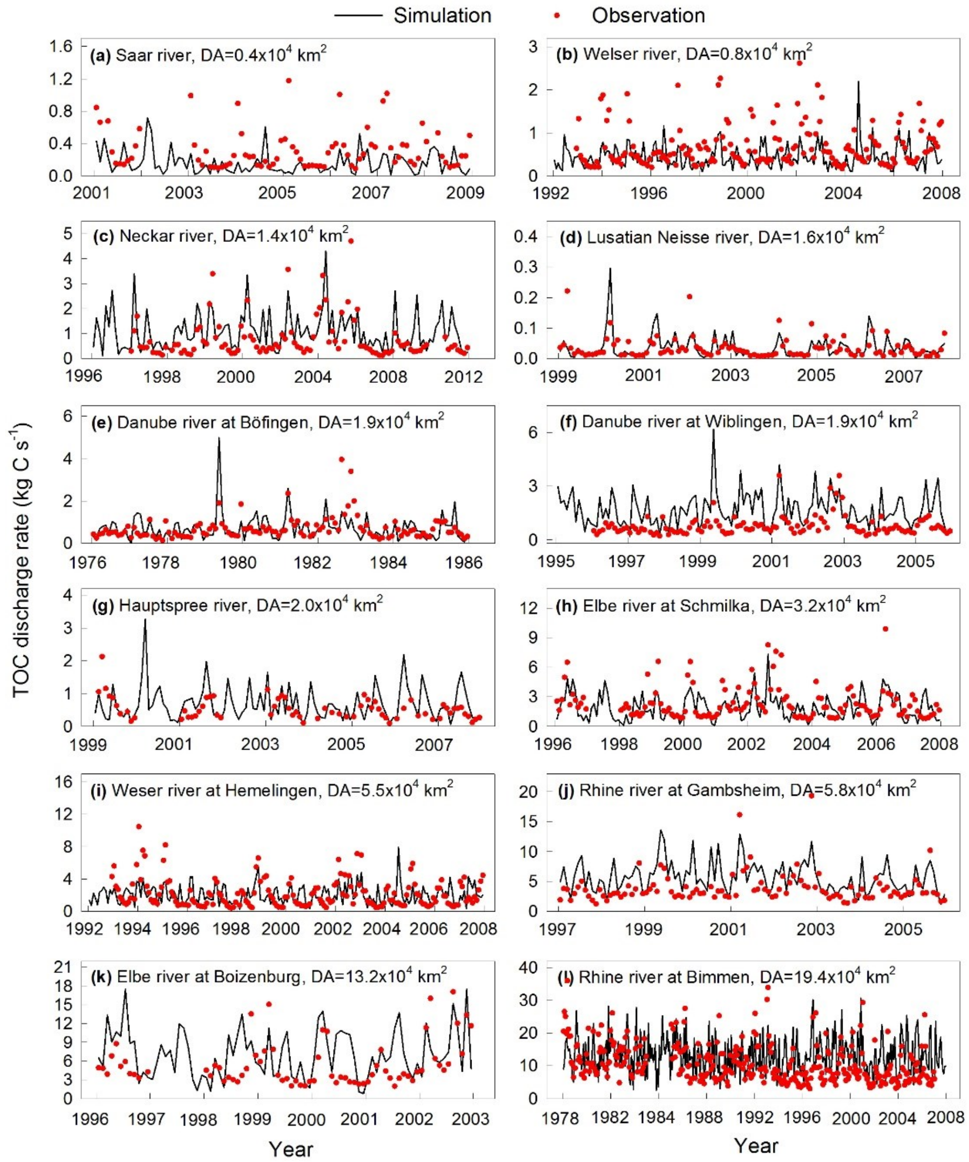


Figure S11 Comparison between simulated and observed discharge rates of total organic carbon (TOC) in representative European rivers. DA is the drainage area of the corresponding gauging station.

