

Review of hess-2021-89

Anonymous Referee #2

Referee comment on "Low hydrological connectivity after summer drought inhibits DOC export in a forested headwater catchment" by Katharina Blaurock et al., Hydrol. Earth Syst. Sci. Discuss., <https://doi.org/10.5194/hess-2021-89-RC2>, 2021

Blaurock et al. investigated the mobilization of DOC during storm events in two nested, forest catchments in southeast Germany: a 3.5 km² catchment that includes flat and wide riparian areas at lower elevations, and a smaller and steeper 1.1 km² catchment upstream. For that, they analysed a number of metrics and parameters associated with four rainfall events distributed along a ca. two-year period, in which they had high-frequency (15 min) measurements of precipitation, discharge, and DOC concentrations. They conclude that antecedent wetness conditions and topography are major determinants of DOC mobilization.

The topic is definitely interesting and fitted for the audience of Hydrology and Earth System Sciences. The paper is more or less well-written, but at times lack clarity and the reading is not always fluent. I am in general supportive of the interpretations made and of the publication of the paper, but I have many questions, comments, suggestions, and a few concerns that will need to be addressed by the authors before acceptance. Hopefully, these can also help with the presentation issues. Below, I list all my considerations and I look forward to reading the author responses and learn more about this interesting story.

General comments

In general, I very much agree with the interpretations made by the authors, but I wonder whether some of them should be toned down given the low sample size ($N = 4$) and the lack of statistical tests supporting the claims. I appreciate the difficulties of gathering all the appropriate data for a large number of events and the further difficulties to perform meaningful statistical tests with a low sample size, but given that there are statements where parameters are claimed to be higher/lower between the two sites, or being dependent/independent of each other, I wonder whether some statistical analysis can be made to support these claims. What about some simple or multiple linear regressions between parameters or some simple comparison of parameter means between the two sites? I don't imply that any of this should be done, but if not, the authors should justify why no statistical analyses were made and warned the reader that interpretations and

based on the hinted evidence.

I agree with a previous reviewer regarding that seasonality is largely disregarded. Two of the studied events happened in spring and the other two in autumn. DOC concentrations in the soil solution and thus in the stream are likely higher in autumn, as shown for other temperate catchments. Do you have an idea if this is the case in your catchment and what role this phenomenon can play in your results? Even if your DOC mobilization is transport-limited and not source-limited, seasonality should still play a role and it has been barely touched (maybe only slightly in LINE 392-393).

The wordings "antecedent hydrological conditions" and "antecedent wetness conditions" appear mixed in the text and my impression is that they are used interchangeably. I don't think they are analogous terms and in the context of the study I find more appropriate to only use "antecedent wetness", as you are using antecedent precipitation as a proxy for wetness and not for hydrological conditions (precisely because, as you argue in the paper, event size is not a good predictor of discharge).

I think all discharge data presented in the paper should be normalized to catchment area, i.e. presented in units of mm. This would allow comparing discharge more easily between the two sites and with other sites.

I find the parameter "DOC load (kg)" largely irrelevant and would remove it together with all the related results and discussions. I would actually change it to "Area specific DOC load (kg m^{-2})", which is a lot more meaningful.

While the use of sensors has allowed obtaining high-frequency data, the measurements obtained with sensor loggers are not "continuous" but respond to a fixed-interval. Please, correct the few instances where "continuous measurements" were mentioned and simply specify their frequency or that they were highly-frequent.

I would define catchment Markungsgraben as "MG" and catchment Hinterer Schachtenbach as "HS" sooner in the text, and then present them, when possible, always in the same order.

Throughout the manuscript, both the term "watershed" and the term "catchment" are used. I would use only one of the two, preferably "catchment".

Specific comments

Title

The word "Connectivity" is too vague in the context. I would rather say "hydrological connectivity". I am also a bit sceptical about the word "missing". Maybe a better word is simply "low"? Finally, I would emphasize that the mobilization was studied during rainfall events. What about then: "Low hydrological connectivity during summer controls DOC mobilization and export during rainfall events in a small, forested catchment"? Or something similar.

