Dear editor,

Thanks for sending us the comments from you and the three referees on our manuscript "*Estimating the lateral transfer of organic carbon through the European river network using a land surface model*" (esd-2022-4). We are grateful for your and the referees' constructive comments and suggested amendments. We have carefully studied them, and revised our manuscript accordingly. As a consequence, we believe that our manuscript has been considerably improved.

The following part is our detailed responses to your comments. Please note that your comments are highlighted in **bold** and followed by our responses in regular text.

Sincerely,

Haicheng Zhang, on behalf of all coauthors

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Guideline:

Response to Referee #1: Pages 2 – 4

Response to Referee #2: Pages 5 – 10

Response to Referee #3: Pages 11 – 16

Referee #1

1. General

I highly appreciate the insights into the discharge rates for POC, DOC and TOC (S11/12). It looks, as if the bias becomes reduced for POC, which is promising for long(er) term simulations. Could you enrich the figures by histograms (both DOC and TOC concentration & discharge rate) similar to Fig. 6 (POC) to also enable easier comparison to Fig. 5 in the main text? Can there be anything said for rivers where the model/observations mean ratio flips from smaller to larger 1 or vice versa, when comparing concentration and discharge rate (TOC,DOC, POC; e.g. happened for POC for Ems river at Rheine, where the mean concentration is under-, while the discharge rate overestimated by the model)? - is it purely a bias in measurements or is it catchment area-specific (e.g. land use, different buffering,...)? The latter might be a bit out of scope of the manuscript, though.

Following your suggestion, we have added boxplots of simulated vs. observed TOC and DOC discharge rates per sampling location to Fig. 6. These boxplots give the statistical distributions with mean, median, inter-quartile range, 10th and 90th percentiles, and 5th and 95th percentiles. Please see Fig. 6b,c in the revised manuscript.