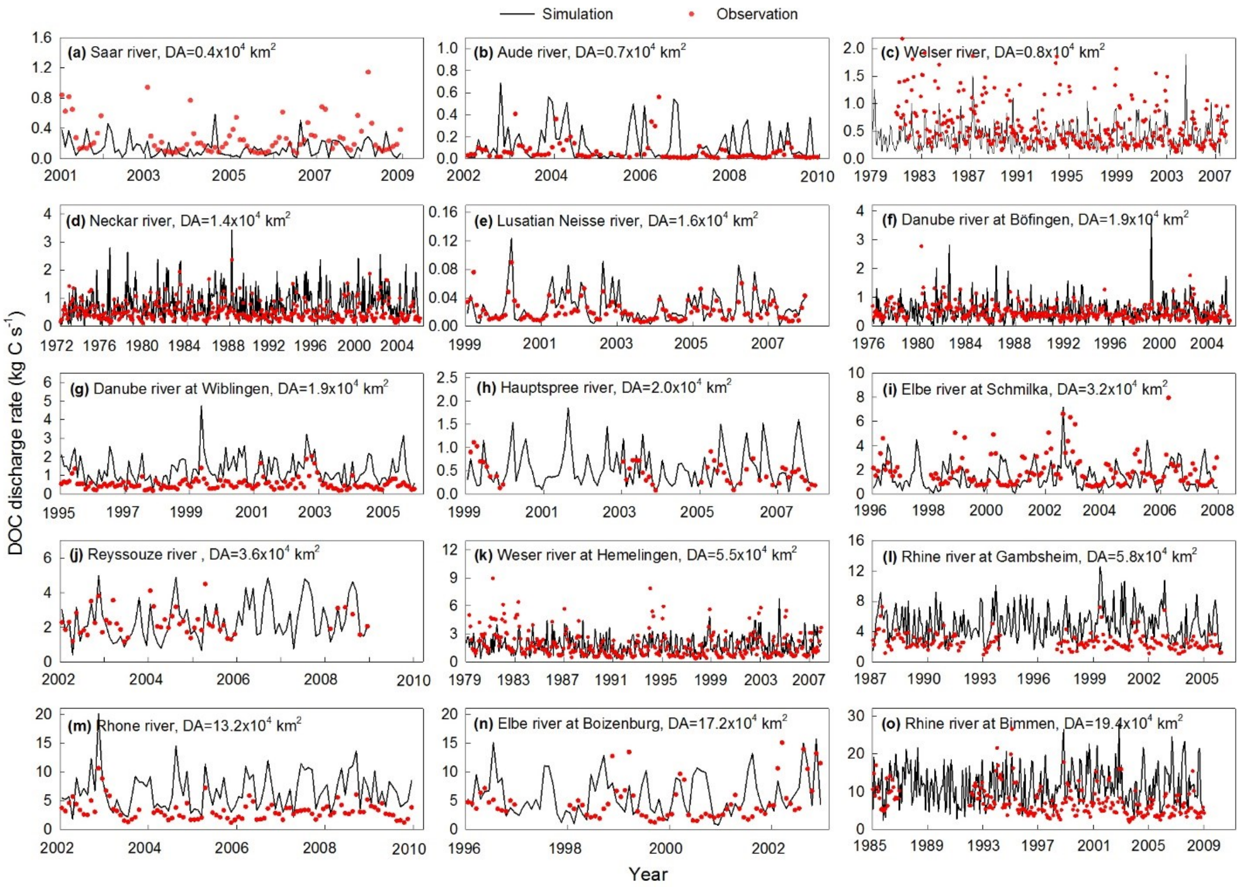


Figure S12 Comparison between simulated and observed discharge rates of dissolved organic carbon (DOC) in representative European rivers. DA is the drainage area of the corresponding gauging station.

4. Third, even though the model is targeted at large and long time scale simulations, the authors point out their models capability to potentially capture also sub-year time scales like e.g. seasons. Capturing variability is often an issue for models. The here presented model seem to overestimate the seasonal variability for DOC and TOC, while not for POC concentrations - why is that? I highly encourage a discussion on the variability. I believe it would be very valuable to investigate and discuss it (e.g. with regards to seasonality e.g. with a relative deviation to observations to enable better comparison between rivers in the time domain). It could provide insights into potential consistent deviation pattern and origins of variability (to me it looks as if there is a blurred, but consistent pattern). I suggest to discuss the potential origins of the too high (DOC,TOC) and low (POC) variability in the light of the model shortcomings and potential future development steps.

Thank you for this highly valuable suggestion. We have investigated the potential reasons for the overestimated seasonal variations in riverine DOC and TOC. Basically, the concentration of soil DOC and the DOC decomposition rate during the lateral transport process in the river network are the two key factors controlling DOC concentration in river flow. Our simulation suggests that only a limited fraction (<20%) of the riverine DOC is decomposed during transport along the river network (Fig. 7). Therefore, we suggest that the uncertainty in the seasonal dynamics of simulated soil DOC concentrations is the main reason for overestimating the seasonal variation in riverine DOC concentrations. In fact, the soil DOC scheme used in the ORCHIDEE model is known to not fully capture the temporal dynamics of soil DOC concentrations (Camino-Serrano et al., 2018), although it reproduces the annual average DOC concentrations rather well. TOC concentrations are calculated as the sum of DOC and POC concentrations, and riverine TOC is often dominated by DOC (Fig. 7). Thus, the overestimated amplitude in TOC concentrations is mainly caused by the DOC seasonal dynamics in the topsoil and, thus, the leaching flux. Following your suggestion, we have added one paragraph to discuss why the seasonal variations TOC and DOC concentrations are overestimated and what future work is needed to improve the model. Please see:

"The simulation of the soil DOC dynamics and leaching in our model need to be further improved to better simulate the seasonal variation of riverine DOC and TOC concentrations. The concentration of soil DOC and the DOC decomposition rate during the lateral transport process in the river network are the two key factors controlling DOC concentration in river flow. As only a small fraction (< 20%) of the riverine DOC is decomposed during lateral transport (Fig. 7), the overestimated (Fig. 5) seasonal amplitude in riverine DOC (and TOC) concentrations is likely caused by the uncertainties in the simulated seasonal dynamics of the leached soil DOC. The current scheme used in our model for simulating soil DOC dynamics has been calibrated against observed DOC concentrations at several sites in Europe (Camino-Serrano et al., 2018). Although the calibrated model can overall capture the average concentrations of soil DOC, it is not able to fully capture the temporal dynamics of DOC concentrations (Camino-Serrano et al., 2018). Given this, it is necessary to collect additional observation data on the seasonal dynamics of soil DOC concentration to further calibrate the soil DOC model" (lines: 841-853)

Averaged over the various DOC and SOC pools we distinguish in the soils, DOC represents a much more reactive fraction of soil carbon (with a turnover time of several days to a few months) than SOC (with a turnover time of decades to thousands of years). Therefore, soil DOC concentrations experience large seasonal variations, while SOC concentrations generally are much more stable and show very limited seasonal dynamics. Therefore, seasonal variations in riverine POC concentrations are mainly controlled by the seasonal dynamics of soil erosion rates, rather than by the seasonal SOC dynamics, which explains a partial decoupling in the behavior of POC compared to that of DOC. We have also added these texts in the revised manuscript to explain the different seasonal pattern of riverine DOC and POC concentrations. Please see lines: 853-861. In our study, we agree that the simulated riverine POC concentrations at three sites in central Europe are lower than the observed values (Fig. 6). The standard deviation of the simulated POC concentrations at these three sites thus is smaller than that of the observed POC concentrations. However, when the seasonal variation of POC concentrations is measured by the coefficient of variation, i.e. the standard deviation divided by the mean, the simulated seasonal variations are overall comparable to the observed one. Given this, we do not fully agree that our model underestimates the seasonal variation in riverine POC concentrations. At Rheine site of the Ems river, the seasonal dynamics of POC discharge rates are even somewhat overestimated (Fig. 6f).

Reference:

Camino-Serrano, M., Guenet, B., Luyssaert, S., Ciais, P., Bastrikov, V., De Vos, B., Gielen, B., Gleixner, G., Jornet-Puig, A., Kaiser, K., Kothawala, D., Lauerwald, R., Peñuelas, J., Schrumpf, M., Vicca, S., Vuichard, N., Walmsley, D., and Janssens, I. A.: ORCHIDEE-SOM: modeling soil organic carbon (SOC) and dissolved organic carbon (DOC) dynamics along vertical soil profiles in Europe. Geosci. Model Dev., 11, 937-957, 2018.

Minor comments

5. While I am not a native speaker, I make some wording suggestions below.

Thanks for your suggestions and corrections below. We have carefully studied them and revised our manuscript accordingly. Please see our responses to your specific comments below.

6. Generally, I would encourage to place the two tables being currently in the supplementary material into the main text.

