We appreciate the detailed and very constructive comments by Referee #2, which will greatly improve the manuscript. In the following, we address the general and the specific comments and explain our intended changes.

Blaurock et al. investigated the mobilization of DOC during storm events in two nested, forest catchments in southeast Germany: a 3.5 km^2 catchment that includes flat and wide riparian areas at lower elevations, and a smaller and steeper 1.1 km^2 catchment upstream. For that, they analyzed 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 were 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.

We aware of the fact that the interpretations are based on a low sample size. We use the figures and regression lines to underline relationships between the investigated parameters. However, we refrain from further statistical analyses as this would not be very reliable nor helpful for further analyses. We will add a sentence about the limitations due to the low sample size in the end of section 2.1.

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).

We agree with both referees that the role of seasonality is an important point. As biological activity is strongly influenced by temperature, DOC production is expected to be higher during the summer months often leading to an increased DOC export. However, this effect seems to

be offset at our study site by the pronounced drought period inhibiting hydrological connectivity. We will discuss this in more detail in the revised manuscript.

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).

We agree that the use of only “antecedent wetness conditions” will prevent misunderstandings and will change the wording accordingly.

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.

We will change the discharge data to normalized data according to the suggestion made by both referees.

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.

We decided to use the unit [kg/hr], which is a unit typically used for load, instead of [kg]. This will help us to put the number in the context of the specific event and to prevent a bias linked to the duration of the events. We will adjust the related results and discussions. The reference to area is already included in the precipitation specific DOC load [kg km^{-2} mm^{-1}].

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.

We will change this according to the reviewer’s suggestion.

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

We will add the explanation of the abbreviations to section 2.1 and check the order throughout the manuscript.

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

We will change the wording as suggested.

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.

We will change the title as follows: „Low hydrological connectivity following summer drought inhibits DOC export in a forested headwater catchment“

Abstract

LINE 10. DOC needs to be defined.

We will add the definition to the Abstract.

LINE 11. “hypothesized” instead of “hypothesize”.

We will change this.

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

We will add “in headwater catchments” as suggested.

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.

We will change the wording as suggested and stick to “hydrological” throughout the manuscript.

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

We will change this.

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.

We agree that the description might be misleading and will rewrite this part to clarify the location of the measurements.

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

We will add “during the events”, which refers to the events mentioned in the sentence before.

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).

As explained above, we prefer to use the reference to a time (the length of the event).

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.

We do not have evidence that there is a complete disconnection and will therefore tone down the statement by using "low hydrological connectivity" as it will then be used in the title.

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

We will use the term "sub-catchment" as we already use it in other parts of the manuscript.

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.

We will change this statement to "when hydrological connectivity will be reduced more often".

1 Introduction

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

As the citation is related to both sentences, we decided to merge the sentences to one.

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

We will remove the citation.

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

We will change the sentence to "Rising DOC concentrations indicate an increased leaching from soils and peatlands and have the potential to deplete the terrestrial carbon pools, which are of global importance for carbon storage".

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.

We will move the reference of Hruška et al (2009) to the other references referring to a decline in atmospheric deposition.

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.

We will add a sentence about the difficulty explained by Clark et al. (2010) concerning the differences in spatial and temporal scales of the studies.

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

We will change this as suggested.

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

We will change the sentence as follows: „Anti-clockwise hysteretic loops usually indicate a delayed arrival of DOC at the stream, which can be caused by the source areas being located further away from the stream (Hood et al., 2006; Vaughan et al., 2017), the sources being connected via flow paths with slow transport velocities (Musolff et al., 2017) or by changes in the dominant flow paths and associated changes in hydrological connectivity (Brown et al., 1999; Hagedorn et al., 2000; Schwarze and Beudert, 2009; Strohmeier et al., 2013; Cerro et al., 2014, Ågren et al., 2008; Pacific et al., 2010)."

LINE 79. Please, remove "itself".

We will 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)".

We will add commas after connectivity and response to mark "and therefore runoff and solute response" as an additional information.

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

We will change this.

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

We will change the tenses used here.

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.

As explained above, we will use the term "sub-catchment".

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.

We will state that the sub-catchment Kaltenbrunner Seige was not studied already in section 2.1. and try to reduce the mention of this sub-catchment to the minimum.

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 Schachtencbach 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.

We agree that some of the information in the text is unnecessary and will therefore follow the suggestion made by the referee.