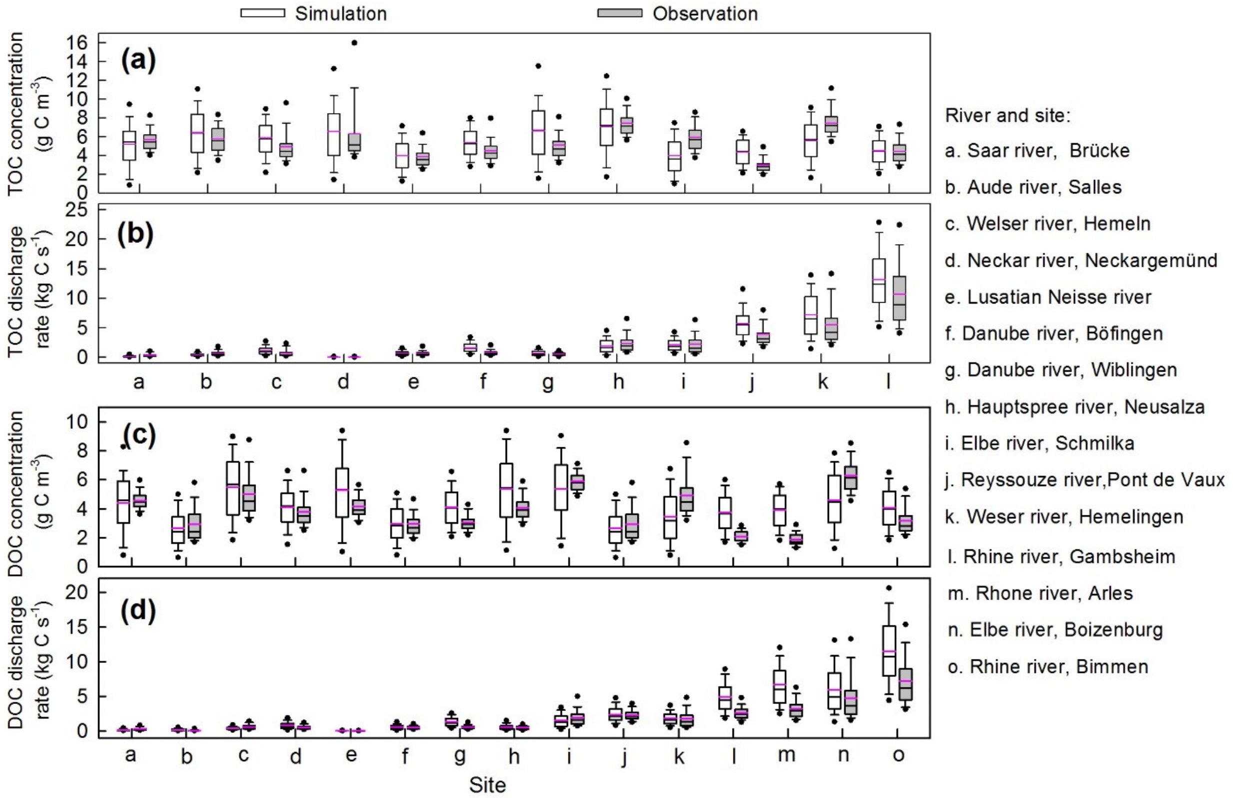

Figure 6 Comparison between the observed and simulated concentrations of total organic carbon (TOC, a) and dissolved organic carbon (DOC, b) in river flows, as well as the discharge rates of riverine TOC and DOC. The black and pink lines in each box denote the median and mean value, respectively. Box boundaries show the 25th and 75th percentiles, whiskers denote the 10th and 90th percentiles, the dots below and above each box denote the 5th and 95th percentiles, respectively.

Indeed, for some rivers, the direction of biases in the simulated TOC (also DOC and POC) concentrations is different from that of the simulated TOC discharge rates, which also depends on the simulated water discharge rates. Uncertainties in the observation data, in the representation of river networks and in the simulated carbon and water cycles of terrestrial ecosystems in our model, as well as the omission of organic carbon inputs from manure and sewage might explain the distinct biases in simulated TOC (or DOC, POC) concentrations and discharge rates. In section 3.4, we have discussed the potential reasons for the uncertainties in our simulation results in details. Moreover, the biases of simulated TOC (or DOC, POC) concentrations or discharge rates depend on specific catchments and there is no general overestimation or underestimation in the simulation results. We also recognize that due to limited observational data, it is still a challenge to quantify the uncertainties in simulated riverine TOC, DOC and POC discharge rates across European catchments, as well as to determine the sources of these uncertainties. In our manuscript, we have now called for more continuous observation data of riverine DOC and POC to better calibrate and evaluate our model as well as reduce uncertainties (lines: 835-968).

2. Further small notes (based on the version, where changes are shown)

p7,l163 enter the free

We have revised the text following your suggestion. Please see "The products of litter and SOC decomposition <u>enter the free</u> DOC pool" (line: 158)

3. p10,l225 much finer

We have revised the text following your suggestion. Please see "CO_2 evasion in inland waters is simulated using a <u>much finer</u> integration time step of 6 minutes." (lines: 218-219)

4. p24,l.564: Bad Honnef

We have revised the text following your suggestion. Please see "POC was measured at only two sites (Bad Honnef (51 measurements) and Bimmen (78 measurements)) in the Rhine catchment and one site (Rheine, 36 measurements) in the Ems catchment (Fig. S3d)." (lines: 529-531)

5. p.25,l585 catchments

We have revised the text following your suggestion. Please see "An over-estimation 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 catchments." (lines: 547-550)

6. p41,l920 capacity? - not sure, what you want to say.

Sorry for the confusing description. We have changed the original text "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" to "Given the difficulty to simulate the detailed hydraulic dynamics of the stream flow at large spatial scale, we thus apply a simple approach (Eq. 8) to calculate the sediment transport capacity" (lines: 865-867)

7. Tab S1: Description

We have corrected the typo of 'Description' in the supplementary Table S1.

8. Fig S6: f) – should it be Koeln/Köln (Cologne)? - and not Koelin?

We have revised the typo of 'Koeln' in the title of supplementary Fig. S5f (i.e. the previous Fig. S6).

