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

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 CO_2 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-$C_{lateral}$ 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.

General comments

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 $ORCHIDEE_{lateral}$ branch. I understand that modeling such a complex river network is extremely challenging. Particularly when considering the trade offs 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.

- 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).

- 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.

Minor comments

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

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

Main document

l.60 predicting or projecting?

l.75 have been developed

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

l.86 How about new production in rivers?

l.154 go to → enter

l.180 a forcing file

l.191 by a basin-specific

l.207 finer

l.220 Sediment and particulate organic carbon delivery... (opposed to CO_2 carbon)
Maybe a question of a non-initiated reader (and potentially something to clarify): are basin and grid cell in your model description interchangable or how is a 'basin' defined in the model realm?

l.230 check the units - also for a

l.254 sentense ends abruptly - something is missing

l.255 typesetting + space

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

Fig.1 since $\mathrm{POC_{a,p,s}}$ appear later in the text, please add the explanation in the caption. Further, I suspect, it's a typo: $\mathrm{POC_c}$ →$\mathrm{POC_s}$?

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

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

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

l.311 enters the stream reservoir

l.313 transports

l.316f sediment in the stream . . . determined by F_{Fout_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)

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)

l.408 different particle sizes

l.447ff 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).

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)

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

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

l.473 all the flooding waters

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

l.478 Following Zhang et al.

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

l.503 as forcing data

l.507 (locations see Fig...)

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.

l.607ff 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.

l.613 Note that (typo)

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

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$\approx$ 18 $\mathrm{Tg\,C\,yr^{-1}}$ and DOC$\approx$ 7 $\mathrm{Tg\,C\,yr^{-1}}$). Otherwise, some explanations would be helpful to understand the discrepancies.

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

l.684 rivers are much smaller

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

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

l.771ff 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 ion 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?

l.787 that ignoring lateral

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

l.820 Estimations by Maavara

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

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

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

l.877 contrasting regions (?)

l.878 to predict

l.879ff 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.

l.900 NEP?

Supplementary material

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

l.17 shown

l.18 investigated time of two years

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

l.25 (...) of these 57

Fig. S3 no response of DOC and CO_2 to ω, c_{ebank} and e_{ebed}? - maybe worth to note this explicitly in the caption (or wasn't it analyzed for these parameters?)

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.528ff)

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