We have moved the supplementary Table S1 to the main text according to your suggestion. As we have provided the definitions of all abbreviations in the main text, and the length of the main text exceeds 13,000 words, we prefer to keep the original Table S2 (i.e. the Table S1 in the revised manuscript) in SI.

7. Main document

l.60 predicting or projecting?

We replaced 'predicting' by 'projecting'. Please see "A reliable model which is able to explicitly simulate the lateral carbon flux along the land-river continuum and also the interactions between these lateral fluxes and the comprehensive terrestrial carbon cycle, would thus be necessary for projecting changes in the global carbon cycle more accurately." (lines: 58-61)

8. l.75 have been developed

We corrected the grammar error by changing the original 'has' to 'have'. Please see "Over the past decade, a number of LSMs have been developed which represent leaching of DOC from soils (Nakhavali et al. 2018, Kicklighter et al. 2013) or the full transport of DOC through the land-river continuum (Lauerwald et al., 2017; Tian et al., 2015)." (lines: 75-77)

9. l.78/79 how does this eventually relate to the seemingly lower POC than DOC concentrations in rivers and to the results presented? → discussion

With the sentence "However, the erosion-induced transport of POC, which is maybe even more important than the DOC transport in terms of lateral carbon flux (Lal., 2003; Tian et al., 2015; Tan et al., 2017), is still not or poorly represented in LSMs.", we do not intend to say the amount of lateral POC flux is larger than that of DOC, but to state that the lateral POC flux might have larger impact on terrestrial carbon cycle than DOC flux. Because the erosion and deposition processes of POC do not only result in lateral redistribution of the eroded POC, but also alter the vertical SOC profile at the eroding and deposition places, and soil erosion might also result in significant decrease of terrestrial vegetation production. We recognize that the relative importance of POC and DOC transfers on the terrestrial carbon cycle shows large spatial variation. To ensure a more precise introduction, we have changed the original sentence to "However, the erosion-induced transport of POC, which has also been reported to be able to affect the carbon balance of terrestrial ecosystems strongly (Lal., 2003; Van Oost et al., 2007; Tian et al., 2015), is still not or poorly represented in LSMs." (lines: 77-80)

10. l.86 How about new production in rivers?"

From a technical point of view, implementation of instream autotrophic production is highly challenging, beyond our possibilities for developing riverine C transfers into a LSM. However, although in-stream primary production can be an additional organic carbon source, field studies have indicated that terrestrial carbon is the major source of riverine organic carbon (e.g., Raymond & Bauer, 2001). In addition, in large rivers the majority of primary production (94%) tends to be locally respired rather than transported far downstream (Howarth et al., 1996). Given these reasons, we conclude that the exclusion of in-stream production in the current version of ORCHIDEE-Clateral model is not a critical omission to assess the importance of land-to-ocean C fluxes for the terrestrial C budget. Nonetheless, we have now discussed this shortcoming in the revised ms. Please see "Although soils are the major source of riverine organic carbon, domestic, agricultural and industrial wastes, as well as river-borne phytoplankton can also make significant contributions (Abril et al., 2002; Meybeck, 1993). Moreover, previous studies have shown that sewage generally contains highly labile POC while most of the aquatic production is generally mineralized within a short time (Abril et al., 2002; Caffrey et al., 1998). Omission of organic carbon inputs from manure and sewage could potentially lead to an underestimation of CO_2 evasion from the European river network. Inclusion of these additional carbon sources should thus help improve simulation of aquatic CO_2 evasion." (lines: 892-899)

Howarth, R. W., R. Schneider, and D. Swaney (1996), Metabolism and organic carbon fluxes in the tidal freshwater Hudson River, Estuaries, 19, 848–865.

Raymond, P. A., and J. E. Bauer (2001), Riverine export of aged terrestrial organic matter to the North Atlantic Ocean, Nature, 409(6819), 497–500.

11. l.154 go to → enter

We changed the ‘go to’ to enter. Please see “The products of litter and SOC decomposition enter free DOC pool”. (line: 155)

12. l.180 a forcing file

We revised the text based on your comment. Please see “Simulation of the lateral transfer of DOC and CO_2 in river networks, i.e. the transfer of DOC and CO_2 from one basin to another based on the stream flow directions obtained from a forcing file (0.5°, Table 1)” (lines: 176-178)

13. l.191 by a basin-specific

We revised the text based on your comment. Please see “the DOC and dissolved CO_2 returning to river channel at the end of each day is calculated based on a time constant of flooding water (= 4.0 days, d’Orgeval et al., 2008) modified by a basin-specific topographic index (f_{topo}, unitless) (Lauerwald et al., 2017).” (lines: 187-190)

14. l.207 finer

We only describe the fine time step (i.e. 6 minutes) for simulating DOC decomposition and CO_2 evasion in inland waters, without comparing it with the time steps for simulating other processes (e.g. soil erosion and lateral C transfer). Thus we still use ‘fine’ rather than ‘finer’.

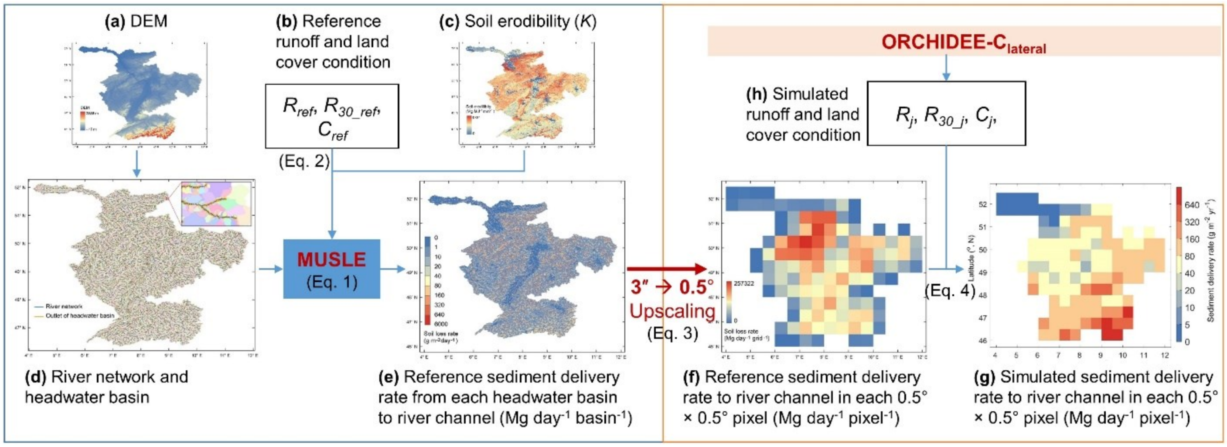
15. l.220 Sediment and particulate organic carbon delivery... (opposed to CO2 carbon)

Maybe a question of a non-initiated reader (and potentially something to clarify): are basin and grid cell in your model description inter-changable or how is a 'basin' defined in the model realm?