Referee #2

1. Comment on the revised manuscript: “Estimating the lateral transfer of organic carbon through the European river 1 network using a land surface model”

The manuscript greatly improved through the revision. From my point of view, the manuscript can be published with minor revisions.

Thanks a lot for your previous comments, as well as your new 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 new comments below.

2. Line 161ff: in this line you use the terminology of labile and stable DOC pools. In the following text you talk about free and absorbed DOC. Later (line 222) you use the term “refractory”. Please use same terminology here.

There are two times two categories of DOC, labile vs. refractory and free vs. adsorbed. Both labile and refractory DOC can be in the soil solution (i.e. free DOC) or adsorbed on the soil minerals (i.e. absorbed DOC). In the previous version of our ms., we sometimes used “stable” as synonym of “refractory”. We acknowledge that this was confusing, and to make the terminology consistent throughout our manuscript, we have now changed ‘stable DOC pool’ to ‘refractory DOC pool’ everywhere in the text. Please see, e.g. “Soil DOC is represented by a labile and a refractory DOC pools, with a high and low turnover rate, respectively.” (lines: 156-157)

3. Line 182ff: “The adsorption, desorption, production, consumption and 184 transport of DOC within the soil column, as well as DOC export from soil to river along with surface runoff and drainage in ORCHILEAK is simulated using the same method as ORCHIDEE-SOM” Does this mean that the processes in soil are the same as in river channel? This is confusing!

With this sentence, we intended to indicate that the method used to simulate soil DOC fluxes in ORCHILEAK is similar to that used in ORCHIDEE-SOM, which we have been introduced at the beginning of section 2.1. These soil DOC fluxes also include the export of DOC from the soil with surface runoff and drainage. These exports were already represented in ORCHIDEE-SOM, but not transport and reaction of DOC in the river channel, the representation of which was introduced with ORCHILEAK. To give a more accurate description, we have changed this sentence to “The method used in ORCHILEAK to simulate the adsorption, desorption, production, consumption and transport of DOC within the soil column, as well as the DOC export from the soil column with surface runoff and drainage is similar to that used in ORCHIDEE-SOM.” (lines: 173-176)

4. Line 352: TC as defined below, is not the maximum load but maximum suspended sediment concentration. Furthermore, rephrase the sentence along the following line: First TC is maximum concentration. Second, if erosion or deposition occurs will depend on the actual concentration with respect to TC.

Following your suggestion, we have rephrased this sentence. Please see “Sediment transport capacity (*TC*, g m^{-3}) is defined as the maximum concentration of suspended sediment that a given flow rate can carry. TC and the flow rate determine the amount of sediment that can be transported to the downstream grid cell (e.g. $F_{down2riv\_sed}$, $F_{riv2fld\_sed}$). Suspended sediment loads that are in excess to maximum possible amount of transported sediment will deposit on the river bed (F_{rd_sed}). If sediment loads are below that maximum possible amount, erosion of the river bed (F_{rero_sed}) or river bank (F_{bero_sed}) takes place” (lines: 348-353)

5. Line 385: please highlight that TC is expressed as suspended sediment concentration

We have revised the text based on your suggestion. Please see “In this study, we used an empirical equation adapted from the WBMsed model, which has been proven effective in simulating the suspended sediment discharges in global large rivers (Cohen et al., 2014), to estimate the *TC* (g m^{-3}) of suspended sediment in stream flow” (lines: 355-357)

6. Line 386ff: What is the difference between daily stream flow rate and daily downstream water discharge?

The ‘stream flow rate’, denoted by q_j (m^3 s^{-1}) in our manuscript, is the average water flow rate on day *i*. The daily downstream water discharge, denoted by $F_{down2riv\_h20}$ (m^3 day^{-1}) is the amount of water flowing out of the stream reservoir of a modelling grid cell to the next downstream grid cell each day. We have provided the definition of these two variables, as well as their units in our manuscript. Please see “q_j (m^3 s^{-1}) is stream flow rate on day j, e_1 is an exponent depending on the upstream drainage area (DA, m^2), $F_{down2riv\_h20}$ (m^3 day^{-1}) is the daily downstream water discharge from the stream reservoir.” (lines: 361-363)

