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

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Dear Authors, The discussion phase of the peer review process has now closed. The reviewers were positive about your manuscript and its contribution to ESurf. Thank you for your replies to the reviewers comments. These are generally thorough and indicate that a revised version of the manuscript can address these comments. However, I have completed my own detailed review, and this has flagged a few additional points to address.

Here, I provide further feedback based on my own reading, and indicate points which perhaps could be further clarified in response to the reviewers.

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Referee 2: Their first comment about temporal changes – I didn't see much included on this point in the revised version. I think it's a fair point to make (indeed there are some discussions about temporal changes in the manuscript). It seems sensible to include some more detail on, over the timescales of sampling, what has (and could have) changed (e.g. land use, flow management, methods of sampling etc.,).

The reviewer asks a question about the filter pore size. Could you please cite the German study you mention, and/or provide a summary (few sentences) of how this was done? Is there any chance the filters being used changed through time for the longer timeseries study?

Their comment regarding L274 – I found this statement quite vague, so I would prefer to remove the jargon and instead explain the processes/factors in more detail.

Otherwise, please address these remaining comments that come from my reading of the original and revised manuscript: (Line numbers refer to the track changes manuscript provided in the response to reviews)

13 – Given the journal audience, and ambiguity of what LOI can be used for, it would be good to specify how the LOI is being used here. e.g. "... (LOI) of suspended matter at two stations along the rivers Moselle and Rhine to provide a proxy of the relative contributions of mineral load and organic matter".

18 – I It was a good idea to add something like this, which summarises the key process-level detail, but I find this new sentence very hard to follow. There is too much jargon, and it is quite vague. I think you are invoking both an increased supply of mineral load (erosion processes), but also a shift in organic matter source, from low mineral associated (i.e. high %LOI) aquatic biomass-derived organic matter at low flow, to mineral-associated organic matter (lower %LOI) eroded from the landscape at higher flow. If I'm correct, please summarise here.

20 – Somewhere in the abstract it would be useful to comment on how the SSC concen-

trations (mg/L) compare to global rivers – this would help to frame the studies findings, and perhaps show in which river systems the clearest comparisons could be found.

53 – to help set this in a wider literature, somewhere in here it would be worth specifying that this framework mostly applies to rivers with generally low turbidity and low suspended sediment concentrations (such as those found here, which are typically « 100 mg/L).

56 – I struggle a bit here – when you talk of low water flow velocities, the process of blooming phytoplankton and its accumulation basically needs zero flow velocity? Else it would be in motion downstream. What about primary producers in biofilms, or aquatic plants? What about leaf litter fall from riparian corridors?

61 – and at high flow runoff and erosion supply materials from outside the channel that swamp the within-river production?

129 – I think this is somewhat unfair given the large body of literature that examines particulate organic matter transport. There are numerous studies that examine POM (or POC) concentrations (% and mg/L), and specially examine it as a function of SSC and/or water discharge in catchments all over the world – New Zealand (Gomez et al., 2003, WRR), Taiwan (e.g. my work- Hilton et al., 2012, GBC), Swiss Alps (Smith et al., 2013, EPSL), USA (Hatten et al., 2010, Biogeochemistry), Peru (Clark et al., 2017, JGR), Guadeloupe (Lloret et al., 2011, Chemical Geology) to name but a few, none of which are referred to in this paper. It is also not just about acknowledging this literature, but also using it to help form broader conclusions. See comments below in the discussion

176 – could you please cite the work (mentioned in the replies that it is a German publication) and provide a brief outline here.

196 – why ‘estimate’. Do you not ‘measure’ LOI?

199 – rephrase? – the whole sample is heated at 500oC, with an aim to combust the

organic fraction of the suspended matter.

200 – to help clarify further “ratio of the mass of organic matter (the mass loss on ignition) to the total suspended sediment mass (ranging from 0 to 1).”

215 – In here, please provide a brief overview of some of the challenges that surround LOI, in terms of different methods (temperatures, combustion lengths) and possibility that the weight loss does not only result from organic matter combustion (i.e. role of clay-bound water, carbonate decomposition etc.,). I think it would be useful to spell out that the LOI is used here as a proxy for the organic matter content – this is what you do, but a sentence stating that would be useful for the ESurf readership.

220 – This needs some more explanation. Were the LOI values used to estimate POC here? If so, please discuss with the caveats above.

410 – Remarkably, this is analogous to results when you examine soil and vegetation derived POC as a function of water discharge in mountain catchments (see Fig. 5 in Hilton et al., 2012, GBC). The reviewer 2 mentions this is to be expected, but I’m not sure too many studies have looked at this. Perhaps the authors can reflect on this - I think this could be worth some more discussion in the paper, with a view to explaining whether this feature should be more widely applicable.

500 – or viewed the other way, a lower dilution of this source (which contributes only a few mg/L) compared to higher flow, when it is swamped by mineral and catchment-derived OC? I’m not sure you can distinguish this explanation from the one given in the text.

540 – ok, but there is not much data to define this decrease on Figure 10.

547 – these ideas are strongly aligned with discussions from other papers on this topic (which are mentioned regarding line 129 above). In particular, Smith et al., 2013 EPSL, in section 5.3 (and check out their figures 2 and 5, for similarities to draw to this work) discussion very similar themes and mechanisms.

There is an opportunity here to draw parallels between these generally low turbidity rivers, and other work on catchments with higher sediment inputs. This could help generalise the findings. This discussion could be included in this section? Generally, this aspect of the discussion is well focused at present. But I wondered if there was an opportunity to take stock of how the process-understanding may make these features more common (or in fact, recognition that they may be specific to certain rivers?)

594 – The final line of the conclusions is not relevant to the findings here. Perhaps rephrase it, instead highlighting that more work is needed to see how generalised these findings could be, or something like that?

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