Abstract

LINE 10. DOC needs to be defined.

LINE 11. "hypothesized" instead of "hypothesize".

LINE 11. In which contexts is topography a key driver of DOC export? Please, specify (e.g. in headwater catchments).

LINE 12. I would rather use "hydrological" instead of "hydrologic", or at least only one of the two terms throughout the paper. Now they appear to be mixed.

LINE 12. Maybe you better mean "To test this hypothesis"?

LINE 14-16. I don't think this is the best way to describe where the measurements were done. Discharge and DOC were measured in two stream locations, not in a steep hillslope or a flat riparian zone as the sentence as written now implies. Please, rephrase this part to make clear that the measurements were done in the stream, maybe specifying that at one of the locations the stream drains a steep area, whereas at the other location it drains a bigger area that includes a flat and wide riparian zone at lower elevations.

LINE 17. By "During events" you mean during the four studied events? I think so and if so, please specify it.

LINE 21. This number (522 kg) is largely uninformative without a reference, which in this case I think it should be a normalization to catchment area (see my general comment related to this issue).

LINE 23. Rather than "lack of hydrological connectivity" I would say "low hydrological connectivity", as the stream is still receiving water from the surrounding catchment area. As I understood, there is no evidence suggesting that the stream is completely disconnected from the catchment under dry conditions, losing water towards the riparian zone (right?). But if there is a complete hydrological disconnection, it should be explained.

LINE 27. I wonder whether there is a better word than "parts" in this context. Maybe "locations" or "compartments"?

LINE 28. Similar to the comment on LINE 23, hydrological connectivity will still occur in the future (unless the stream completely disconnects from the catchment, which I assume it is not the case, not even in summer), only that its degree will be lower depending on the conditions. Thus, I would say something like "will be reduced" or something similar.

1 Introduction

LINE 36. Please, move the citation to Drake et al. (2018) to the end of this sentence.

LINE 42. The conclusions drawn by Freeman et al. (2001) were admittedly questionable and I would suggest not to cite this paper.

LINE 43. "influences terrestrial carbon pools". How? By depleting them? Please, specify.

LINE 50. Please, note that a reduction in ionic strength is not an independent process but rather a consequence of a decline in atmospheric acid deposition. Thus, it does not fit in this list.

LINE 48-54. In this context, I would suggest having a look at Clark et al. (2010), who nicely summarized the potential factors behind rising DOC concentrations (which have not really changed since that paper was published) and who importantly highlighted that these factors operate on varying temporal and spatial scales. This might be more relevant to your study, although this topic is in general tangential to what it is investigated.

LINE 62. I would write "which can then be mobilized as DOC", rather than " including DOC, which is easily mobilized".

LINE 72-74. This part of the sentence seems incoherent with respect to the first part of it. Please, rephrase.

LINE 79. Please, remove "itself".

LINE 79. Does "appears" refer to the beginning of the sentence, i.e. to "Hydrological connectivity". If so, please add commas in between "and therefore [...] McDonnel , 2010)".

LINE 82. I would write "DOC" instead of "C".

LINE 89-91. This sentence should be written in past tense, as the hypotheses should define your expectations prior conducting the experiments.

LINE 93. I am still not satisfied with the wording "parts of the catchment". Maybe write "between sub-catchments dominated by either of these two topographical configurations", or something similar.

2 Material and Methods

LINE 105. Maybe it is better to mention here that the Kaltenbrunner Seige sub-catchment was not explicitly studied in this paper. It is also probably better not to mention this catchment again to avoid adding unnecessary unfamiliar names for the reader to keep track.

LINE 123-130. This information can be presented in a more clear and simplified manner. I would just mention that you have one sampling location close to the outlet of the Markungsgraben catchment at an elevation of 888 m a.s.l., and that this location would be referred thereafter as MG. Briefly say that this catchment is steep and refer to Table 1. Then mention that the second sampling location is close to the outlet of the Hinterer Schachtenbach catchment at an elevation of 771 m a.s.l., and that this location would be referred thereafter as HS. Briefly say that this catchment drains flatter areas with wide riparian zones at lower elevations. I would avoid presenting any other information.