7. Line 605ff: Indicate why you use WATEM / SEDEM results to compare sediment delivery rates from your model.

To our knowledge, there is still no large-scale observation data on sediment delivery rates from land to river networks in Europe. Therefore we compared our simulation results to the estimates from WATEM / SEDEM, which simulate soil erosion and upland deposition rates across Europe using high-resolution data of topography, soil erodibility, land cover and rainfall. The WATEM / SEDEM model has been calibrated and validated using observed sediment fluxes from 24 European catchments (Borrelli et al., 2018). We have added some texts to explain why we use the simulation results from WATEM / SEDEM. Please see “To our knowledge, there is still no large-scale observation data on sediment delivery rates from land to river networks in Europe. Therefore, following Zhang et al. (2020), the parameters a, b, c and d in Eq. 1 and 2 (Table 2) were calibrated for 57 European catchments (Fig. S3d) against the modelled sediment delivery data obtained from the European Soil Data Centre (ESDAC, Borrelli et al., 2018). The sediment delivery data from ESDAC was derived from WaTEM/SEDEM model simulations using high-resolution data of topography, soil erodibility, land cover and rainfall. This model was calibrated and validated using observed sediment fluxes from 24 European catchments (Borrelli et al., 2018).” (lines: 484-492)

8. Line 613: ‘sediment discharge rates’ □ use same terminology as above (e.g. sediment delivery) and highlight that you compared with observed measurements here!

Similar to previous publications, we actually use the ‘sediment delivery’ to describe the sediment transfer from land to river channel, and use the ‘sediment discharge rate’ to describe the sediment transfer in the river channel. That is why we have used different terminologies here. In addition, we have revised our manuscript to highlight that our simulation results of sediment discharge rates were compared with observed measurements. Please see “ORCHIDEE-$C_{lateral}$

reproduces 83% of the inter-site variation of the observed riverine sediment discharge rates across Europe (Fig. 4b).” (lines: 576-577)

9. Line 644ff: Please shortly describe why the differences between the DBs occur.

We have added some texts to explain the differences between SOC stocks extracted from the observation-based soil databases. Please see “We noticed that the SOC stocks extracted from these observation-based soil databases show considerable differences (vary from 106 to 249 Pg C), as they have been produced using different clusters of site-level SOC measurements and different interpolation methods to produce global gridded SOC stocks from site-level measurements (Shangguan et al., 2014; Hengl et al., 2014; Sanderman et al., 2017).” (lines: 608-612)

10. Line 688ff: POC is a function of discharge in many river systems and in the Rhine river (see for instance Hoffmann et al 2020). It would be interesting to see rating plots of POC~discharge for measured and modelled systems. This will highlight importance differences for various flow regimes (e.g. low flow / high flow).

Thank you for your suggestion. We agree that POC might be a function of discharge in many river systems, and the function generally follows a power law (Syvitski et al., 2000; Hoffmann et al, 2020). Actually, we have used a power law (see Eq. 8 in our manuscript) of discharge rate to calculate the sediment transport capacity of river flow, which can strongly affect riverine POC transport. We also agree that an analysis of the rating curves between POC and water discharges is very interesting and is helpful to better understand the riverine POC transfers in different flow regimes. However, we feel that this analysis is a bit out of the scope of the present study, which is mainly intended to describe our model development and its primary evaluation across all ecosystems it encapsulates. We also discuss uncertainties and shortcomings of the current version of our model.

Nonetheless, following your suggestion, we have analyzed the relationship between riverine POC concentration and river discharge rate (Fig. R1). We find that the water discharge rate cannot well explain the POC concentrations at the three sites included in our study, based on both observation and simulation data. In addition to the amount of runoff, seasonal variations of vegetation cover, rainfall intensity and SOC content might have also significant effects on the riverine POC concentrations. Note that the simulated POC concentrations and water discharge rates shown in Fig. R1 is the monthly average values, which thus might not be able to represent the actual instantaneous relationship between POC concentration and water discharge.

