Response to Review comments (original comments in bold with responses following)

General comments RC1

Interactive comment on "Subsurface release and transport of dissolved carbon in a discontinuous permafrost region" by E. J. Jantze et al.

This manuscript reports on novel data from a high-latitude catchment that adds to the important body of knowledge on catchment solute export. Further the paper couples these interesting data with a simple but elegant model framework to gain conceptual understanding. The paper is also very well written. I recommend publication subject to the comments and suggestions below.

We thank this reviewer (Jim Jawitz) for his positive comments with regards to the value of this current study in the context of catchment solute export.

1. I found the use of a uniform distribution for g(tau) not very well defended (p. 10). As the authors note, other distributions are more realistic. I suggest either including a more realistic function or more strongly defend why the improvement in results would not be enough to warrant the added complexity of a 'more realistic' distribution.

The reviewer is correct that there are potentially other or more "realistic" distributions that could be implemented; however, we have selected the uniform distribution for $g(\tau)$ because it allows for purely analytical solution rather than requiring numerical approaches to arrive at a solution. Further, to investigate a full suite of the possible distributions offering more "realistic" solutions is not the aim of this paper where we are exploring the modeling framework. We have added text in the manuscript clarifying this motivation and more strongly defending our choice of distribution for $g(\tau)$. Other, more realistic functions for $g(\tau)$ have for instance been studied by Persson et al. (2011) and Cvetkovic (2011), and their often greater τ variability than that studied for uniform $g(\tau)$ are likely to further only enhance the main implications of spatial τ variability that are shown and discussed in this study.

2. In at least three parts of the paper (pp. 11, 14, and 15) the authors discuss the relative temporal variability of concentration, flow, and load. I have used the ratio of the variances of ln_c and ln_q to describe and understand the relative dominance of c and q variability in relation to chemostasis. As shown in the paper cited below, the mass flux correlation to discharge, such as the authors found in Fig. 6, can be shown to be analytically explained by this variance ratio. However, the authors have used range rather than variance when discussing the effect of "fluctuation around temporal mean" (p.15). As the author of the paper below, I naturally encourage these authors to consider the benefits of ln-variance ratio. At a minimum, they might find it instructive to review: Jawitz, J.W., and Mitchell, J., 2011. Temporal inequality if catchment discharge and solute export. Water Resources Research, 47, W00J14, doi:10.1029/2010WR010197.

The reviewer makes a valid point in this regard. We have included discussion highlighting the potential benefits of considering variance ratios of the log transforms (specifically, the possibility of obtaining an analytical solution). This expanded discussion allows for reference to Jawitz and Mitchell (2011) (and the relevant text there in); however, full application of the approach is outside the scope of this study.

Specific comments RC1

1) Abstract: Consider reporting the solute dissolution/release rates here

We have opted to include the characteristic release times in the Abstract text as these are more appropriate for this study and modeling approach. These are estimated to be 1 year for DOC and greater than the average advective travel time (>>1 year) for DIC.

2) P. 2, line 11: Include the country here

This change has been made in the text

3) P. 2, line 13: Check this again. Concentration may have been flow-independent but not load.

We have rewritten the sentence to clarify this misunderstanding.

4) P. 2, line 16: Again, consider rephrasing since probably all loads are 'high flow-dependent'

See answer for question no 3)

5) P. 3, line 26: I suggest modifying "described" with "qualitatively", "conceptually", or similar

This change has been made in the text

6) P. 4, lines 5-12: It is not necessary to cite every paper on the subject. Two or three of these will suffice.

This text has been modified (from line 51 in the revised manuscript), decreasing then also the number of references.

7) p. 6, lines 15-18: The point being made is that the regression slope is very close to one. Thus reporting a good r2 is not enough here, the slope is also important. The text says "1:1" but the slope is really 0.935. Was this used as a correction factor, then? I suggest being more explicit here.

To clarify in this regard, the slope equation has been added to the text and used to define the TOC/DOC relationship. Furthermore, to be more precise, we added the trendline with intercept; $[\mathrm{TOC}] = 1.195 \cdot [\mathrm{DOC}] - 0.28$ with $r^2 = 0.98$.

8) p. 6, lines 23-27: The load estimation method described is appropriate when daily flow and daily concentration data are available. Concentration data were available on a limited temporal frequency, so more consideration is suggested for the appropriate interpolation of concentrations and loads. Cohn and collaborators argue for regression methods (see the USGS software LOADEST). There is a rich discussion on this topic in the literature.

In the current study, the simple load estimation method was selected as we are not explicitly interested in the daily or short-time dynamics, but rather annual export (and relationships there in). The reviewer is correct that discussion of potential limitations of this approach or benefits of others (LOADEST being one of available and possible methods for calculation of loads, used e.g. by Tank et al. (2012) should be presented for completeness. We have added such consideration in the methodology and discussion of the revised manuscript.

9) pp. 6-7: I was surprised that an annual value for DOC was dismissed as an outlier so casually. This value presumably arose from many independent measurements. I think more examination and/or discussion about this is warranted.

To clarify, the DOC value the reviewer refers to was not casually dismissed but rather was statistically determined to be an outlier as it was more than twice the interquartile range above the median of the annual data. Further, this outlier status can be compared with the annual DIC for the same year which was not seen to be an outlier. This implies possible error in the sampling around DOC in the monthly values. For the year in question, the month where the observed peak DOC concentration occurs does not correspond to the late season peak streamflow observation while the observed minimum DIC concentration does (which is atypical for this system according to Giesler et al. (2013)). We have clarified these circumstances and the statistical method used in the text to avoid sounding "casual" in the dismissal of the value.