LINE 132. At what resolution? Please, specify.

LINE 134-136. What was this done for? What is the aim of this in the context of the study?

LINE 138-141. The three locations where groundwater level data was monitored should be included in the map of Figure 1. As it is described now, it is difficult to know where they were located with respect to the stream measurement locations. For example, what does "uphill" mean? How far from streams were these three groundwater monitoring stations located, and in which type of soil? In any case, the integration of these data into the story of the paper should be improved. As they are presented now, they do not appear very relevant.

LINE 149. What was the resolution of the discharge measurements from the MG site? Given that comparing discharge and exports between the two locations was a major aspect of the study, consideration should be given to the uncertainties associated with the discharge measurements, especially when you have two sources of data with different resolutions. How confident are you that the two discharge time series from the two stream locations can be directly compared?

LINE 155. So, the grab sample values were added to the software in order to update the internal calibration into a so-called "local calibration", right? This is critical, as I wouldn't trust the default calibration.

LINE 160. Any reason why the DOC calibration for MG was not as good as the calibration for HS?

LINE 163-165. It feels like this sentence would fit better in the next section. In any case, this part has to be better presented and justified, as it is the basis of all subsequent analyses. Why these four events? What criteria were followed to select them? How do they compare with other events during the study period? Why no other events were included?

LINE 167. For this first sentence to be compelling, first you would need to describe how baseflow was classified. Thus, I would move the sentence to a later point, after you have described how you define events.

LINE 176. The 15-min resolution values, right? Please, specify it.

3 Results

LINE 196. Please, write "1990-2010" instead of "1990 and 2010".

LINE 197. Do you mean “compared to the long-term average of 1600 mm”?

LINE 198-200. I would start the paragraph with this sentence instead.

LINE 195-200. I wonder how relevant this information and Figure 2 are for the paper. If it is just to put your study period into a long-term context (weather-wise), I would consider removing it, at least the figure. Otherwise, please integrate this part better into the story.

LINE 204-206. This part related to the groundwater tables (including Figure 3) should also be better integrated into the story. In any case, I am a bit puzzled by what I see in Figure 3. To me it appears that, in general, groundwater tables do not really react to any of the studied events. Is there any reason for this? Where are the groundwater monitoring stations located? It seems like soils are very deep there.

LINE 218-221. This part feels like it belongs to the discussion.

LINE 239-240. I don't know what it is meant here. If you want to refer to the baseflow periods immediately prior to the four events, please describe it explicitly.

LINE 242. “without a clear relation”. Did you plot this?

LINE 262. “where concentrations decreased soon after reaching the DOC peak”. I assume this refers to MG, and not to HS nor to what it is written in parenthesis, but the way the sentence is written makes it confusing. Please, rephrase.

LINE 283. It is unlikely, but a good theoretical approximation. I would leave this for the discussion, and here just say that you assume equal area contribution.

LINE 290. I realize that the different panels of Figure 4 are not presented in the natural order (a to f) within the results. Could you please either reorganize/relabel the figures or the text to present them in order?

4 Discussion

LINE 300. But is this driven by P or by AP_{14} ?

LINE 306. Please, rewrite this sentence as it is unclear.

LINE 312. In which way is hydrological connectivity the driver here? Please, make it explicit at this point, or mention that you will explain it in the following paragraph.

LINE 315. The figure number seems to be missing.

LINE 321. Please, change "starts sooner" by "is faster".

LINE 323-324. This needs to be better explained. What kind of "lowlands" and "headwaters" did Zimmer and McGlynn studied and where? Briefly specify it and make the connection to your study.

LINE 332-333. This might be the "expected" range for forested catchments in temperate regions, but it is not the normal range for e.g. boreal, Mediterranean, or tropical sites, so please specify your ecoregion. Also, I would change "expected" by other wording such as "comparable with" or "similar to".