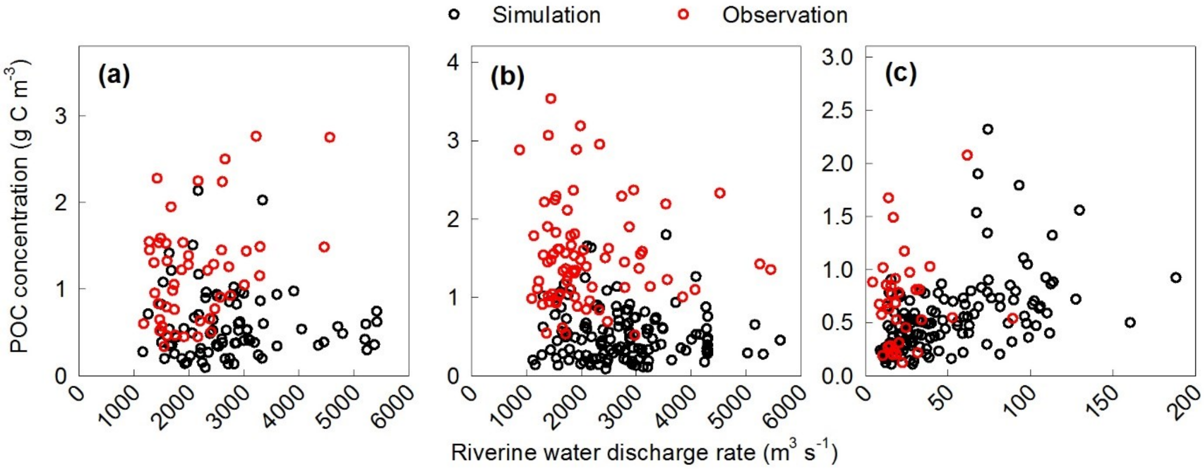

Figure R1 Relationship between riverine particulate organic carbon (POC) concentration and riverine water discharge rate at three sites in Europe (a: Rhine river at Bad Honnef; b: Rhine river at Bimmen; c: Ems river at Rheine).

References:

Hoffmann, T. O., Baulig, Y., Fischer, H., and Blöthe, J.: Scale breaks of suspended sediment rating in large rivers in Germany induced by organic matter. Earth Surf. Dynam., 8, 661–678, 2020.

Syvitski, J. P., Morehead, M. D., Bahr, D. B., and Mulder, T.: Estimating fluvial sediment transport: The rating parameters, Water Resour. Res., 36, 2747–2760, 2000.

11. Line 708: Bare rock and ice are not per se indicative of low erosion rates. Typically, bare rock is observed in mountainous regions, which are characterized by high erosion rates. Ice, if associated with glaciers, is also indicative of increased rates. Please rephrase!

We have deleted this sentence in the revised manuscript.

12. Line 720: The Danube suspended sediment yields strongly declines due to the construction of dams, with are considered as the major sediment sinks along the Danube. If you mention the Danube in the context of sediment deposition, you should indicate the importance of dams. Please refer to Habersack et al (2016, Science of the Total Env) in this context.

We agree that the construction of dams can strongly decrease suspended sediment yield. Omission of the representation of dams in our model might result in an underestimation of sediment deposition in river channels. We actually have discussed this issue in our manuscript. Please see "In addition, the impact of artificial dams and reservoirs on riverine sediment and carbon fluxes is also not represented in our model. Construction of dams generally leads to increased water residence time, nutrient retention, and sediment and carbon trapping in the impounded reservoir (Habersack et al., 2016; Maavara et al., 2017), and can also affect the downstream flooding regime and frequency (Mei et al., 2016; Timpe and Kaplan, 2017). Estimation by Maavara et al. (2017) suggests that the organic carbon trapped or mineralized in global artificial reservoirs is about 13% of the total organic carbon carried by global rivers to the oceans. To more accurately simulate the lateral carbon transport, we plan to include the soil and carbon redistribution within headwater basins and the effects of dams and reservoirs on riverine sediment and carbon fluxes into our model in the near future." (lines: 902-912)

Moreover, we have cited Habersack et al (2016, *Science of the Total Env*) in the revised manuscript.

