

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 submitted manuscript describes a development of ORCHIDEE, in which several branches have been combined to better assess the global carbon cycle. As the authors state it has long been ignored that the carbon transport from land, via rivers to the ocean also plays an important role. I was developing something comparable a few years back and am very certain about the importance and high relevance of such a model enhancement.

The results are promising. But the description of the methods and the presentation of the results can be improved, e.g., there are lengthy quotes in the methods which can be reduced, or some of the table/figures in the SI should be moved to the main text for clarification and a better understanding.

Overall, I think the content of the manuscript is convincing but can be strengthened by removing unnecessary parts and moving some explaining information from the SI to the main text. I believe that the manuscript can be improved, and I recommend it for publication after major revision.

Below I list some unclear sentences, problematic points, and questions. This list is chronological and not based on the importance of the points. I was much more detailed in the methods, but think that the results/discussion section can also profit from the listed points.

- l. 86. 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?
- Please also mention the branch names (e.g. in l. 114)
- An overview map of the catchments would be helpful or at least of how much of the drainage area of Europe is covered.
- l. 135. Is 'bare soil' a PFT (plant functional type)?
- l. 148: Are the two litter pools part of the POC in soil? Please clarify.
- ll. 156-162 is a quote. Please shorten or refer right away to the cited paper.
- l. 183. How does the DOC 'enter' the water? Does it depend on vegetation cover? Please clarify.
- 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°.
- l. 207. The temporal resolution of 6'' for CO<sub>2</sub> evasion is totally reasonable, but for the DOC decomposition it seems to be too much (as for the litter added, l. 212), also given the fact that the temperature (on which the decomposition is also depending) only changes every 3h.
- Why is there a water temperature of 28° assumed? Is this realistic?
- ll. 212-218. Again, a quote. Can be shortened. Is wind speed considered for the CO<sub>2</sub> evasion?
- ll. 227-238. Again, a pretty long quote.
- l. 243 mentions a 'management factor', which is only explained in l.264.

- II. 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.
- II. 254-258. The sentence is not understandable.
- I. 268. Why only headwater?
- Figure 1. (I like it.) It would be helpful if the naming in part a and b would be consistent. Having the time steps mentioned in a separate box is helpful and keeps the figure clean, but it is not clear which processes belong to which group. E.g. 'deposition' - mentioned several times in the figure is not part of the 'time step' box; 'decomposition' in the time-step box consists of 'litter and SOC' but the litter can't be found in the figure. It could also be helpful to add the abbreviations from part b to part a, to make the link clearer and easier to understand. (b) is missing in the caption.
- II. 329-343 and II. 344-353. This is much more discussion than methods. Can be shortened and should be moved.
- II. 384. What is the effect of scale here if you move the OC from one catchment to the next? How does it differ between large and small catchments?
- Section 2.2.2. What is the exact difference between sediment flow and the described POC flow, if it's closely linked to sediments? Is the POC calculations the same as clay?
- I. 438. You use the water temperature to calculate the processes, but as input you use air temperature. How do you accommodate for the difference and the time-lag (e.g. over the course of a day)?
- II. 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.
- L. 468. You refer to Figure S1 in the SI, but this figure shows the return period over two years.
- I. 484. A map or a list of the catchments would be helpful (as in SI Figure S2(d)).
- I. 549. Why is that set to 0.1? It is not clear at this point.
- Fig 3-5 are convincing.
- Section 3.1.3. Your model simulates a too low SOC stock while the TOC and DOC concentrations look better. I would also discuss more here that the temporal pattern of observation and simulation does not match (Fig. S 8), although the mean looks promising.
- Fig 5. I would add the names of the rivers as a side panel instead of having the letters.
- Fig. 6. I suggest to add a panel for discharge (observed vs. simulated).
- II. 645. I am glad to read about the effect of the vegetation type more explicitly here. It could be mentioned earlier.
- I. 662. Is the difference between 3.0 Pg and 2.3 Pg significant? In the corresponding figure S11a there is a lot of fluctuations over the years.
- Fig 7a. The percentage for the fluxes does not sum up to 100. Why?
- I. 682. You mention several times 'small rivers'. Did you conduct a classification for the rivers or is it more a vague grouping?
- Section 3.3. There are several strangely set parentheses.
- II. 738. I like this clear listing and explanation (first, second, third). It makes it easier to follow the argumentation.
- II. 752. What is the effect of anaerobic conditions in the sediment? Wouldn't the decomposition be lower then?
- Section 3.4. I think that is a necessary part of papers like that. Thank you.

Some of the questions have been answered later in the manuscript but already at that (above mentioned) point I would like to know.

There are also some more colloquial phrases such as 'basically' (e.g. l. 299) or 'so-called' (e.g. l. 302 and 303). I suggest exchanging or removing them.