

Response to **Anonymous Referee #3**

1. There is a clear goal stated at the end of the introduction, but there are not clear hypotheses until the conceptual model is presented (Figure 6). I think that putting the conceptual framework at the front of the paper (introduction or at latest the methods) would help the reader understand how the authors are viewing the system, better appreciate the findings, and better grasp why certain methods were used.

2. The paper spends quite a bit of time discussing long-term trends in DOC attributed to changes in acid deposition, land use, and climate change. This focus was something of a red herring, as the paper is strongest on a much shorter timescale, which does not speak directly to this literature. Additionally, most of the cited papers on DOC trends are older, which I think is a recognition that while many regional trends exist (for either increasing or decreasing DOC), there is not a clear pattern or signal of anthropogenic effects on DOC concentration. There is more evidence of anthropogenic effects on DOC properties (e.g. Butman et al., 2014), and this could be fruitful, but, I think the ecohydrological focus on sources and fate of DOC is most compelling. This fits in better with the conceptual model and approach of the paper. There are many other reasons to study DOC, many of which are brought up elsewhere in the introduction (Zarnetske et al., 2018), so starting the paper with this observation is less effective.

3. The discussion seemed somewhat uneven to me with the authors still defining some concepts and findings and even describing methods. I think that reorganizing the paper around a clear set of hypotheses would strengthen this already interesting piece of work.

(R3GC1)

We appreciate your evaluation of our manuscript (MS). We acknowledge that the hypotheses of our work were not clearly stated in the introduction. Thus, we will focus the introduction more on how we see the system and mechanisms of DOC export in headwater catchments. In summary, this includes

1 - the addition of clear hypotheses, based on our conceptual framework. We reason that changes in DOC concentration and quality can greatly be explained by the hydrologic situation in the system. The DOC signal in the stream is generated by the exposure of DOC sources to mobilization. The hydrological (mobilization) and biogeochemical (production and processing) dynamics are thereby generating the runoff DOC-response. See also our response to Referee #2 (R2GC1) and #4 (R4GC1), who similarly noted the lack of a clear hypothesis.

2 - More focus on short-term dynamics in general by removing parts of the long-term DOC trend section while adding a more hydro-mechanistic point of view. We will amplify the awareness of hydrological events as a first order control on DOC dynamics. This will go hand in hand with a

3 - reorganization of the discussion section in terms of carefully reviewing the text and move methodological sections to Materials and Methods. Concept explanations which can already help to clarify the specific aim of this paper will be moved to the introduction.

We agree that all these points were not addressed clear enough in our MS as correctly pointed out by the Referee#3. We hope by addressing the above mentioned changes, we will be able to sufficiently channel the focus on the actual claims of our MS.

4. Throughout the paper, I was surprised at the lack of discussion of interactions with other elements. DOC does not cycle in isolation, and stoichiometry can have a strong influence on DOC production and consumption (Helton et al., 2015). not to find greater discussion of DOC removal mechanisms, including heterotrophic respiration and abiotic removal (Raymond et al., 2016). I imagine that nitrogen and phosphorus data are available (NO_3^- data, specifically should be available through the whole time period), and including and integrating them could greatly strengthen the paper. For example, how do N and P vary during the chosen seasonal periods and how might that influence temporal patterns currently attributed to changes in source and transport limitation?

(R3GC2)

We agree that there are factors which would be useful to add understanding to the actual mobilization and production/processing/mineralization mechanisms and, as correctly mentioned by the Referee strengthen the paper. But yet we have decided to keep the focus solely on in-stream DOC quantity and quality dynamics:

Since we measured DOC in the stream, we view DOC as an integrated response signal, already carrying all the information from processing and transformation up to abiotic removal in the riparian zone. Thus, we argue that hydroclimatic dynamics are a first order control of the DOC dynamics in the stream, able to explain large proportions of the DOC variability. Based on actual measurements of the DOC dynamics, this is presented in the MS by a high correlation coefficient of hydroclimatic variables with DOC quantity and quality as well as a high R^2 of our statistical models. Continual NO_3^- data as well as biweekly P data is available, and it would probably allow a more in depth biogeochemical discussion, but including this data would go beyond the scope of the paper and further amplify the chance of losing focus by drifting into a more biogeochemical eco-hydrological paper. Instead, we decided to sharpen the focus only on these first order hydro-dynamical mechanisms which are considered the most dominant drivers not just in our catchment. This allows us to satisfy the - in the introduction mentioned - need to facilitate work on transferable, parsimonious models. For clarification, the above mentioned mechanisms will be discussed in the MS with the here presented point of view.