Authors' response to Comments of Reviewer 1

Review of hess-2024-250

Title: Hydrological Controls on Temporal Contributions of Three Nested Forested Subcatchments to DOC Export, by Blaurock et al.

Blaurock et al. present high frequency DOC and discharge data for a period of one year from three nested forest headwater subcatchments in the Bavarian Forest National Park (Germany). They aim to explore differences in DOC export between the three subcatchments, which have distinct vegetation, microclimate, soil types, and topographical characteristics, among different "hydrological periods" namely spring, summer, autumn, winter, and snowmelt. Precipitation inputs drive overall exports whereas differences in runoff contribution and hydrological connectivity between the different catchments drive seasonal differences in DOC exports between the subcatchments.

High-frequency sensors and increasingly used to better understand biogeochemical mechanisms and mobilization processes at the catchment scale, particularly in relation to the important constituent DOC. This study provides further insights into the topic and should be of general interest for the readers of *Hydrology and Earth System Sciences*. I do have a number of concerns and suggestions and a list of other relatively minor comments that the authors should address before the manuscript can be accepted for publication.

We thank the reviewer for their very constructive comments and the overall positive assessment of our study. Below we provide our responses to the reviewer's comments in italic font.

General comments

The specific comments I provide below are extensive enough for the authors to consider the revision of their manuscript. Here I summarize my main points of criticism:

1. The definition of "hydrological periods" needs a more rigorous explanation, including e.g. information on how dry or rainy periods are determined.

   Please see our reply to your comment "L 136-150". We agree that the definition of the hydrological periods needs to be explained in more detail.

2. I miss a more compelling explanation/interpretation for the higher runoff generation at MG.

   Please see our reply to your comment "L 351-362". We appreciate your constructive comments about this point.

3. Likewise, I am not convinced about the explanations given for the low runoff generation and high flow-weighted DOC concentrations across all conditions observed at HSsub. The authors repeatedly mention that hydrological connectivity needs to be established at this site, but it appears that even during low hydrological connectivity HSsub provides water with high DOC concentrations. Where does it come from?

   Please see our reply to your comment "L 375-380". We will expand the development of hypotheses that could explain the relatively lower runoff generation of HS_{sub}.

Specific comments

Abstract

12. Maybe "soil type" instead of "soils"?

We will replace "soils" by "soil types".

12-14. In the abstract, you associate limited hydrological connectivity with snowmelt, summer, and winter. In general, snowmelt periods are associated with high hydrological connectivity instead, and I think it is similar in your study. Moreover, you do associate autumn with limited hydrological connectivity later on in the text (at least for subcatchments HSsub), so this part of the abstract is somewhat inconsistent with your interpretations.

Indeed, the wording in the abstract is inconsistent with our main text. We will rephrase the respective sentences in the abstract.

1 Introduction

33. More explicitly, "because of in-stream metabolic processes".

This will be added to the text.

35. I would say "In addition" rather than "But".

We will correct this as suggested.

44. The high groundwater levels also favour the build-up itself of organic matter in the soil (because of limited mineralization due to hypoxic conditions).

We will add this point to the line of reasoning.

47. I would say concentrations "generally" increase.

We will add "generally" to the sentence, as suggested.

2 Material and Methods

72. Do you perhaps mean "2.1 Study site"?

Yes, thank you for pointing this out.

75 – Figure 1. If I understand correctly, the study is based on data only from the Hinterer Schachtenbach catchment (delineated in red in the figure), which is part of the bigger Grosse Ohe catchment, which I understand is also depicted in the figure outside the delineated area. I would suggest to only present the Schachtenbach catchment in the figure, as the rest of the illustration is more distracting than informative.

We will follow the suggestion of the reviewer and will show only the catchment Hinterer Schachtenbach with its subcatchments in the map in Fig. 1.

Kommentiert [KB1]: Ich denke, das reicht als Antwort, oder? Ich schaffe es mit QGiS leider nicht, aber kann es noch mit einem Bildbearbeitungsprogramm hinbekommen, denke ich.