LINE 132. At what resolution? Please, specify.

All data used for the long-term mean values were measured at a daily resolution. We will add this information.

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

We will change the sentence to: „In order to assess the general meteorological conditions during the sampling period 2018, 2019 and 2020, long-term mean monthly values for the two stations for the period from 1990 to 2010 were calculated." We further explain the characteristics of the sampling periods in section 3.1.

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 where 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.

We will include the locations in the map of Figure 1 and add some additional information about the depth and soil characteristics. We use the groundwater level data to characterize the long-term hydrological conditions of the catchment throughout the sampling periods. As we describe in section 3.1., we can distinguish between two events following dry periods with declining groundwater tables and two events following higher groundwater tables after snowmelt.

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?

For both locations, the resolution of the discharge measurements was 15 minutes. We are therefore sure that the discharge time series can be directly compared. We agree that the description was confusing and will clarify this sentence and add the information about the resolution for MG.

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.

We did not add the values to the software by using a "local calibration" but adjusted the default calibration afterwards by using the values measured in the laboratory as we are aware of the fact that the default calibration is not completely reliable. We will therefore add: "In order to refine the internal calibration, the DOC concentrations measured…".

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

We are not able to explain why the DOC calibration for MG was not as good as for HS. Due to a technical failure, we had to replace the spectrolyser at MG in July. Therefore, the calibration for the event in September 2020 is different with a R^2 of 0.97. This suggests that the mediocre calibration is linked to the specific device. We will add the information about the different calibrations at MG to section 2.2.4

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?

As the sampling period was very dry, not many events were available for analysis. Only some small events and very few large events could be observed. Small events led to small discharge and/or DOC responses or no responses at all, which would make the analysis of hysteresis patterns difficult, for instance. We decided to focus on the largest events in order to be able to analyze DOC responses in the stream in detail. We will add a sentence about the reasons for selecting the four events presented in the manuscript in order clarify this.

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.

We will restructure this paragraph as suggested.

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

Yes, we will add the information about the resolution.

3 Results

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

We will change this.

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

Yes, we will change this accordingly.

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

We will change this as suggested.

LINE 195-200. I wonder how relevant this information and Figure 2 are for the paper. If it is just to put you 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.

We think that the information is relevant to put the study period into a long-term context, as the study period was particularly dry. However, we agree that the figure is not necessary as the important information is given in the text. We will therefore remove the figure.

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 station located? It seems like soils are very deep there.

The data is representing the deep groundwater. As it can be seen in Figure 3, the groundwater level varies between 2 and 16 meters below ground. In our opinion, it is therefore not surprising that we observe seasonal variations only instead of a response to events. As explained above, we will add the location of the groundwater wells to Figure 1 and add some additional information.

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

We think that this information is useful at this part of the manuscript to explain the reasons for studying the selected events and to highlight the differences between them.

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

As this information is not really relevant for the study, we decided to remove this sentence.

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

We plotted the relation but decided not to include it in the manuscript in order to focus on other points. However, we will add a reference to Table 2, where all values are presented.

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.

We will change the sentence to: “This resulted in wider hysteretic loops at HS than at MG (larger absolute values of h) as the concentrations at MG decreased soon after reaching the DOC peak. “

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.

We prefer to leave the sentence as it is. However, as suggested by Referee #1, we will add some more detailed information about how we derived this value.

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?

We agree and will reorganize this section.

4 Discussion

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

As explained in the following sentence, precipitation alone is not the main driver of Q generation but the antecedent hydrological conditions are of importance.

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

We will change the sentence to "To some extent, this observation can be attributed to the larger catchment area contributing to water fluxes at HS, resulting in longer flow pathways and a delayed Q response." and add two references to back up this statement.

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.

We will change this paragraph as follows: "We suggest that hydrological connectivity between the wide riparian zone and the stream is the major driver for delivering water to the stream. The hydrological connectivity is dependent both on topography as on the antecedent conditions as we will explain in the following."

LINE 315. The figure number seems to be missing.

We will add the missing figure number.

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

We will change this as suggested.

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.

We think that the sentence does not fit well here in general. We will remove it and discuss the results by Zimmer and McGlynn in more detail in section 4.2.

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".

We will change the sentence as follows: "Concentrations were similar to values found in other temperate forested catchments in low mountain ranges."

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.

We will add a reference to back up this statement.

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.