Referee #3

1. The authors revised the manuscript thoroughly. It is much clearer now and mistakes/errors have been removed. Most of the questions and comments I had have been answered convincingly. Thanks for that. However, some of the explanations did not make it into the manuscript. I would work on that further. Below are the points listed that should be changed.

I recommend it for publication after minor revision.

Below I list the points that only have been revised insufficiently.

Thanks a lot for your positive feedback on our responses to most of your previous comments. We are sorry for have not sufficiently solved all of your previous concerns. We have carefully studied your points listed below and revised our manuscript accordingly. Please see our specific responses below.

2. My original comment:

Is the litter also included as a part of OC input? It is mentioned a few times, but not explained or shown in Fig 1?

Author's response:

No, litter is not included as a part of OC input. In our model (also many other erosion models), litter is an important factor (the C-factor in MUSLE, Eq. 1) for protecting soil from being eroded.

My comment on that:

This is still not clear in the manuscript.

As we explained before, litter is not a part of riverine OC input. However, litter cover can affect the cover management factor of MUSLE (denoted by C_j, Eq. 4), and further affect the sediment and SOC erosion rates. We assumed that no litter can be eroded and transported to the river networks. Therefore, the lateral transport of litter is not represented in the original Fig. 1 (i.e. the Fig. 2 in the revised ms.). To address your concern, we have given a more clear description of the fate of litter in the revised manuscript. Please see "Daily POC delivery to river headstream in each 0.5° grid cell is finally simulated based on the sediment delivery rate and the average SOC concentration of surface soil layers (0-20 cm). We assumed that litter cannot be eroded and transported to the river network, however, it can affect soil erosion rate through the cover management factor of the MUSLE model (denoted by C_j, Eq. 4).” (lines: 290-294)

And “C_j (0-1, unitless) is the daily actual cover management factor, calculated based on the fraction of surface vegetation cover, the amount of litter stock and the biomass of living roots in each PFT within each 0.5°×0.5° grid cell.” (lines: 285-288)

3. My original comment:

Are the two litter pools part of the POC in soil? Please clarify.

Author’s response:

The litter pools are also organic carbon pools. However, they did not contribute to the lateral POC transfer. In our model, only soil organic carbon (SOC) contributes to the lateral transfer of POC. To avoid misunderstanding, we have revised this sentence from the original “ORCHIDEE-SOM subdivides the particulate organic carbon stored in soil into two litter pools (metabolic and structural) and three SOC pools (active, slow and passive) that differ in their respective turnover times.”. to “ORCHIDEE-SOM represents two litter pools (metabolic and structural) and three SOC pools (active, slow and passive) that differ in their respective turnover times.” (lines: 151-153)

My comment on that:

Still not clear in the text. Please clarify.

We actually use POC to describe the particulate organic carbon in river streams. The riverine POC is contributed by SOC pools, without any contribution from upland litter pools. Or we can say that the particulate organic carbon in soil is called SOC, and the particulate organic carbon in streams is call POC. We have clearly indicated this in the revised manuscript (lines: 285-294). Please see our response to your comment #2.

4. My original comment:

How does the DOC 'enter' the water? Does it depend on vegetation cover? Please clarify.

Author's response:

When flooding water is infiltrated into soil, the DOC and CO2 in flooding water will naturally enter into soil along with the infiltrating water. The infiltration rate of flooding water depends on soil properties and soil water content, but does not depend on vegetation cover. As this process is originally developed in the ORCHILEAK model and has been introduced in detail in Lauwerwald et al. (2017), we only gave a brief overview about this part of the model.