Kommentiert [KB2]: Ich denke, das reicht als Antwort, oder? Ich habe es leider bisher nicht mit Qgis geschafft wegen Probeleme mit dem Koordinatensystem, aber notfalls schaffe ich es sicher mit einem Bildbearbeitungsprogramm und muss es halt manuell ausschneiden.

90. Are these rock outcrops or exposed bedrock, or how are these rocks "interspersed" in the soils?

The rocks (or small boulders) are mostly located within the soil profile. Some of them also intersect the soil surface and peak out. We will change the sentence to: "Almost 40 % of the area of MG is characterized by soils that are interspersed with rocks, mostly below the surface."

91-95. Just out of curiosity: in the period 2018-2021 there was a drought followed by a large infestation of Norway spruce by bark beetles in large parts of Central Europe. Was the forest in your study are not affected by this disturbance?

The forest in our study area (the "Rachel-Lusen area" within the Bavarian Forest National Park) was affected by large barkbeetle calamities in the 1990s and early 2000s. Since then, a stable and diverse forest has resulted from rejuvenation that can resist barkbeetle infestations better. However, other areas in the Bavarian Forest National Park have been hit by barkbeetle infestations in the past years.

104-111. The gap filling at HS and Q construction at KS are fine. However, I am confused about the range of values shown in Figure S1 and Figure S2 compared to the range of values that you present here and that I could see in your raw data in the Figshare file. Specifically, the upper values are much larger in Figures S1 and S2 compared to the upper values shown in the study for all three subcatchments (e.g. the highest Q at HS according to the data here is 0.75 m3/s whereas it appears to be as high as ca. 3 m3/s in Figure S1 for an antecedent period). Could you clarify this point?

The datasets used for gapfilling comprise data from before the study period that is presented in the manuscript. In Section 2.2.1 we wrote: "This data gap was filled using the discharge data of a neighboring catchment within the Große Ohe Catchment of similar size (Vorderer Schachtenbach, 5.9 km^2). Using the discharge values of one year prior to the data gap, a relationship between the discharge of the two catchments was established (R2=0.94) and calculated values were used to fill the data gap (Figure S1)." Also the tracer dilution experiments for discharge calculation in the subcatchment KS span a period that was not part of our study period (April-Dec 2021).

Figure S1 includes data from the snowmelt period of the year 2020, when a large snowmelt event led to discharge values as high as ca. 3 m^3/s in the beginning of February. This period is not presented in the manuscript, where we show the period June 2020 – May 2021. We will add the period from which data were taken for gapfilling to the figure caption in the SI.

120-125. Thank you for this detail explanation. Just out of curiosity: the fact that you move D3 from KS to MG following the failure of D2 makes me think that you prioritized having data from MG compared to having data from KS. Is this correct and if so, why?

Yes, that is correct. We had been working in the MG subcatchment since 2018, and our goal was to continue the data collection there to add to our multi-year dataset of MG. The measurements in the subcatchment KS started only with this study.

125-135. Nice!

Thank you!

138-139. However, you have an additional period which you define as "snowmelt", which in fact is the longest of all.

We will add following sentence: "In addition, we also introduced the hydrological period "snowmelt" to account for the very different hydrological conditions with increased runoff generation." (p. 6, L 144-145).

139. Perhaps "large extent" instead of "larger extent".

We will correct this as suggested.

136-150. I think this classification is fine, but I wonder whether more details can be provided so it appears less arbitrary. For example, you mention "starts" or "ends" of "dry" or "rainy" periods to delimitate your hydrological periods, but no information on how you define a dry or rainy period is given or in reference to what. Also, the snowmelt period appears to be excessively long (Feb to Apr 2021). Did snow cover take that long to melt?

We defined rainy and dry periods based on clear changes in precipitation regime and discharge response. For this, mean daily precipitation for preceding 14 day intervals, frequency of events and changes in discharge (lag times and peak values), together with visual inspection of the time series of precipitation and discharge, were considered. Admittedly, we did not perform statistical analyses but based our definition of hydrological periods on this more qualitative approach that was also supported by our combined experience and knowledge of the hydrologic response at the study site.