10) P. 7, line 20: Should the numerator be dc*?

This change has been made in the text.

11) P. 8, line 7: Remove "first" here (it is repeated on the next line).

This change has been made in the text

12) P. 8, "flown" is not the correct usage here. Some might use "flowed" but that's not very good either. I suggest a different sentence construction in these cases.

We have changed this sentence accordingly and reconsidered sentence structures in similar cases throughout.

13) P.8, line 11: It is not necessary to add the clause that begins "i.e. …," It is definitely not necessary to add such clauses for each equation.

This change has been made in the text.

14) P. 9, line 1: Neither "c" nor "s" are in the following equations. Further, equation 4a appears to be exactly equation 3a and thus does not need to be repeated. Also equation 4c seems to be exactly equation 3c.

The reviewer is correct and these formulations are provided to give connection and consistency with previous work. In addition, Equations 3a, 3b, 4a, 4b are repeated in regard to completeness with respect to the values of *t*.

15) P. 9, line 18: should be "flux-average" (or "flux-averaged")

This change has been made in the text

16) P. 10, lines 22-24: Consider specifying what method was used to determine this

This was determined using a distributed travel time model based on Darcy's Law and confirmed regionally using isotope tracers in a nearby catchment. We have added the appropriate text.

17) P. 11, lines 6-7: Remove the clause that starts "even if. . ." as this is repeated from the previous line.

This change has been made in the text

18) P. 11, line 9: I think the "catchment-average" formula here is only correct if the two variables are uncorrelated.

The running text formula for catchment-average mass flux $\bar{s} = \overline{c_f} \cdot \overline{q_s}$ commented on here is exactly the same as the second part of equation (5), which follows directly, by definition, from the first part of equation (5). The first part defines the flux-averaged concentration on the catchment scale $\overline{c_f} = \frac{1}{\overline{q_s}} \int_0^\infty sg(\tau)d\tau$, in which the integral $\int_0^\infty sg(\tau)d\tau$ is by definition the average mass flux $\bar{s}$. As a consequence, the catchment-average mass flux must then equal $\bar{s} = \overline{c_f} \cdot \overline{q_s}$, without any need for any independence assumption to underlie this equation. One should not be misled here into thinking that the catchment-average mass flux expression $\bar{s} = \overline{c_f} \cdot \overline{q_s}$ is some simplistic translation of the local relation $s = c\, q_s$ by just averaging each of the local variables. Such a simplistic (and erroneous) procedure would namely involve the simple average (resident) concentration $\bar{c} = \int_0^\infty c\, g(\tau)d\tau$, which differs from the flux-averaged concentration $\overline{c_f} = \frac{1}{\overline{q_s}} \int_0^\infty (c\, q_s)g(\tau)d\tau = \frac{1}{\overline{q_s}} \int_0^\infty sg(\tau)d\tau$ that is involved in the correct $\bar{s} = \overline{c_f} \cdot \overline{q_s}$ expression.

For improved clarity, the running text commented on by the reviewer has now been removed in the revised manuscript, the first part of equation (5) has been expanded to show the full local mass flux expression in the $\overline{c_f}$ integral as:

$$\overline{c_f} = \frac{1}{\overline{q_s}} \int_0^\infty (c\, q_s)g(\tau)d\tau = \frac{1}{\overline{q_s}} \int_0^\infty sg(\tau)d\tau \;\; ; \; \bar{s} = \overline{c_f} \cdot \overline{q_s} \qquad (5)$$

and the following clarification has been added directly after the sentence that introduces equation (5):

"The catchment-average mass flux $\bar{s} = \overline{c_f} \cdot \overline{q_s}$ in equation (5) follows directly from the definition of flux-averaged concentration on the catchment scale $\overline{c_f}$, in which the integral $\int_0^\infty sg(\tau)d\tau$ is by definition the average mass flux $\bar{s}$. For further physical explanation and quantification of the difference between the flux-averaged concentration $\overline{c_f}$ and a simple average (also called average resident) concentration

expression $\bar{c} = \int_0^\infty c\, g(\tau) d\tau$, *and on the direct relation of (only)* $\overline{c_f}$ *with the large-scale average mass flux* $\bar{s} = \overline{c_f} \cdot \overline{q_s}$, *we refer the reader to Destouni and Cvetkovic (1991; specifically their section 5. Comparison between the flux-averaged and the resident concentration models)."*

19) P. 14, lines 23-26: I was unclear if these statements were intended as conjecture or if there were data to support them.

These statements are not conjecture but deal with the theory for this work which are described in detail in section *2.3 A mechanistic framework for modeling of solute release and transport* (in the new manuscript version). Thus, the statements are supported both by the mechanistic model and also in the observed DIC data. We make this clearer by referring to section 3.

20) P. 16, Consider moving section 5.3 up ahead of the current 5.2

This change has been made in the text

21) Figure 2b: I was not clear why this was labeled concentration or mass flux.

Following Eq. 3-4 we find that the solution of the calculation $c^*\theta/c^*_0$ is dimensionless. Because of θ - the average volumetric water content [-] is dimensionless (L^3-water/L^{-3}-bulk, =n, porosity, under full water saturation) in the stream tube.

22) Figures 2 and 3: Nothing new happens in these 4 graphs after 2 to 4 years, so it is not clear why they are extended for 10 years.

The figures 2 and 3 (figure 4 and 5 in the new manuscript version) have been changed so that the x-axes extend to 5 years.