Hydrology and Earth System Sciences Discussions

Manuscript Title: Subsurface release and transport of dissolved carbon in a discontinuous permafrost region

Authors: E.J. Jantz et al.

General Comments:

1) The authors use long-term solute and hydrologic datasets in conjunction with a modeling framework to explore the dynamics of DOC and DIC release and transport in a high-latitude catchment.
2) The writing here is often vague and should be greatly edited to improve clarity and concision. While I believe that some of the findings have very broad implications, the authors seem to have written this article with a very narrow and specific audience in mind. The general structure of the manuscript lacks traditional method/results/discussion sections. And while this may be a stylistic criticism, I feel that a reorganization of the text – so that a distinct methods, results, and more expansive discussion sections – would greatly improve the flow and readability of the manuscript.
3) This manuscript obviously builds off of the prior work published by Lyon et al. (2010). However, the manuscript could be improved by more clearly articulating how this approach presents a novel contribution or conceptual advancement based beyond this prior study. In the Introduction, the authors should be more clearly define what the Lagrangian approach is (Page 192) and what are its advantages (beyond flexibility) compared to alternative or previously published approaches.
4) The authors often invoke permafrost as possible control on the temporal and spatial dynamics observed and modeled in this study. However, there is no evidence or citation presented to support the presence/existence of permafrost in the study catchment. The authors cite Ridefelt et al. (2008) to acknowledge a "probable presence of permafrost" at higher elevations. Moreover, modeling framework is not designed or parameterized to account for changing soil thermal dynamics, permafrost configurations, or C pool sizes. It just happens that the study was conducted in a discontinuous permafrost region – but permafrost has very little to do with this study – at least as presented.
5) I recommend including a table, similar to Table 1 in Lyon et al. (2010) that summarizes model parameters, values, data sources, etc. Also, it would useful to include a conceptual diagram that illustrates the stream tube and how the different parameters are influence solute concentration and export.
6) I also recommend expanding the discussion to put these findings in a much broader context, particularly in relation to C cycling at high-latitudes. Recent publications by Suzanne Tank in Global Biogeochemical Cycles explore landscape and source controls on DIC and DOC flux in arctic rivers. How might these findings presented here scale to larger catchments or to the region? What are the implications for DOC and DIC release under projected warming

or future hydrologic conditions? Is there any evidence that permafrost was present at the beginning of the data record, but has since thawed?

Specific comments

1) Page 191, Line 1 – Omit "are believed to"
2) Page 191, Lines 3-4 – Should cite Harden et al. 1999 (Science) to support long-term C sink statement.
3) Page 191, Line 26 – Omit "While these phenomena can be described in such a straightforward manner"
4) Page 192, Line 15 – Omit parentheses around "Lyon et al."
5) Page 192, Line 25 – While seasonal frost is a definite, it's not clear that permafrost is present in the study catchment based on the evidence cited by the authors.
6) Page 192, Line 25-26 – Sentence stating, "we further link previous modeling components" – this is quite vague? What modeling components? How were the distinct and how are they now going to be coupled?
7) Page 194, Lines 1-5 – Should provide genus and species for dominant vegetation types.
8) Page 195, Lines 5-6 – The "arbitrary stream tube" needs to be more clearly defined and its function explained.
9) Page 195, Line 22 – "DIC in solid, soil or aquifer" confusing as written. What is the difference here between solid and soil?
10) Page 197, Lines 16-18 – Please describe further the "ensembles" discussed here. How does one stream tube differ from another in terms of parameters, etc.?
11) Page 198, Line 8 – Replace "exemplify" with better word choice
12) Page 198, Line 18 – "under various conditions of relevance" – needs clarification and further explanation. Vague as written.
13) Page 201, Line 20, 22 – Typically, significance is evaluated at $P< 0.05$. If you're going to consider trends significant at $P>0.05$, this should be stated upfront in the methods section.
14) In general, the section and subsection titles in section 5 are pretty vague and could be improved. For instance "Relating observed fluctuations to model implications" could be restated as "Relationship between observed and modeled C dynamics"
15) Fig 2 and 3 – Add DOC or DIC to y-axis labels
16) Fig A1 and A2 are replicates of each other. Figure A2 should show relationship between alkalinity and DIC.