Reply to the Associate Editor (Robert Hilton) Decision from 28th April.

By Thomas Hoffmann (on behalf of the co-authors) (May 10th, 2020)

Comments to Copernicus and the AE:

Dear Copernicus Team, dear Robert

First of all, I have to apologize for the delayed reply to the decision of the AE. Due to the Corona-Crisis, the closed Kindergarten and the order to work from home, I was not able to reply in time. We hope that the editors of ESurf will still consider our manuscript for publication.

We are very thankful to Robert Hilton for his valuable and constructive comments, which certainly increased the value of the manuscript. We considered every comment, changed the manuscript to implement the suggestions and gave a detailed reply in the following lines (indicated in green letters). We hope that the revised manuscript is not ready for publication.

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.,).

- ⇒ We added a full abstract at the beginning of the discussion reiterating the time scale of this study and make some statements about potential changes and impacts. We state that long-term changes of SSC (as observed at the gauging stations only effects the rating coefficient a, but not the rating exponent b, which is the focus of this study.

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?

- ⇒ After discussions with the lab group and the co-authors we changed the argumentation ranging the pore size of the filters. We avoid to give a clear pore size, since pore sizes are not clearly defined for the coffee filters. However, we argue that a substantial fraction of the suspended clays is not recorded. We added some statements in the method chapter 2.2 and reiterated the uncertainty at the beginning of the discussion, by adding a new paragraph at the beginning of chapter 4.

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.

- ⇒ We rephrased this part to avoid the confusion, which were sated by reviewer II and the AE.

Otherwise, please address these remaining comments that come from my reading of the original and revised manuscript:

(note - 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". → rephrased as suggested

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. → yes, correct. We rephrased this sentence as suggested.

20 – Somewhere in the abstract it would be useful to comment on how the SSC concentrations (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. → we add that the concept refers to large (> 10 000 km2) and low turbid (SSC < 1000 mg/l) rivers, this should help to evaluate the value of this concept in a global context. Certainly, this SSC < 100 mg/l threshold is rather high, but SSC might rise up to several hundred mg/l during floods. Thus, this threshold is an upper boundary.

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). → we added a similar state as in the abstract to the end of the introduction.

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?

- ⇒ We detailed the text in this paragraph. Basically, the background is: If phytoplankton grows at a certain growth rate (depending on light, temperature, nutrients), it can accumulate higher biomasses if it spends more time in a certain river stretch (i.e. at low flow velocities). Vice versa, if flow velocity is high, the growth rate cannot compensate the shorter water residence times and phytoplankton is washed out from the system.
  Primary producers in biofilms play a certain role as they also react on the factors mentioned above, but play a minor role in contributing to suspended load. They are therefore neglected in this discussion. Resuspended biofilm material would of course appear in the data, but its proportion is most probably extremely low as can be seen by lower proportion of organic matter at high discharges.
  Leaf litter contributes to the allochthonous suspended matter mentioned in the first

line of the paragraph, while the rest of the paragraph explicitly deals with autochthonously produced organic matter.

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

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. → We are thankful for this suggestion and fully agree that this literature has been ignored. We added some of the references in the paragraph, which explains the expected relationship between POC and Q in the third paragraph of the introduction. We furthermore, included some of the results of this literature later 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. → We added a refence (Hillebrand et al. 2015) and gave more information about the quality of the measured SSC based on coffee filters. We basically state that we underestimate SSC by approx. 20%, which is in the order of the clay content. We further argue that the clay content is not a function of discharge, and thus no discharge specific effects of the filter method are expected. In the discussion, we further argue that the same rating behavior is evidenced for the two LOI stations in Koblenz, where SSC is measured using standard glass fiber filters. These arguments should give sufficient information regarding the concern of the filter method.

196 – why 'estimate'. Do you not 'measure' LOI? → changed!

199 – rephrase? – the whole sample is heated at 500oC, with an aim to combust the organic fraction of the suspended matter. → done

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)." → done

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. → we added a sentence earlier in this section: "*Here we use the LOI as a proxy for the organic matter content of the suspended sediments, despite the challenges that are related to this method (i.e. different protocol regarding the temperature and combustion length result in various LOIs and combustion may*

originate not only from organic matter but as well from clay-bound-water and carbonate decomposition).“

220 – This needs some more explanation. Were the LOI values used to estimate POC here? If so, please discuss with the caveats above. → we added several lines at the end of this section on how we used the relationship of living biomass (as derived from Chla) and the organic fraction of SSC.

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. 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. → thanks for this hint: we added some sentences to threshold hillslopes here and referred to Hilton et al. (2012)

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. → correct, we rephrased this paragraph to add the aspect on dilution through catchment derived OC.

540 – ok, but there is not much data to define this decrease on Figure 10. → we added a not in the text on the rare observations of this decline.

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. → We related our finding to those of Smith et al and explained differences due to site specific sources and soil conditions.

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?) → at the end of the discussion, we argued that the findings are representative to large river systems with a similar human impact and indicated that more work is needed to test the application to other rives.

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? → we rephrased the last sentence in the line of reasoning, as suggested by the AE.