

Interactive comment on “ORCHIDEE MICT-LEAK (r5459), a global model for the production, transport and transformation of dissolved organic carbon from Arctic permafrost regions, Part 2: Model evaluation over the Lena River basin” by Simon P. K. Bowring et al.

Anonymous Referee #2

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General points

In this manuscript, using the ORCHIDEE MICT-LEAK described by the first part in accompanying paper, the authors assessed production, concentration, CO₂ evasion, and riverine transport of dissolved organic carbon (DOC) over the Lena River Basin. They conducted long-term simulations and made attempts to factor out driving factors in DOC change in the study area. The research topic is potentially interesting in terms

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of large-scale carbon budget, land-ocean linkage, and carbon-climate interactions. For example, the long-term increase of DOC discharge (e.g., Fig. 4a) looks intriguing, because this can affect biogeochemistry in the Arctic Ocean.

On the other hand, I have two major concerns on this manuscript. First, the simulated results were compared only with several literature data: e.g., Raymond et al. (2007), Kutscher et al. (2017), and Denfeld et al. (2013). The comparisons were not adequately quantitative, and so I could not figure out whether the model well captured observations. The low performance in simulating river discharge may indicate that the model hydrology should be improved before conducting DOC-related analyses. Second, the model simulations were conducted at a spatial resolution of 1 degree (about 100 km), but it looks too coarse to capture the spatial heterogeneity in this region. As the authors stated (Line 535), the model could not include small streams because of the coarse-scale river-routing scheme.

The manuscript provides numerous figures and text is a bit lengthy. In contrast, Simulation Rationale and Setup sections are brief and I felt inadequate. Data for comparison were described in Results and Discussion sections (e.g., Line 365–368, Line 615–621). I recommend moving these data descriptions to a section in Methods and Data. Therefore, the manuscript can be largely truncated and should be thoroughly reorganized.

Specific points

Line 45–46: In main text, no part discussed about ‘1.8°C warming’ and ‘+85.6 ppm CO₂ rise’. Why did you mention these values in Abstract?

Line 81: Did you examine the accuracy of GSWP3 in the study region? Especially for precipitation, some climate datasets may have serious biases.

Line 580: Remove (g C m⁻² d⁻¹).

Line 787: Why did you discuss about NPP and soil respiration of Siberian forests in

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this position of the manuscript? I lost context here.

Line 869: As long as I know, a version of ORCHIDEE (e.g., Naipal et al., 2018, *Biogeosciences*, 15, 4459–4480) includes POC erosion module.

Line 924: The ratio of DOC export relative to NPP, $\sim 1.5\%$, would be an important result but does not appear in Abstract.

Interactive comment on *Geosci. Model Dev. Discuss.*, <https://doi.org/10.5194/gmd-2018-322>, 2019.