LINE 333-334. "Larger events generally lead to higher DOC concentrations in streams". Are you referring to your study or to other studies? If the latter, please add a reference. If the former, please remind the reader how you showed this.

LINE 336-339. This is an important conclusion, but it is not universal. To make it more broadly relevant, please argue in what contexts might be applicable.

LINE 354-356. Maybe remind the reader that you can make this claim because in this catchment DOC appears to be transport-limited rather than source-limited.

LINE 357-360. I don't know if I agree with the way the transmissivity feedback mechanism is invoked here. The mechanism explains the fast, but deaccelerated increase in groundwater tables due to the saturation of highly conductive shallow soil layers. Thus, at the beginning of an event the increase of groundwater tables would be fast, and then would slow down due to the activation of the highly conductive layers that have a higher lateral water transfer rate. How does the mechanism really connect to your findings? How

deep are your soils and how does the groundwater table behave during events? This is where the groundwater table data can be useful.

LINE 363. Why later during the event?

LINE 366-369. These explanations are critical in the study, but I am not sure I fully understand them in light of the results. Wouldn't this process imply clockwise hysteresis loops instead of anti-clockwise loops. Why is the activation of sources so slow in your catchment? As I understand it, you are implying that there is a relationship between antecedent wetness and type of hysteresis, but from the data presented in Table 2 and Figure 4b, it doesn't look like there is a relationship between wetness and "h index" in the HS catchment. This point needs to be carefully addressed.

LINE 370-374. The contrast with other studies in this sense might be also explained by the fact that DOC is transport-limited rather than source-limited, as you argue.

LINE 410-411. Precisely, as I commented in LINE 366-369, I don't see this pattern in Figure 4b. If I understood it correctly, there might be a weak relationship between catchment wetness and h index for the MG site, but not for the HS site. Is there any type of error in the figure? I might be misunderstanding something, but if the figure and values shown in Table 2 are correct, this part needs to be corrected and some of the discussions you present need to be reconciled with this observation, which is the opposite of what you arguing now.

LINE 417-419. Where and in what type of catchment did Correa et al. (2019) made this observation.

LINE 425. I would end the sentence with "[...] a higher general wetness that favours the build up of DOC in the soil" and would add a reference.

LINE 429-431. I think this sentence is largely irrelevant and I would remove it.

LINE 442. "is proportionally higher". Already taking into account the differences in precipitation between the two locations? Please, specify.

LINE 439-454. The addition of a column in Table 1 with the same information for the entire HS catchment would help interpreting and supporting these explanations.

LINE 463-464. Which in a way is a specific case of the previous explanation, rather than another different reason. Please, reformulate.

5 Conclusions

LINE 477-479. Do you have reasons to suspect that at this stretch of the stream is a net looser of water at any time of the year? Another reason why DOC can be lost is in-stream mineralization. Can this play a role?

Tables and Figures

Table 1. I would remove the column with the information about the entire Grosse Ohe catchment, as it is more distracting than anything else. As I understand, the information presented for the Hinterer Schachtencbach catchment only reflects the local sub-catchment, but I would also like to see the analogous, integrated information for the entire catchment (i.e. for the whole 3.5 km² that include the other two subcatchments).

Figure 5. Please, add HS and MG on top of the left and right panels, as in Figure 6. Please change to "mm/15 min" the units of the precipitation (if that's the case).

Figure 6. Besides the colour code, a small arrow (or a couple of arrows in the lower panels) indicating the direction of the hysteresis loops in each panel would help visualizing and interpreting the results.

Suggested references

Clark, J. M., Bottrell, S. H., Evans, C. D., Monteith, D. T., Bartlett, R., Rose, R., Newton, R. J., and Chapman, P. J.: The importance of the relationship between scale and process in understanding long-term DOC dynamics, *Science of the Total Environment*, 408, 2768-2775, 10.1016/j.scitotenv.2010.02.046, 2010.