

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

Thanks to the authors for their thorough incorporation of the reviewers comments. This improved the manuscript. I only have some minor general comments and a few small further points left. After this, I believe the manuscript is suitable for publication.

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.

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

p7,l163 enter the free

p10,l225 much finer

p24,l.564: Bad Honnef

p.25,l585 catchments

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

Tab S1: Description

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