

Dear editor,

we are pleased to read that you and the referees think that the manuscript has improved as a result of the revisions. We appreciate the additional comments of both referees during this second review. In the following, we will address all comments and explain which changes were made. In addition to the suggestions made by the referees, we made some minor changes, which we explain following the referees' comments. We refer to the line numbers of the marked up manuscript.

## Referee #1 (Report #2)

### Summary

I reviewed a previous version of this manuscript and have read through the response to reviewer document, track changes document, and the newly revised manuscript. The authors did a thorough job responding to the two referees, who had some overlapping comments and suggestions. These revisions greatly improved the quality and clarity of the manuscript. The authors addressed concerns about sample size, explanatory variables, and other clarifying requests. While I don't necessarily agree with all of the authors' final interpretations (e.g. season is not a critically important control on DOC export), I find that the authors rationalize their findings and interpretations in a convincing way and support their claims well with an up-to-date literature review. Overall, I believe the manuscript is suitable for acceptance at HESS pending some minor revisions, as described below.

### Minor

I find the manuscript to be convoluted since the authors are trying to discuss a range of explanatory variables for two catchments over four events. I find a manuscript to be most impactful when it is easily accessible to readers, but as the paper is currently presented there is a lot left for the reader to untangle. Thus, one strong suggestion I have for the authors is to include a conceptual figure at the end of the manuscript that visually summarizes the main key points in terms of controls on DOC export.

We created a conceptual figure (Fig. 7), which summarizes the link between antecedent wetness conditions, event size, topography and DOC export. We refer to this figure in section 4.2. (L. 426) and section 4.3. (L. 493).

I appreciate the changes to the title of the manuscript to try to clarify the message. However, I find the current title still doesn't capture the main objective of the paper. That is because not all of the events occurred during summer, yet this title suggests the study is focused in summer. I would recommend clarifying the title to reflect the range of event timing that was included in this study.

We do understand the referee's wish to present the main objective of the paper in the title. However, our reason for choosing this title is the goal to highlight the main finding of the paper. Therefore, we prefer to leave the title generally as it is. However, we decided to exchange "during" with "after" as the observed events end the summer drought.

The revised abstract reads very well and the minor changes to describe the two catchments helped clarify the site specifics.

L 39 – need comma after "5.1 Pg C"

We added the comma.

L 57 – missing period at the end of the sentence

We added a period.

Figure 1 – It is still unclear how the authors used the 5 m DEM to identify stream channels. Did they use a contributing area threshold, or base it topographic proxies, or something else? Our understanding of the stream network is important for understanding DOC export behavior as it can help us understand the aquatic connection to the terrestrial landscape, thus I think it is important to be clear.

The stream channels used in Figure 1 are based on the official shape files provided by the Bavarian State Office for Environment, which had identified the stream channels using topographic proxies and calculating contributing areas. We added this information to the caption of Figure 1.

L 218 – change Figure 23 to correct Figure reference (Figure 2).

We changed the Figure references.

L 253 – I would not classify event-scale measurements as “long-term”

In this sentence, we do not refer to the event-scale measurements but intend to present the DOC concentrations found during baseflow, which we measured during our long-term measurements. Therefore, we decided to leave the sentence as it is.

L 313- The events were also characterized by contrasting seasonality.

In section 4.1., we do not yet discuss processes involving DOC. Therefore, we mention the antecedent wetness conditions only rather than discussing seasonality at this point as well as this would include other factors (e.g. temperature), which indeed do influence DOC production but not the processes involving discharge only. We do discuss seasonality in section 4.2., where we focus on DOC mobilization processes.

L 412 – Wouldn't an event in May have a different seasonal effect than an event in September in terms of biological activity, not just wetness conditions? May is following a wetter dormant season and September is following a dry growing season. One would expect this seasonal difference to influence DOC availability. While the authors mention 'warm summer months' in the paragraph, it is buried in the paragraph and should be included in the topic sentence at least.

We added the information about the contrasting seasonality more prominently.

L 480 – add period after “conditions”

We added a period.

Conclusion - I am still not fully convinced that season is not an important contributor to DOC export differences seen across events – perhaps the authors could add one sentence in the conclusions that state why event size, antecedent wetness, and topography are important, but season is not.