We changed the subtitle to ‘2.1.2 Sediment and particulate organic carbon delivery from upland soil to river network’ based on your suggestion.

The headwater basin and grid cell are not inter-changeable in our manuscript. The headwater basins are extracted from high-resolution DEM data, and there can be many headwater basins in each 0.5° grid cell of ORCHIDEE-Clateral (Fig. S2, see below). To simulate the sediment and POC deliveries from land soils to river networks, we first extracted the headwater basins and river networks from high-resolution DEM data (Fig. S2a,d). Then the MUSLE model (Eq. 1) was applied at each headwater basin to calculate the reference net soil loss rate (Fig. S2e) under a given set of reference runoff and vegetation cover conditions. By summing up the net soil loss from all headwater basins in each grid-cell (Fig. S2e), we can calculate the total soil loss rate from land to river network under reference runoff and vegetation conditions (Eq. 3, Fig. S2f). Finally, the aggregated data of soil loss rate at 0.5° is used as a forcing file of ORCHIDEE-Clateral to calculate the actual daily soil loss rate from land to river (Eq. 4, Fig. 2g). The upscaling scheme and the method for extracting headwater basins and river networks has been described in details in Zhang et al. (2020). That is why we only give a brief overview in this manuscript.

Nonetheless, we have now revised the method section in this manuscript and added a supplementary figure (i.e. Fig. S2) to provide a better explanation of our method to simulate the sediment delivery from uplands to river channels. Please see "To give an accurate simulation of sediment delivery from uplands to river network and maintain computational efficiency, an upscaling scheme which integrates information from high-resolution (3″) topographic and soil erodibility data into a LSM forcing file at 0.5° spatial resolution, has been introduced (see details in Zhang et al., 2020, Fig.S2). With this upscaling scheme, the erosion-induced sediment and POC delivery from upland soils to the river network, as well as the changes in SOC profiles due to soil erosion had already been implemented in ORCHIDEE-MUSLE (Zhang et al., 2020). The sediment delivery from small headwater basins (which are basins without perennial stream and are extracted from high-resolution (e.g. 3″) digital elevation model (DEM) data, Figs. S2a&d) to the river network (i.e. gross upland soil erosion – sediment deposition within headwater basins) is simulated using the Modified Universal Soil Loss Equation model (MUSLE, Williams, 1975). As introduced in Zhang et al. (2020), "the daily sediment delivery rate from each headwater basin (S_{i_ref}, Mg day^{-1} basin^{-1}) is first calculated for a given set of reference runoff and vegetation cover conditions (Fig. S2e)" (lines: 216-227)



(d) River network and headwater basin

(e) Reference sediment delivery rate from each headwater basin to river channel (Mg day^{-1} basin^{-1})

(f) Reference sediment delivery rate to river channel in each 0.5° × 0.5° pixel (Mg day^{-1} pixel^{-1})

(g) Simulated sediment delivery rate to river channel in each 0.5° × 0.5° pixel (Mg day^{-1} pixel^{-1})

Figure S2 Upscaling scheme used in ORCHIDEE-MUSLE (Zhang et al., 2020) for calculating the sediment delivery rate from headwater basins to river networks. MUSLE is the Modified Universal Soil Loss Equation; DEM is the digital elevation model (m); *K* is the soil erodibility factor (Mg MJ-1 mm-1); R_{ref} is the assumed reference daily runoff depth (= 10 mm day^{-1}); R_{30_ref} is the assumed reference maximum 30-minutes runoff depth (= 1 mm 30-minutes^{-1}); C_{ref} (= 0.1, dimensionless) is the assumed reference cover management factor; R_j, R_{30_j} and C_j are the simulated daily total runoff depth, daily maximum 30-minutes runoff depth and daily cover management factor on day *j*, respectively.

Zhang, H., Lauerwald, R., Regnier, P., Ciais, P., Yuan, W., Naipal, V., Guenet, B., Van Oost, K., and Camino-Serrano, M.: Simulating Erosion-Induced Soil and Carbon Delivery From Uplands to Rivers in a Global Land Surface Model. J. Adv. Model. Earth Syst., 12, e2020MS002121, 2020.

16. l.230 check the units - also for a

We have double-checked the units of all variables to make sure they are all correct.

17. l.254 sentense ends abruptly - something is missing

l.255 typesetting + space

l.257 conditions to ...? something seems to be missing

Sorry for the edit errors in this sentence. We have modified it. Please see "This aggregated dataset is then used to force the simulation of the actual daily sediment delivery (S_j, g day^{-1} grid^{-1}) in ORCHIDEE-C$_{\mathrm{lateral}}$, simply based on the estimated reference sediment delivery rates of Eq. (1) and on the ratios between actual runoff and land cover conditions and the assumed reference conditions used to create that forcing file (Eq. 4, Fig. S2g)." (lines: 254-257)

18. Fig.1 since POCa;p;s appear later in the text, please add the explanation in the caption. Further, I suspect, it's a typo: POCc !POCs?

We have added the explanation of POC$_a$, POC$_s$ and POC$_p$ in the caption of Fig. 1 and corrected the typo of POCs. Please see "POC$_a$, POC$_s$ and POC$_p$ are the active, slow and passive POC pool, respectively." (lines: 296-297)

19. l.302f is the S_{fast_h2o} correctly placed? - I suspect, it should be after 'water reservoir', since the residence time is _fast or not?

