

Review of

Zhang et al. Estimating the lateral transfer of organic carbon through the European river network using a land surface model.

by: Haicheng Zhang, Ronny Lauerwald, Pierre Regnier, Philippe Ciais, Kristof Van Oost, Victoria Naipal, Bertrand Guenet, and Wenping Yuan

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.

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.

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.

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.

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.

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.

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.

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.

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. >'