Due to the elevation difference between catchment outlet and catchment ridge (771-1373 m), the catchment is not snowfree until April or even early May. In 2021, there was a snow cover of 10 cm even at the lowest outlet of the catchment until the beginning of April. We do not have snow cover data for the upper part of the subcatchments but for adjacent catchments, where there was a snow cover of 15 cm present at the elevation of ca. 1300 m until the end of April. Snowmelt is also clearly visible as diurnal fluctuations in the discharge data.

In the revised version of the manuscript, we will add more detail to section 2.2.2 so that our definition of hydrological periods becomes clearer.

168-170. Fair assumptions but what do you know about the in-stream processing of DOC in your system? I would be inclined to think that it is probably limited, but there is increasing evidence in the literature that in-stream DOC processing might be larger than previously thought, even in low order streams with non-labile DOC. What is the chemical character of the DOM? You can probably have a proxy for this with the absorbance data.

The extent of in-stream processing of land-derived DOC is still not fully understood and a field of active research to better understand the export of terrestrial carbon into streams and its metabolic fate in aquatic systems. There are studies that indicate that the relevance of in-stream metabolic processes influencing stream DOC concentrations is limited (e.g., Singh et al. 2015 (https://doi.org/10.1002/hyp.10286); Bernal et al. 2019 (https://doi.org/10.3389/fenvs.2019.00060); Dawson et al., 2001 (https://doi.org/10.1016/S0048-9697(00)00656-2), at least in headwater streams where residence times of stream water are expected to be short. Very few studies have compared in-stream metabolic processing of DOC between periods of baseflow and event runoff, and it is not clear yet if runoff events stimulate or lower in-stream processing of DOC (e.g., Bernal et al 2019 (https://doi.org/10.3389/fenvs.2019.00060) and Demars 2018 (https://doi.org/10.1002/lno.11048).

DOM quality has been used to infer in-stream processing of terrestrial DOC. Kothawala et al. (2015, https://doi.org/10.1002/2015JG002946) found no indication for in-stream transformations to soil DOM composition, using absorbance metrics, in boreal headwater catchments. A companion study to our research in the Hinterer Schachtenbach catchment investigated DOM composition of stream DOC and soil DOC sources and found: "The analyses revealed that at comparatively high dissolved organic carbon concentrations, the composition of DOM in-stream reflects the composition of DOM stored in the superficial soil layers", from which we concluded that there was no indication for alteration of DOC in the stream, i.e., no in-stream metabolic processing (da Silva et al. 2021 (https://doi.org/10.1029/2021JG006425)).

We will add the two latter references to this sentence to support our assumption that neglecting effects of in-stream metabolic processes on stream DOC concentrations are appropriate.

3 Results

177-178. The "leading to sharply rising DOC…" implies that precipitation events are responsible for the increase. This is of course true, but via hydrological activation of upper soil layers that have build-up DOC during summer. Anyway, these explanations belong to the discussion so I guess what I am trying to say here is that you could avoid using terms like "leading" and simply describe the observed patterns without further implications on the processes.

We agree with this comment and will change the sentence accordingly to: "In autumn, several large precipitation events occurred, and sharply rising DOC concentrations up to 20 mg/L were observed (Fig. 2)."

191. Typically, DOC export is reported in kg/ha or g/m2. Please, transform the 3931 kg/km2 into either of these other units for better comparison with other studies.

This will be done.

210. Do you mean "DOC export" instead of "DOC concentration"?

Yes, thanks for pointing this out.

210-218. To me, the interesting thing about this kind of figure is to relatively compare the evolution of cumulative discharge and cumulative DOC export, with focus on when the lines deviate. For example, after both discharge and DOC evolved comparatively, DOC disproportionally increases at some point in mid-autumn, but this disproportionate increase is cancelled out during winter (with the exception of the mid-winter snowmelt event). Then again at the beginning of the snowmelt period DOC disproportionally increase relative to discharge, but as the snowmelt period advances, DOC decreases relative to the discharge, suggesting some kind of dilution effect or even production-limitation taking place then. You have some explanations in the discussion around this figure, but I would suggest to make these points more explicit, here in the results when you describe the figure, and later in the discussion.