We changed the place of 'S_{fast_h2o}' based on your comment. Please see "the sediment delivery (F_{RO_sed}, g day^{-1}) from uplands in each basin (i.e. each 0.5° grid in the case of this study) initially feeds an aboveground water reservoir (S_{fast_h2o}, m^3) with a so-called fast water residence time." (lines: 300-302)

20. l.305 from the fast water reservoir into the stream reservoir

l.308 τ is thus far undefined, if I am not mistaken

Line 305-308: "Daily water (F_{Fout_h2o}, m^3 day^{-1}) and sediment (F_{Fout_sed}, g day^{-1}) flows from fast water reservoir to stream reservoir are calculated from a grid cell-specific topographic index f_{topo} (unitless, Vörösmarty et al., 2000) extracted from a forcing file (Table 1) and a reservoir-specific factor τ which translates f_{topo} into a water residence time of each reservoir (Eqs. 5, 6)." (lines: 303-307)

To our knowledge, 'to' is better than 'into' in this sentence. Thus we did not change 'to' to 'into'. For the definition of τ, it is a reservoir-specific factor which translates f_{topo} into a water residence time of each reservoir. We clarified this point in lines 306-307.

21. l.311 enters the stream reservoir

We revised this sentence based on your comment. Please see "we assumed that there is no sediment deposition in the fast reservoir, and that all of the sediment in the fast reservoir enters the stream reservoir." (lines: 309-311)

22. l.313 transports

We revised this sentence based on your comment. Please see "The belowground drainage (F_{DR_h2o}, m^3 day^{-1}) only transports DOC and dissolved CO_2 to the stream reservoir (Fig. 1b)." (lines: 312-313)

23. l.316f sediment in the stream . . . determined by $F_{out\ sed}$ . . . sediment input by flooding water

l.318 maybe a matter of specialization of science communities, but is there a difference between resuspension (or erosion) and re-detachment of sediment? (also in Fig.1)

We have revised this sentence based on your comment. Please see "The budget of the suspended sediment in the stream (S_{riv_sed}, g) is determined by F_{out_sed}, the upstream sediment input (F_{up2riv_sed}, g day^{-1}), the sediment input by flooding water returning to the river ($F_{fld2riv\_sed}$, g day^{-1}), the re-detachment of the previously deposited sediment in the river bed (F_{rero_sed}, g day^{-1}), the bank erosion (F_{bero_sed}, g day^{-1}), the sediment deposition in the river bed (F_{rd_sed}, g day^{-1}) and the sediment transported to downstream river stretches ($F_{down2riv\_sed}$, g day^{-1})" (lines: 316-321)

In our model, the resuspension, erosion and re-detachment of sediment are assumed to be the same. All of them describe the sediment flux from previously deposited sediment in river channel to the river stream.

24. l.343 each grid, and we thus require/apply a different approach described in the following (or something similar - as a reader, I was a bit lost with this last sentence)

We have revised this sentence to make it easier to be understood. Please see "Given the difficulty to simulate the detailed hydraulic dynamics of the stream flow at large spatial scale, we thus apply a simple approach described below to calculate the sediment transport capacity." (lines:833-835)

25. l.408 different particle sizes

We revised this sentence based on your comment. Please see "Many studies described the selective transport of POC and sediment of <u>different particle sizes</u>." (line: 387)

26. l.447_ Why did you chose the turnover time for the passive soil organic carbon content the same as the active one and not as the slow one? This also puzzles me a bit later on in the discussion, where you seem to provide good reasons for slower turnover times when soil passive organic carbon is released to rivers (see below).

The decomposition and transformation of soil organic carbon in ORCHIDEE is simulated following the CENTURY model (Parton et al., 1988). In CENTURY, the passive pool (SOC_p) represents the physically protected fraction of SOC. When it is decomposed, 55% of the decayed SOC_p is assumed to be released to the atmosphere in the form of CO_2, and all of the remaining fraction of the decayed SOCp feeds into the active SOC pool (SOC_a), <u>while none of the decayed SOC_p feeds into the slow pool (SOC_s)</u>. Following the scheme of carbon flows between different SOC pools in CENTURY, we thus assume that the passive POC which is originally protected by soil aggregates only feeds into the active POC pool when soil aggregates break down during the sediment transport process. Moreover, even if we assume that the turnover time of the passive POC in river flows is similar to that of the active POC (characterized by the shortest turnover time), the total decomposed POC during the transport process in river networks is only 2.9% (Fig. 7) of the total POC delivered from land to river. This means that uncertainties in simulated POC export to the sea caused by this assumption should be very limited. To demonstrate this further, we have conducted a simulation with the turnover time of the passive POC in river flows assumed to be identical to that of the passive SOC pool (characterized by the longest turnover time). In this case, 0.9% of the total riverine POC is decomposed during the transport process in river networks. Overall, we conclude that the decomposited fraction of riverine POC should range from 0.9% to 2.9%.

27. l.456 I believe this is a bit misleadingly written. I suspect that you mean that the deposited sediment becomes part of the surface soil layer (and does not become a new layer - otherwise, one would end up with lots of layers)

We have revised this sentence based on your comment. Please see "The sediment deposited on the floodplain <u>becomes part of the surface soil layer</u>, and the active, slow and passive POC flow into the active, slow and passive SOC pools in surface soil layer, respectively." (lines: 435-437)

28. l.467 over Europe and parts of Middle East and Africa (. . .)

We have revised this sentence based on your comment. Please see “In this study, ORCHIDEE-$C_{lateral}$ was applied over Europe and parts of Middle East (-30W– 70E, 34N-75N),” (lines: 446-447)

29. l.470 is there any reason for 2 days (and not for one or more than 2)?

To give a more accurate description, we have changed ‘days’ to ‘events’. Please see “The return period of daily bankfull flow ($P_{flooding}$, year), which represents the average interval between two flooding events and is used in this study to produce the forcing file of S_{rivmax} from a pre-run of ORCHILEAK.” (lines: 448-450)

30. l.473 all the flooding waters

We have revised this sentence based on your comment. Please see “as the flooding may occur in several continuous days and all the flooding waters occurring on these continuous days are generally regarded to belong to the same flooding event (supplementary Fig. S3).” (lines: 451-453)

31. l.476 First you make the point that there is substantial spatial variation of $P_{flooding}$ and then you assume it the same for Europe. Why and what are the consequences? - also for your later comparison, l. 506/507

To our knowledge, observational data on $P_{flooding}$ are still very limited, and there is no data capable of providing spatially distributed $P_{flooding}$ for each half-degree grid cell. Therefore, following Schneider *et al*. (2011), we use a constant $P_{flooding}$ to simulate the bankfull flows from all European rivers. Both Schneider *et al*. (2011) and this study (Fig. 2b) indicate that the simulated bankfull flows compare reasonably well with observations even with a constant $P_{flooding}$. To give an explanation on why we use a constant $P_{flooding}$, we have deleted the original sentence “$P_{flooding}$ shows substantial spatial variations following climate and topography (Schneider *et al*., 2011).”, and added the texts “To our knowledge, existing observational data on $P_{flooding}$ are still very limited. Therefore, following Schneider *et al*. (2011), we also use a constant $P_{flooding}$ to simulate the bankfull flows from European rivers and the observed long-term (1961–2000) average bank full flow rate ($m^3 s^{-1}$) at 66 sites obtained from Schneider *et al*. (2011) was used to calibrate $P_{flooding}$ (= 0.1 year, Table 2).” (lines: 453-457)

32. l.478 Following Zhang et al.