We included a sentence about the effect of seasonality in the conclusions (L520).

## Referee #2 (Report #1)

Blaurock et al. sent a revised version of their previously submitted manuscript, together with their replies to reviewer comments. In this study, they investigated the mobilization of DOC during storm events in two topographically contrasting forest catchments and concluded that event size, antecedent wetness conditions, and topography are major determinants of DOC mobilization and export.

I carefully read the response letter and the revised manuscript, which I enjoyed. I thank the authors for addressing all my comments and making the pertinent changes in the manuscript or explaining why they didn't in those cases where they disagreed with my points. For the most part, I am satisfied with the responses and the changes made in the manuscript, which I believe have helped increasing its clarity. I do however have a few further comments that follow up on a few of the discussed topics and that I would like to see considered/clarified before a final version of the manuscript can be accepted.

I understand now that the groundwater tables plotted in Figure 2 are from three deep wells and that they are used as a proxy for some kind of overall wetness conditions in the catchment that follow seasonal patterns rather than event-dynamics. However, the data from these deep wells should not be confused with groundwater table variations occurring in the shallow soil, which do show small-temporal scale dynamics responding to events and which you also discuss at times (e.g. L. 323-327 and L. 335-339). The distinction between these two types of groundwater tables has to be more clearly made, for example by explicitly differentiating between deep groundwater tables (with seasonal dynamics) versus shallow groundwater tables (which responds to precipitation inputs and relate to water and solute delivery during events). The way groundwater tables are presented now leads to confusion.

We now write "deep groundwater" in section 3.1., where we use this data as a proxy for the wetness conditions. Moreover, we specify this in the title of section 2.2.2., where we explain, which data was used as "deep groundwater". In section 4.1., we talk about groundwater in a more general way or specify "shallow groundwater" when necessary.

Additionally, I wonder if a further distinction can be made by describing the water storage structure in the catchment. The way you describe things makes me think that the catchment has a perched shallow aquifer that overlies a saturated deep aquifer with varying degrees of connectivity to the stream. Is this the case? If so, please specify it and link it to the deep versus shallow groundwater table distinction.

As we have only limited information on shallow groundwater levels, we cannot discuss processes involving deep and shallow groundwater in detail. In our opinion, we do lack the data to conclude that the catchment shows a perched shallow groundwater later and overlies a deep aquifer as you suggest, although this definitely could be a possibility. However, the deep groundwater wells are also located at different locations than the other sampling site and it is therefore difficult to compare the data.

While I tend to believe that in-stream DOC mineralization might not be significant during rainfall events that produce high water velocities and lead to short water residence times, this paradigm might not be as universal as previously thought as there are recent studies suggesting the opposite might be true in certain settings (e.g. Bernal, S., Lupon, A., Wollheim, W. M., Sabater, F., Poblador, S., & Martí, E. (2019). Supply, Demand, and In-Stream Retention of Dissolved Organic Carbon and Nitrate During Storms in Mediterranean Forested Headwater Streams. *Frontiers in Environmental Science*, 7, 14. doi:10.3389/fenvs.2019.00060). Therefore, consider this information and back up your statement in L. 499-500 with some references.

We added additional information about in-stream mineralization to the conclusion including references.

L. 91. Do you mean "hydrological conditions" here, or rather "(antecedent) wetness conditions"?

We changed this to "antecedent wetness conditions".

L. 101-102. For coherence, I think it is better to write "antecedent wetness conditions" here.

We changed this to “antecedent wetness conditions”.

L. 163. Please, remove “continuously”.

We removed “continuously”.

L. 178-179 and L. 218. Please, correct figure numbers.

We corrected the figure numbers.

L. 235. What is “HQ1”? Please, consider removing if not relevant.

We removed the HQ1 information and only mention the mean high-flow discharge.

L. 500-503. Check this sentence, it includes the same information twice.

We adjusted the sentence in order to avoid duplicates.

#### **Additional minor changes**

Fig. 3. We adjusted the limits of the y-axis of Fig. 3e and therefore inserted a new version of Fig. 3.

L. 454. We moved the sentence about the comparison of the events in June 2018 and September 2020 to L. 460.

Katharina Blaurock

On behalf of all co-authors