Thank you for this suggestion. We will slightly reword this paragraph and add the following sentence: "The deviation between the two cumulative curves indicated that DOC export increased disproportionately relative to discharge during these periods. In contrast, towards the end of the snowmelt period, DOC export decreased relative to discharge."

229 – Figure 5. Again, I would rather present DOC exports in kg/ha or g/m2.

We will insert revised versions of Figure 5 and Figure 6a, showing DOC export in kg/ha.

242. But KS contributed less than expected according to its area ratio, right?

In Figure 6c the relative contribution of the subcatchments to DOC export is shown and compared with the contribution that could be expected due to their area ratio. In the winter period, KS contributed more to DOC export than expected by area ratio (yellow bar and yellow line) whereas HS(sub) contributed less than expected (black bar and black line).

4 Discussion

268. See my previous suggestions regarding the units of annual DOC export.

We will correct the unit to kg ha^{-1}.

312-313. I don't think I can agree here. The discharge time series show that flow is low towards the end of snowmelt, and therefore I can hardly imagine soils being saturated at this point. Also, according to Figure 4, DOC decreases with respect to discharge during this period, and it is only the activation of DOC source areas during spring rainfall events that can explain this pattern. Note as well that the way you define the different periods is very much influencing your findings in terms of DOC because you are using main DOC drivers in the definitions.

We assume that after the end of snowmelt (the cited sentences refer to the beginning of the spring period) the soils are very wet and especially in the flat areas of the HS(sub) riparian zone saturated/close to saturation. Admittedly, the expression "high continuous DOC export" is not correct, as DOC export had decreased relative to discharge towards the end of the snowmelt period and picked up again during the first few weeks of the spring period. The sentence will be changed as follows: "Soils were saturated after the snowmelt period and, therefore, hydrological connectivity between the DOC sources and the stream existed facilitating the connection of distal DOC sources (Croghan et al., 2023) and an increased DOC export (Fig. 4) during the spring events."

343-345. Also, potentially lower evapotranspiration as the deciduous trees drastically reduce transpiration during winter.

Thank you for pointing this out. The sentence will read: "Additionally, litter fall (a) induces a lower water loss via canopy interception in autumn and (b) leads to strongly reduced transpiration rates. Both factors would result in shifts in the water balance, leading to an increased availability of water in the soil profile and active flow pathways, as suggested by the high runoff ratio at KS during winter (Table 4)."

345. Rather than "after the snowmelt period", it could be better to write "during spring" "or from spring on".

We will change the expression as suggested.

351-362. To me, one of the most striking results in your study is the significantly higher runoff contribution of MG compared to the other two subcatchments, especially compared to KS which a priori are more comparable. In this part of the text you provide hypotheses that aim to explain this observation, but I am not convinced that they can fully account for it. What about the role of vegetation? After all, evapotranspiration is a main component of the water balance. I can see that MG has the highest percentage of forest in a state of rejuvenation. Could it be that the overall evapotranspiration in this subcatchments is relatively lower than at KS because the forest is not fully developed and this contributes to the higher runoff contribution from MG?

We will add this consideration of vegetation differences to this paragraph. We agree that this factor could also contribute to an increased runoff generation as it shifts the water balance. Ranking the hypotheses responsible for the increased runoff from MG, however, remains difficult. We would argue that the combination of the factors mentioned leads to the markedly higher runoff generation in MG as compared to KS. We will add following sentence: "The subcatchment MG also has the highest area percentage in the state of forest rejuvenation. It is therefore likely that due to these differences in extent of mature trees, the transpiration component in the water balance of MG is markedly lower compared to KS, resulting in an increased availability of water for runoff generation."

375-380. But the higher flow-weighted DOC export at HSsub occurs across all periods and not only during high wetness. And, as you mention before in the text, autumn is a particularly limited period in terms of hydrologic connectivity between HSsub catchment soils and the stream, which "inhibit DOC mobilization". Therefore, I remain puzzled as to why HSsub (i) is so inefficient at generating stream runoff, especially during autumn (the runoff ratio of 0.13 is strikingly low), and (ii) can still provide water with high DOC concentration so that flow-weighted exports are high across all conditions. I think these points are the most critical to revise in a new version of the manuscript as they are also the most interesting in terms of catchment process understanding.

