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Earth Surface Dynamics Discussions

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Interactive comment

Interactive comment on "Scale-breaks of suspended sediment rating in large rivers in Germany induced by organic matter" *by* Thomas O. Hoffmann et al.

Anonymous Referee #2

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Hoffmann et al. present a new conceptual model that allows to distinguish sediment load into organic and inorganic shares. The authors apply this model to an impressive number of gauging stations where manually sediment concentrations are estimated. Basically, the authors apply the classical sediment rating curve, though, extend it to account for varying ratios between organic and inorganic constituents. In general, I see the manuscript by Hoffmann et al. as a relevant contribution and, thus, consider it as worth being published in ESurf.

While reading the manuscript, several concerns and/or suggestions arise:

- The model the authors present is a way to analyze a static system. However, the

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authors mention that the model is also applicable to study river dynamics. I think it is important to highlight, what the authors refer to when analyzing the dynamics. As I understand, the authors restrict dynamics in a spatial mode, i.e. intrinsic vs. extrinsic. What the author don't study, and I think this is important to mention, is the temporal dynamics. As the authors state in the introduction, temporal dynamics may be analyzed using hysteresis loops (among other). Maybe it is too much additional work and maybe beyond the scope of this manuscript: Did the authors looked on the hysteresis loops, too? I think this is important, at least, to be discussed.

- The section of the three methods applied to quantify the scale-breaks of suspended sediment is a bit unclear. I am convinced that better explaining the three distinct methods, eventually doing a bit more math, would improve the manuscript. For example, I cannot see how the authors defined the subsets used in the second method, i.e. how do the authors construct the "sequences" of discharge Qi? I am also curious why the authors did not use a change-point detection algorithm and applied a piecewise regression to a lower and higher flow regime. I am not saying that the approach chosen by the authors is "wrong", yet I was just interested in more details on the methods chosen.

- The authors considered the geometric mean in their study. Later in the manuscript, they state, however, that the simple average is $\sim$0.8 x the geometric mean (L 269). I am wondering why the authors did not chose a simple average from the very beginning?

- Regarding the sampling routine, I was wondering if the same sampling protocol has been applied for both the daily and weekly measurements? Did the sampling involve also depth-integration?

- The authors explain possible interpretations of the coefficients. Yet, the part around line 159 (MTb L-(1+3b) is not clearly written. Maybe the authors can provide some better explanation to follow their reasoning.

- L 178: Therefore, Q values were classified into equally spaced classes at a log-scale. How many classes exactly?

- L 150: "Chla was used as a proxy. . . for biomass dynamics" What do the authors refer to here exactly when mentioning dynamics? Better to use simply load?

- L8: major relevance for sustainable sediment management. What is that exactly and maybe I missed it, but where do the authors consider this in their manuscript?

- L 44: "Water flow velocities regulate the water residence times, which in turn affect the time for phytoplankton growth in river systems. Low flow conditions with increased residence times provide favourable conditions for phytoplankton growth or even blooms. In contrast, short residence times can strongly reduce the share of autochthonous biomass in suspended sediments, even if light availability, temperature and nutrient levels are not limited (Fischer, 2015; Quiel et al., 2011)." This argument is not completely clear to me. I see the time restrictions for phytoplankton growth given a fast draining river. However, it depends on where you sample, I guess, too. Given high flow velocities, I assume that the concentration of phytoplankton is indeed relatively low in the water column. However, as load is the product of concentration times discharge, the overall phytoplankton load may be high, too. I am not a biologist. Maybe the authors can better explain their thoughts on that and how this may affect the results and findings they present here.

- The authors used coffee filter and stated that the pore diameters of 0.7 to 1 μm. How was this number determined?

- L 109: Specific discharge. I assume that this is well known to most of the readers. Regardless, I think it would be good to define it here. The same is true for "long-term discharge weighted averages of SSC". Please define this, too.

- L 143 ff. The way LOI is explained here is not completely clear. Based on the context, LOI is here defined as the fraction of the total load, i.e. 0-1. However, the authors also write that "The organic component was combusted at 500°C for 1 hour to estimate the LOI of the suspended matter." This sentence implies a mass involved and, thus, units. Please clarify. See also L 291: "Here we use LOI as a measure of the organic fraction of the total suspended solids." Maybe the latter sentence can be moved into the methods section?

- The authors applied the t-test to test the rating coefficients. Are the samples normally distributed and all other requirements met? If not, the t-test is not applicable.

- L 194: "For 52 out of 62 stations, SSC - Q rating curves show a distinct break in scaling relation (for examples see Fig. 3) with similar values for Qb estimated from three different approaches (Tab. 2)." Is there any spatial pattern in terms of signal propagation along nested catchments? This would be an interesting finding.

- L 310: "At stations where the organic fraction of the SSC adds a substantial share to the total SSC,. . ." What is substantial?

- L 207: "This control is highlighted in Fig. 7, which plots bh with respect to the fraction of the contributing catchment area that is steeper than 10% slope gradient. Catchments with a higher fraction of steep slopes are characterized be higher bh -values." While this finding is somehow expected, I was wondering how the authors decided to choose the 10% value? Why didn't the authors consider all percentiles, i.e. involving the entire topography? 10% sounds a bit arbitrary to me.

- L 220: "However, the lower number of measurements at the LOI-stations (approx. 1000 at both stations) resulted in a larger uncertainty of the parameter estimation (Δbl and Δbh) from the bootstrap regression". Can the authors somehow quantify the involved uncertainties?

- L 226: "resulting in rating exponents b of -0.51 ± 0.03 and -0.47 ± 0.01 , and a -coefficients of 0.202 ± 0.003 and 0.319 ± 0.006 for the Rhine and the Moselle, respectively". Please include b here; It makes the reading a lot easier.

- L 261: "The dry continental climate in the Elbe and Oder catchments likely reduces the reactivity of the river systems, requiring larger increases of rain and discharge to increase the specific sediment supply in these basins compared to basins with higher/more frequent precipitation in the western part of Germany." This is a reasonable interpretation. Yet, can the authors provide a reference? Or can the authors estimate catchment-averaged rainfall and relate this to the sediment fluxes observed?

- L 274: "Thus, the transition from bl to bh is likely to be a result of a change of controlling factors of the suspended sediment from intrinsic (within the river system) to extrinsic (outside the river channel but within the catchment) factors." Well, this is just a personal suggestion: I suggest do avoid intrinsic and extrinsic in this case here: It is a hydrological system, though. Given the catchment scale used here, intrinsic suggest within the catchment and extrinsic from outside the catchment. However, I leave this up to the authors and editors.

- L 353: "In the case of substantial share of the organic SSC to the total SSC, our results suggest that the common practice using a continuous sediment rating results in large errors that can be reduced applying rating relationship including scale breaks." Well, does this really matter if organic transport shares are only important during low flows? I would assume that temporal changes in the sediment rating (hysteresis) might be equally important or even more important. In fact, this study shows that larger fraction of organic matter remains unconsidered during low flows only.

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