We have revised this sentence based on your comment. Please see "Following Zhang et al. (2020), the parameters *a*, *b*, *c* and *d* in Eq. 1 and 2 (Table 2) were calibrated at 57 European catchments (Fig. S2d) against the modelled sediment delivery data obtained from the European Soil Data Centre (ESDAC, Borrelli et al., 2018)." (lines: 457-460)

33. l.493 I suspect you want to refer to Eq. 10 (not 5)?

Sorry for the mistake. The original Eq. 5 has been changed to Eq. 10.

34. l.503 as forcing data

We have revised this sentence based on your comment. Please see "In the 'spin-up' simulation, the PFT maps, atmospheric CO_2 concentrations and meteorological data during 1901–1910 were used repeatedly as forcing data." (lines: 486-487)

35. l.507 (locations see Fig...)

We have revised this sentence based on your comment. Please see "The finally simulated water discharge rates in European rivers were evaluated using observation data at 93 gauging sites (locations see Fig. S4a) from the Global Runoff Data Base (GRDC, Table 1)." (lines: 488-490)

36. l.516f if I interpret your caption of Fig. 4 correctly (and to my knowledge), you can derive POC content by TOC-DOC=POC, which should result in a much larger data base you can compare your model simulations to, right? See my main comments.

Please see our response to your comment #2.

37. l.607_ As mentioned in the main comments part, I would like to see the temporal variability of the simulation in contrast to observations more in-depth discussed (also for POC). I believe understanding this model behavior better could provide important insights.

Please see our response to your comment #4.

l.613 Note that (typo)

We have corrected this typo based on your comment. Please see "Note that the mean and standard deviation of the simulated concentrations at each site are calculated based on the monthly average value" (lines: 600-601)

38. l.625-634 this potentially requires re-working, if TOC-DOC=POC

Please see our response to your comment #2.

39. Fig.7 I might misinterpret here something, but isn't upland loss = delivery to river? If it is, then I cannot relate the values for DOC and POC loss in Fig.7 to the DOC and POC delivery shown in Fig. S11b of the supplementary material (where POC≈18 Tg C yr^{-1} and DOC≈ 7 Tg Cyr^{-1}). Otherwise, some explanations would be helpful to understand the discrepancies.

Sorry for having not updated the Fig. S15 (i.e. the Fig. S11 in the original SI) based on our final simulation results. Now we have replaced the old figure with the latest one. Please see:

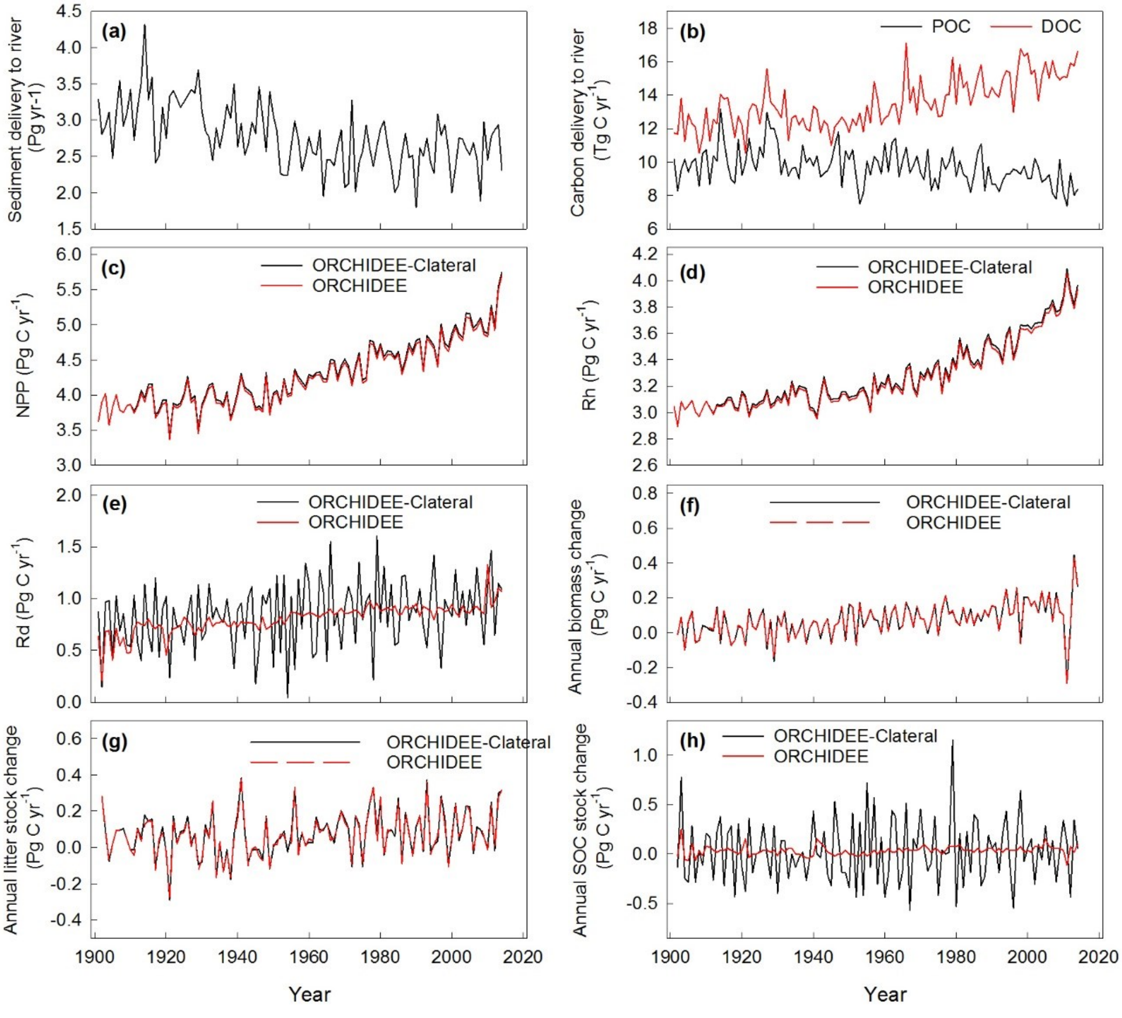


Figure S15 Simulated time series of annual total sediment delivery from upland to river network (a), DOC and POC delivery from land to river network (b), vegetation net primary production

(NPP, c), heterotrophic respiration (Rh, d), respiration due to disturbances like harvest and land cover change (Rd, e), changes in living biomass (f), changes in litter carbon stock (g) and changes in SOC stock (h) in whole Europe from the year 1901 to 2014.