Lauerwald, R., Regnier, P., Camino-Serrano, M., Guenet, B., Guimberteau, M., Ducharne, A., Polcher, J., and Ciais, P.: ORCHILEAK (revision 3875): a new model branch to simulate carbon transfers along the terrestrial–aquatic continuum of the Amazon basin. Geosci. Model Dev., 10, 3821-3859, 2017.

My comment on that:

Thanks for this explanation. Please add this explanation in short form to the manuscript.

We have added this explanation to the revised manuscript. Please see "DOC and CO_2 in flooding waters can enter into soil DOC and CO_2 pools along with the flooding water infiltrated into soil. The infiltration rate of flooding water depends on soil properties and soil water content, <u>but does not depend on vegetation cover</u>." (lines: 190-191)

5. My original comment:

Table 1: Is a spatial resolution of 0.5° (55km*55km max) sufficient to inform the model about the 'Area fraction of river surface'? Later it is mentioned that for the delta of the Danube the high resolution was problematic (because of gauging station data available). But is there also a problem of scale for other data? E.g. 'maximum water storage in river channel' is also only on 0.5°.

Author's response:

The area fractions of river surface or floodplain in each grid cell at 0.5° are derived from high-resolution (e.g. 3″ or 90 m) topographic or satellite data (see the references in the Methods) (e.g. Fig. R1 below). Thus, these area fraction data should be reliable. Indeed, there can be many different river channels in each 0.5°×0.5° pixel in reality. However, it is almost impossible for a global land surface model to explicitly simulate the riverine processes for each individual river channel. Thus we assume that there is one virtual river channel in each 0.5°×0.5° pixel (line 346). The surface area of this virtual river is the sum of all real rivers and the flow direction of this virtual is assumed to be same to the largest real river.

My comment on that:

Thanks. This is helpful. Please add a short version of it to the manuscript.

Following your suggestion, we have added these contents to the revised manuscript. Please see "Note that the maximum area fractions of river surface and floodplain in each basin (i.e. each 0.5°×0.5° grid cell in this study) are derived from high-resolution topographic data (Table 1). As it is difficult to explicitly represent all real river channels in a global land surface model (due to the limit of computing efficiency of current computers), we assume that there is one virtual river channel in each 0.5°×0.5° pixel. The surface area of this virtual river is the sum of all real rivers and the flow direction of this virtual is assumed to be same to the largest real river (Lauerwald et al., 2015)." (lines: 179-185)

6. My original question:

Is wind speed considered for the CO2 evasion?

Author's response:

In current version of our model, the effect of wind speed on CO2 evasion is not represented.

My comment on that:

Please add this information to the manuscript.

We have added this information in the revised manuscript. Please see "The effect of wind speed on CO_2 evasion is not represented in the current version of ORCHILEAK." (lines: 223-226)

7. My original comment:

l. 243 mentions a 'management factor', which is only explained in l.264.

Author's response:

In the original line l.243, we only give a general explanation on the definition of Cref in MUSLE model (i.e. the cover management factor) (see lines 241-242 in the revised ms). In original line 264, we provided the specific method for calculating the cover and management factor (Cj) (see 265-268 in the revised ms).

My comment on that:

I still think that a short explanation of 'management factor' should be added at the first appearance of the phrase. Please just move '(calculated based on the fraction of surface vegetation cover)' from the second to the first occurrence.

C in the MUSLE model represents the cover management factor, which depends on the vegetation cover and storage of plant debris. In different studies, the C-factor can be calculated using different equations. At the first appearance of C-factor, we intended to give a general explanation of its meaning in the MUSLE model, and also provide the preset value of the C-factor at the reference state (i.e. $C_{ref} = 0.1$, Eq. 1). In the second instance where C-factor appears (i.e. C_j, Eq. 4), we introduced the specific method for calculating C_j in our model. Nonetheless, to make the readers better understand the meaning of the C-factor on its first occurrence in the text, we have now added some text. Please see "C_{ref} (0-1, dimensionless) in Eq. 1 represents the cover management factor which depends on vegetation cover and storage of plant debris (see below). The value of C_{ref} is set to 0.1 for the reference state." (lines: 253-254)

8. My original comment:

ll. 250. It is not clear on which resolution the model runs. Some of the input is fine scale (e.g. 250m for floodplains), but then the results are aggregated to 0.5°. Please clarify.

