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Interactive comment on "Spatiotemporal transformation of dissolved organic matter along an alpine stream flowpath on the Qinghai-Tibetan Plateau: importance of source and permafrost degradation" *by* Yinghui Wang et al.

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General Comments:

The abovementioned manuscript describes research on the effect of climate change on permafrost degradation in the Tibetan Plateau and its potential impact on associated fluvial systems, in particular on the dynamics of dissolved organic matter. This research is of global significance as little is known about permafrost degradation in areas other than the arctic, and nearly 70% of alpine permafrost is located in the geographical area of this study. The research team is composed of highly qualified scientists with ample experience and expertise in the specific field of study, and applying ideal methodologies to reach the outlines objectives of this research initiative. The manuscript is well written, and the data properly presented. The literature is also properly reviewed and well represented. As such, this manuscript is well-suited for the journal Biogeosciences and I recommend it to be published. However, some aspects of the manuscript should be improved prior to acceptance. For example, seasonal variability observed needs to be fully explained; explanations regarding the observed differences in DOM leachate composition between the AL vs PL needs to be better explained; discussion on in-stream generation of DOM through microbial primary productivity should be enhanced and variations along the sampling transect better described; etc. These pending issues are described in more detail below.

Specific Comments:

1) L43: "...in-stream metabolism...": Throughout the manuscript make sure DOM degradation via molecular transformations vs mineralization to CO2 is specified as needed. Similarly, dilution (concentration decrease) vs. 'dilution' (change in relative abundance) through mixing with in-stream DOM from microbial PP?

2) L 54: As in #1 – bio- and photo-transformation vs. mineralization? Both?

3) L61-62: Not sure 'hydrologic inputs' is the best way to word this! Please re-phrase.

4) L116: Please indicate distance in Km. This can be deduced from Fig. 1, but would be helpful here for the reader to easily gain a grasp of the spatial extension of the study.

5) L120-124: Please add more details on the methodology used for leachate collection.

6) L124-127+: Please add distances in m or Km as needed.

7) L156-160: Leachate/Water volumes used for the SPE? How did you avoid break-through?

8) L180: ‘Freeze-dried retentates’? Meaning SPE-DOM? Explain or rephrase accordingly.

9) L206-207: Does that mean the in-stream microbial generation of DOM is negligible?

10) L213-216: I do not see any detailed discussion on this inter-annual variability. Please add.

11) L243: Please expand on the discussion of these differences in chemical composition between AL and PL leachates. The information shown in the discussion is highly selective to age and very limited with regards to molecular composition and optical properties. In the first paragraph on page 11 there is some discussion on this with regards to sample Q-1, but nothing much else (i.e. along the sampling transect).

12) L256: How were STDs obtained from n=2?

13) L260: Remove the ‘(‘ before ‘and’

14) L261-264: idem as above – explain differences in composition between AL and PL.

15) L280: What about seasonal variations in the optical properties and MS data? Missing important information here. Please add.

16) L285-288: This statement seems to make sense, but at the same time the DOC concentration from PL is significantly more elevated compared to AL. How much is ‘percolation’ due to freezing reduced?

17) Section 4.2: I encourage the authors to actually calculate physical dilution to see if it indeed agrees with the estimation determined based on age variation. Mineralization and in-stream contributions could be roughly estimated by difference based on dilution only.

18) L304-314: Not clear why the authors make comparisons with values observed in coastal systems. Seems irrelevant in this case.

19) L314-318: The size-reactivity continuum (Amon and Benner, 1996) applies well for marine systems. However, it is controversial for terrestrial systems as both similar and opposite trends have been reported in the literature. Considering this, I would focus on the photo-degradation process, which is more likely dominant in this case.

20) L346-353: I would like to see an effort by the authors in enhancing the interpretation of the MS data here. Can molecular formulas generated/added along the transect through microbial in-stream activity be identified? What about photo-transformation products? I assume not all photo-degraded DOM is mineralized to CO2.

21) L392-393: This seems to make sense, but is still mainly speculative. Can you find partial evidence for this from your MS data (i.e. in-stream DOM)? Not sure it is possible.

22) L413: As above – seasonal variations discussion needs to be enhanced.

23) Figure 5: Color code 'dots' are VERY hard to see. Please enlarge accordingly.

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