40. l.682 Although the . . . (no 'but')

l.684 rivers are much smaller

We have revised this sentence based on your comment. Please see "Although the sediment discharge rates in some rivers in the Middle East can be as high as that in the Danube or Volga river (Fig. 8c), the POC delivery rates in these rivers are much smaller than in the larger ones (Fig. 9c)." (lines: 666-668)

41. l.723 NEP is undefined, or do you mean NPP? (also in the next line)

We have provided the definition of NEP in the revised manuscript. Please see "which is about 4.7% of the net ecosystem production (NEP (993±255 Tg C yr^{-1}), defined as the difference between the vegetation primary production (NPP) and the soil heterotrophic respiration (Rh) due to the decomposition of litter and soil organic matter (i.e. NEP=NPP–Rh))" (lines: 702-705)

42. l.759 becomes part of the surface soil layer (see comment above)

We revised this sentence based on your comment. Please see "In the floodplains, the newly deposited sediment becomes part of the surface soil layer" (lines: 741-742)

43. l.771_ As written above, I am a bit puzzled about the choice of the turnover time of passive POC in rivers. If I am getting you right, you try to justify the parameters value choice with the decomposed fraction of the overall POC pool. Since you unfortunately never show the fractions of fast, slow and passive POC (would be nice to find such plot in the supplementary material), I wonder, which of the POC contributes most to the overall decomposition loss. How relevant is the choice of this value for your results?

Please see our response to your comment #26 for the reason why we assume the turnover time of passive POC in rivers to be the same as that of active POC. Accurately quantifying the contributions of fast, slow and passive POC to the overall POC decomposition rate in the river network is difficult, because these three POC pools can be transformed to each other during the decomposition process (Fig. 1). Nonetheless, based on the fractions of fast (11%), slow (21%) and passive (67%) POC in the total POC delivery from land to rivers (Fig. R1), and the turnover time of each POC pool, we can still roughly estimate the relative contributions of the three POC pools to the overall decomposition loss. If the turnover time of passive POC is assumed to be same to that of active POC (0.3 year) as performed here, the passive POC then contributes most to the overall decomposition loss. However, had we assumed that the turnover time of passive POC is equal to that of passive SOC (462 years), the overall decomposition loss would have been largely dominated by the two other pools.

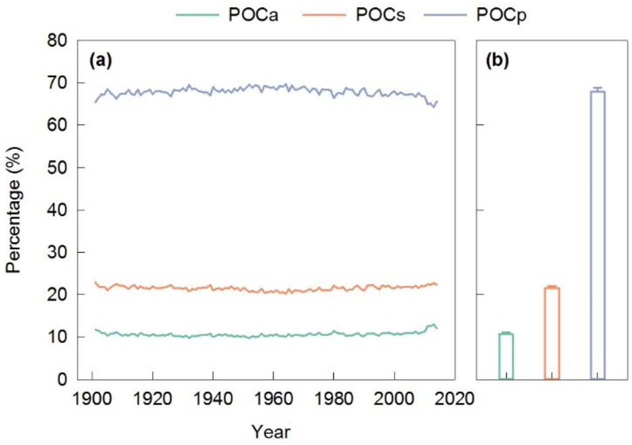


Figure R1. Fractions of the active (POC_a), slow (POC_s) and passive (POC_p) POC in the total POC delivery from land to river networks from 1901 to 2014. Bars and whiskers in figure (b) represent the mean and standard deviation of the fraction, respectively.

44. l.787 that ignoring lateral

We have revised this sentence based on your comment. Please see "Comparison between the simulation results from ORCHIDEE-$C_{lateral}$ with activated and deactivated erosion and river routing modules indicate that ignoring lateral carbon transport processes in LSM may lead to significant biases in the simulated land carbon budget (Figs. 10 and S15)." (lines: 771-774)

45. l.791 here NBP is differently defined than before, if NEP was not a typo (see before)

In our study the net ecosystem production (NEP) is defined as the difference between the vegetation primary production (NPP) and the soil heterotrophic respiration (Rh) due to the decomposition of litter and soil organic matter (i.e. NEP=NPP–Rh)), and the net biome production (NBP) is defined as the difference between NEP and the land carbon loss (Rd) due to the additional disturbances (e.g. harvest, land cover change, and soil erosion and leaching, i.e. NBP=NEP–Rd–DOC and POC to river). Therefore, it is correct to calculate NBP as NPP–Rh–Rd–DOC and POC to river in the original line 791. Nonetheless, to avoid readers misunderstanding the definition of NBP, we have changed the original 'NPP–Rh–Rd–DOC and POC to river' to 'NEP–Rd–DOC and POC to river' in this sentence (lines: 774-777).

46. l.820 Estimations by Maavara

We have revised this sentence based on your suggestion. Please see "Estimation by Maavara et al. (2017) suggests that the …" (line: 875)

47. l.831 cancel out one of the 'e.g.'

Sorry for the mistake. We have deleted one of the 'e.g.'

48. l.843+846 I am a bit uncertain, if evasion is the right word here, maybe better 'effux' (or net outgassing)?

Thanks for your comment. Following many previous studies (e.g. Lauerwald et al., 2015; Duvert et al., 2018; Horgby et al., 2019), we use CO_2 evasion to describe the release of CO_2 from water bodies to the atmosphere.

Lauerwald, R., Laruelle, G., Hartmann, J., Ciais, P., and Regnier, P.: Spatial patterns in CO_2 evasion from the global river network: Spatial patter of riverine pCO_2 and FCO_2. Global Biogeochem. Cycles, 29, 2015.