Author's response:

(…) From Fig. S2, you can find the model is finally run at a spatial resolution of 0.5°. The headwater basins cover all upland areas, including not only the upland regions where the main streams originate, but also all upland regions where the tributary streams originate.

My comment on that:

I think that is a very helpful figure. I know that is mainly taken from Zhang et al. 2020. But could you move it to the main manuscript and refer to it as 'adapted after Zhang et al.'? It shows very informative how the different resolution and inputs work together.

Following your suggestion, we have moved the original Fig. S2 to the main text. Please see:

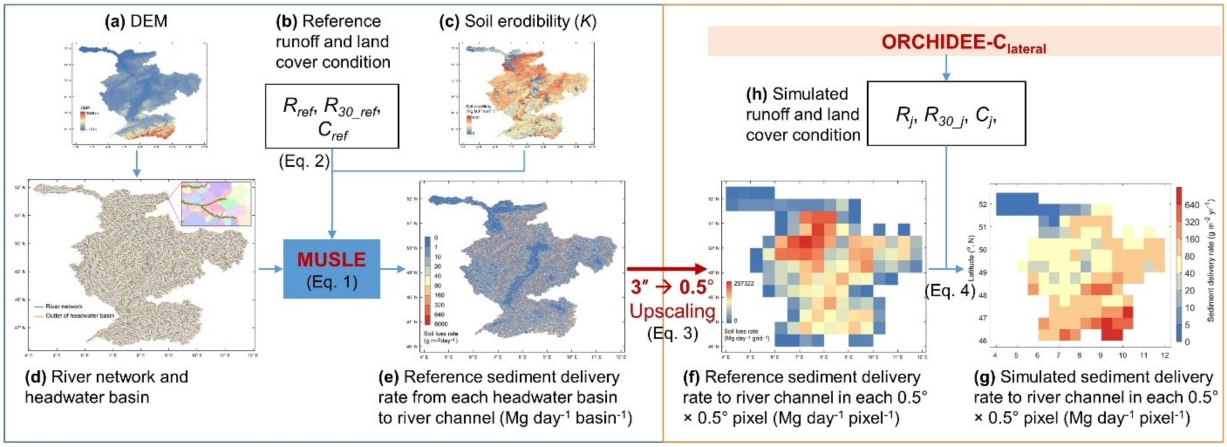

Figure 1 Upscaling scheme used in ORCHIDEE-MUSLE (Zhang et al., 2020) and ORCHIDEE-$C_{lateral}$ for calculating the sediment delivery rate from headwater basins to river networks. MUSLE is the Modified Universal Soil Loss Equation; DEM is the digital elevation model (m); *K* is the soil erodibility 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_{iday}, R_{30_iday} and C_{iday} are the simulated daily total runoff depth, daily maximum 30-minutes runoff depth and daily cover management factor, respectively. This figure is adapted from the Fig. 1 in Zhang et al. (2020).

9. My original comment:

ll. 442. Why do you refer and explain so much about SOC here? The section title is 'POC transport and decomposition'. Maybe some reference to the SOC section would help to clarify.

Author's response:

The scheme for simulating POC decomposition in waters follows that for SOC. With the explanation on SOC decomposition here, we intend to explain how the decomposition of POC is simulated and how we represented the accelerated POC decomposition during the transport process due to the breakdown of sediment aggregates.

My comment on that:

Please add this shortly at line 481. 'We assumed that the base 479 turnover times of active (0.3 year) and slow (1.12 years) POC pools are the same as for the 480 corresponding SOC pools. < In this paragraph we therefor refer to the scheme for SOC. >'

We have added this information to the revised manuscript. Please see "The representation of POC deposition and transformation in the aquatic reservoirs and bed sediment involve as well decomposition, which follows largely the scheme used for SOC (Fig. 2a)." (lines: 436-438)