We argue that the high flow-weighted DOC export in the subcatchment HSsub is the result of the interplay between hydrological processes and DOC production and accumulation. After the warm and dry summer with low runoff generation in the Hinterer Schachtenbach catchment, the soils in the flat riparian zone around the stream in HSsub had dried out, as did the microtopographic features ("ponds") in the riparian zone. The few events in August led to only small Q increases but still high DOC concentrations (resulting in a low DOC load). The month of September was particularly dry. Figure 2 in Blaurock et al. 2022 (https://doi.org/10.1029/2022JG006831) shows water levels below ground surface for three piezometers installed in the riparian zone of HSsub (one of them being installed in a pond) for summer and beginning of autumn 2020, thereby overlapping with the study period presented here. During July, August and September 2020, water levels in the soils did rise following precipitation events but also quickly receded again. This points to a quick drying-out of the soils in the riparian zone of HSsub and lost hydrologic connectivity. Particularly low water levels were reached at the beginning of August and at the end of September. The first autumn events led to a more substantial wet-up of the soils and produced strong DOC concentration increases in the stream (see Fig. 2). However, Q remained still low. The highest value of water level in P1 (again Fig. 2 from Blaurock et al. 2022) corresponds to spill-over of the pond; this is the occasion when the ponds are fully filled, flow over and connect with each other and the stream. This spill-over was reached only once during June to end of October 2020, and even though the pond remained filled during most of October (water level > 0 cm), it did not spill again. This indicates that wide-spread hydrological connectivity was not reached yet, therefore runoff generation in HSsub remained still low. Towards the end of the autumn period, in November 2020, the baseflow level of Q rose, suggesting that runoff generation had picked up. Unfortunately, we stopped our piezometer measurements at the end of October 2020.

This dependence of the establishment of hydrologic connectivity on the flat topography in the riparian zone of HSsub is paired with the high accumulation of DOC in the soils with often higher groundwater tables (which limits mineralization of DOC) and the presence of the microtopographic features, the ponds. Our analyses presented in Blaurock et al. 2022 showed that upon spill-over and connection of the ponds the DOC signature of the pond DOC can be found in stream DOC during event flow. At the same time, the pond DOC concentrations can rise up to values of 100 mg/L (unpublished data, observed during a field hydrology course at the same site in 2023), which points to a very strong DOC pool being present in these ponds in the riparian zone of HSsub.

In a revised version of the manuscript, we will explain in a little more detail the role of the ponds as DOC accumulation and release hot spots and also refer to the Figure 2 in the Blaurock et al. 2022 study in chapter 4.2.

382. In Figure S6, how is it that the mean of daily flow-weighted DOC export of "all" periods is lower than each of the periods for all three subcatchments? Are you using the summer period to calculate the mean in "all"? Even if you do, I find it difficult to arrive to those values. I would expect to see something similar in relative terms to what is shown in Figure 6b (in fact, the values should be proportional so the relative differences should be the same).

The flow-weighted values for "all" are in a similar range as for the individual hydrological seasons (because they refer to a larger amount of Q). When this value is divided by all days (entire study period), the resulting value becomes small. However, after evaluating the "all" columns" again, we decided to remove them in a revised version of this figure (and Figure S6). We realized that we do not refer to the "all" values in the text because the focus is on comparing the hydrological periods. Therefore, the "all" columns are confusing. Thank you for pointing this out.

5 Conclusions

425-431. Droughts could also lead to bark beetle infestation and death of trees, with important hydrological and biogeochemical consequences at the catchment scale.

We agree that droughts can lead to the death of trees, e.g., by facilitating bark beetle infestations and/or windthrow. This would affect the partitioning of water within the catchment water balance and provide a disproportionately high carbon pool for decomposition within a short period. In a revised version of the manuscript we will incorporate this thought into the text of the conclusion section.