Duvert, C., Butman D.E., Marx, A., Ribolzi O., Hutley, L.B. CO_2 evasion along streams driven by groundwater inputs and geomorphic controls. *Nature Geoscience*, 11, 813-818, 2018.

Horgby, A. et al. Unexpected large evasion fluxes of carbon dioxide from turbulent streams draining the world's mountains. *Nature Communications*, 10, 4888, https://doi.org/10.1038/s41467-019-12905-z

49. l.873 do you mean: Even though there are still . . .

We have changed this sentence based on the comments from you and other referees. Please see "Even though our model can overall capture the lateral transfers of sediment and carbon in many rivers in central and northern Europe, more observation data are crucially needed to further evaluate the performance of our model, in particular in southern Europe.” (lines: 922-925)

50. l.877 contrasting regions (?)

l.878 to predict

We have changed this sentence based on the referees’ comments. Please see “Thus, in the future, an important aim will be to further calibrate our model against more detailed observation data (e.g. sediment deposition rate in river channels and floodplains) and extend the model application to regions of contrasting climate, vegetation and topography.” (lines: 929-932)

51. l.879_ I am a bit puzzled with regards to the models versus the observations variability at this point. Usually, I would expect that the instantaneous observations show larger scatter than e.g. monthly averaged model simulations. In your simulations, the model range for DOC and TOC is often even beyond the mean envelope range of observations (Fig. S7 and S8). While I understand that the model cannot capture the observational instantaneous values, I would at least expect/tune it to capture annual (or seasonal) to decadal mean values and the envelope (or are there any known biases in the data base with this respect?). Hence, I am not sure, where the origin of this mismatch is coming from and if I can follow that the problem arises only from too little river observations (while I agree that more - long term- observations would help in other aspects like seeing trends, etc.).While not being an expert in LSM modeling, I feel that the model lacks some (potentially important) sort of buffering or counteracting mechanism that modulates down the DOC amplitudes (and other processes that increase the POC variability...). As pointed out above, I believe it would be of value to investigate this deeper and to discuss potential reasons for this model behavior.

We have investigated and discussed the potential reasons for the overestimation of the seasonal dynamics of DOC and TOC. Please see our response to your comment #4 for details.

52. l.900 NEP?

Yes, it is NEP here.

53. Supplementary material

l.11 description of ω sediment transport capacity (typo)

Thanks. We have corrected the typo.

54. l.17 shown

l.18 investigated time of two years

We have revised this sentence based on your comment. Please see “$P_{flooding}$ shown in this figure is 0.1 year as the bankfull flow occurred in 20 days during the investigated time of two years.” (see caption of Fig. S3)

55. l.23 used in this study? (something is missing here)

Thanks for your comment. We have revised this sentence. Please see “**Figure S4** Geographical location of the gauging stations for river discharge (a), bankfull flow (b), sediment discharge (c) and riverine organic carbon discharge (d) used in this study.” (see caption of Fig. S4)

56. l.25 (. . .) of these 57

We have revised this sentence based on your suggestion. Please see “The simulated average net soil loss rates (g m^{-2} yr^{-1}) of these 57 catchments were compared to the average net soil loss rates extracted from the sediment delivery data provided by the ESDAC (see section 2.3 of the main text).” (see caption of Fig. S4)

57. Fig. S3 no response of DOC and CO2 to ω, c_{ebank} and e_{ebed}? - maybe worth to note this explicitly in the caption (or wasn't it analyzed for these parameters?)

We analyzed the response of DOC and CO_2 to parameters ω, c_{ebank} and e_{ebed}. However, as these parameters mainly control sediment erosion and deposition, only sediment and POC show large response to changes in these parameters, and the responses of DOC and CO_2 are very limited. With a 10% changes in these parameters, the relative changes in DOC and CO_2 are generally less than 0.1%.

58. Fig. S4 provide offsets and potentially standard deviations. You mention the offset for the Danube river delta in the text, but are there similar explanations for the Elbe (d) and particularly the Rhine (the latter seem even to be underestimated)? I would suggest to extend this discussion part in the main text (l.528_)

Thanks for your suggestion. Stream channel bifurcation is one of the important reasons for explaining the difference between simulated river discharges and the observed discharge from the main river channel. In the Danube river delta, a fraction of the discharge is actually exported to the sea through the Saint George Branch, in addition to the water discharge through the main river channel (Fig. S5b). This explains why the simulated water discharge rate at the outlet of the Danube catchment is larger than the observation at the Ceatal gauging station, Romania, where only the main stream discharge is measured. However, analysis of satellite images of the Rhine and Elbe catchments do not reveal obvious branching channels bypassing the hydrological gauging sites included in our study. In these latter two catchments, the systematic biases in simulated river discharge should thus be due to other factors, and in particular the discrepancy between the stream routing scheme (delineation of catchment boundaries) extracted from the forcing data at 0.5° resolution and the real river network (Fig. S5). More precisely, we found that the area of the Rhine catchment defined by the forcing data at 0.5° resolution is larger than the area in reality, while the area of the Elbe catchment defined by the forcing data is smaller than the real area.

Following your suggestion, we have extended the discussion to give a more detailed explanation regarding the biases in simulated river discharges caused by the uncertainties in the forcing data of the stream routing scheme. Please see "We recognize that ORCHIDEE-C$_{\mathrm{lateral}}$ may overestimate or underestimate the water discharge rate in some rivers (Fig. 2a), particularly in smaller rivers where discrepancy between the stream routing scheme (delineation of catchment boundaries) extracted from the forcing data at 0.5° resolution and the real river network (Fig. S5) can be substantial. An overestimation or underestimation of the catchment area by the forcing data as respectively found for the Elbe and Rhine will introduce a proportional bias in the average amount of simulated discharge from these catchment. Another problem are stream channel bifurcations which occur in reality, but which are not represented in a stream network derived from a digital elevation model. For example, in the Danube river delta, a fraction of the discharge is actually exported to the sea through the Saint George Branch, in addition to the water discharge through the main river channel (Fig. S5b). This explains why the simulated water discharge rate at the outlet of the Danube catchment is larger than the observation at the Ceatal gauging station, Romania (identify number in the GRDC database is 6742900, Fig. S4m), where only the main stream discharge was measured." (lines: 515-527)

59. Fig. S11 see comment in the main text on budgets of POC and DOC

Please see our response to your comment #39.