We agree that we can discuss the broader context in more detail. However, we prefer to do this in the Conclusions section, where we discuss possible implications of climate change for the relative contribution of different sub-catchments. There, we will add a sentence regarding the relevance of transport limitation in the context of climate change.

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.

We will add the suggestion as follows: "As DOC appears to be transport-limited rather than source-limited, the persistently high concentrations, in combination with a high discharge generation due to the existing hydrological connectivity, could then cause the pronounced DOC export during events following wet antecedent conditions."

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.

We do not think that the groundwater table data shown here can be used to investigate the transmissivity feedback, as this is a process occurring in the upper soil layer. The groundwater data shown here, however, refers to the deep groundwater level representing slow changes of the groundwater table. However, we also installed piezometers in the shallow groundwater later during the sampling campaign. There we do see the relationship between the groundwater table and discharge as explained by the Referee. We do not include this data as it cannot be linked to the selected events of this study due to different sampling periods and will be part of a different manuscript. Nevertheless, we think that the transmissivity feedback can be of importance in the riparian zone of the lower part of the catchment because there we do see shallow groundwater tables, which can quickly rise into the upper soil layers.

LINE 363. Why later during the event?

During dry periods, those pools are empty and start filling only with the beginning of the precipitation event. They connect to the stream once a certain water level is reached and therefore contribute to discharge later during the event. We will therefore change the sentence as follows: “The possibility that these pools contribute to DOC export when filled with water later during the event is…”.

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.

Although we compare only four data points, we do think that a relation is visible in Figure 4b. We observe smaller hysteresis loops (h closer to zero) during wet conditions than during dry conditions. The event in October, following the dry summer, shows the broadest loop, the event in June the smallest. It is not unusual to only find anti-clockwise hystereses when comparing our results to other studies. Anti-clockwise hysteresis loops are caused by first having to activate the most potent DOC sources in the shallow soils by bringing the water table up to hydrologically connect them with the stream during the rising limb of the event hydrograph and during the falling limb the DOC rich upper soil layers are still draining while the discharge recedes. The activation of sources is generally slow in our catchment but seems to be accelerating if a certain hydrological connectivity is present. We will add an explaining sentence at the end of section 3.2.

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.

We do not think that the transport-limitation in our catchment is in contrast to other studies as many catchments are transport limited. To further stress the importance of transport limitation, we will change the sentence as follows: “If the soils are wet prior to an event, connected flow paths can quickly be established and DOC transport to the stream occurs faster than during dry conditions, which highlights that DOC export is transport limited in this catchment.“

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.

As described above, we do think that there is a (admittedly not very strong) relation between catchment wetness and h index. However, we agree that the paragraph needs to be restructured in order to clarify our arguments. We will rewrite it accordingly.

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

We will add this information: “Correa et al. (2019) made a similar observation in a tropical alpine headwater catchment with anti-clockwise hysteresis...”

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.

We will change the sentence to: „According to these studies, a flat area would therefore tend to export more DOC than a steep area due to a higher general wetness that favors the build-up of DOC in the soil and hydrological connectivity."

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

We will remove the sentence as suggested.

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

We will change the sentence as follows: "The proportional amount of Q arriving from the upper catchment is higher than during the other events; however, the DOC proportion is even higher."

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.

We agree that the information would be useful and will add the column in Table 1 but remove the information about the entire Große Ohe catchment as suggested further down.

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

We will rephrase the sentence as follows: "A consequence of the reduced connectivity of the lower catchment could be that the riparian pools mentioned above are not connected and thus an important DOC source is not active."

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?

We also investigated possible groundwater gains or losses along the stream using tracer experiments and radon data. The data indicate that exchange with groundwater is especially important in the lower part of the catchment. However, we decided not to include this data as it is beyond the scope of this study. Nevertheless, we will discuss the possibilities of groundwater recharge and contributions in more detail in section 4.1. as suggested by Referee 1. We do not think that in-stream mineralization is if importance as the processes investigated happen at the event-scale and are therefore rather fast in contrast to mineralization processes.

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 km^2 that include the other two subcatchments).

As explained above, we will add the column in Table 1 but remove the information about the entire Große Ohe catchment as suggested further down.

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).

We will add HS and MG as suggested and add the units of precipitation (mm/hr).

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.

We will add arrows as suggested.

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.

Katharina Blaurock

On behalf of all co-authors