**Referee 2: Micha Fritz**

The manuscript provided by Bulger et al. investigates the role of retrogressive thaw slumps in moderating dissolved organic carbon delivery to stream ecosystems in north-western Canada. Comparing the similarities and differences in biogeochemical parameters upstream of such slumps, within slumps and downstream of slumps the authors convincingly conclude that adsorption processes between fine-grained mineral surfaces and DOC occur. The study shows that DOC gets removed from solution on short timescales and along short pathways, which is an important finding if we think about potential organic carbon mineralization into greenhouse gases and the potential destination of permafrost organic matter after thaw and mechanical mobilization.

A minor shortcoming of study is that it touches inorganic hydrochemistry (e.g. major cations) but it does not use these data in the statistics for hypothesis testing - or I have missed this. In a next step I can see the potential in analyzing organo-mineral complexes and mineral surfaces upstream and downstream for validation of the different results in Canada, Alaska and Siberia.

The authors present original data and provide a very thorough and detailed description of the methods. In general, this topic and the presented data are of interest for researchers studying thermokarst, rapid permafrost degradation and arctic biogeochmistry. The language is generally very good and the figures and tables usefully complement the text.

I suggest the manuscript to be accepted after minor revisions.
*We thank reviewer 2 for these positive comments, and also for the very thorough review of our manuscript. A brief response to the comment on our major cation data: we present (and collected) these data to bolster this work, which was designed to focus on dissolved organic carbon release from slumps. As a result, these data are presented in an ancillary fashion. However, we do incorporate them into our statistical analyses (see Table 2). Although the cations are not a focus of the current manuscript, we certainly agree that this study has uncovered organo-mineral interactions as a fruitful direction for future study: both in this region, specifically, and for cross-region comparisons.*

**General comments:**

Title: I suggest to change the title into: "Retrogressive thaw slumps moderate dissolved organic carbon delivery to streams of the Peel Plateau, NWT, Canada". This would highlight the process-driven character of biochemical interaction between DOC and ...
*We use 'temper' as : "Act as a neutralizing or counterbalancing force to (something)" [Oxford dictionary], and prefer this to 'moderate' in the title. We've kept the title as-is, rather than switching 'temper' to 'moderate'. Thanks for this suggestion, however!*

The Introduction has 4 manuscript pages is therefore very long. Please cut (I have made some suggestions in the annotated manuscript attached) and align the internal structure according to the following points:
1. global relevance of the topic
2. specific relevance to the research field
3. previous work in this direction
4. knowledge gap(s)
5. overall aim how to fill the knowledge gap
6. objectives (specific and measurable)

*Thanks for this useful comment. We have worked to streamline the Introduction by: making the majority of changes suggested in the annotated manuscript (see further details below); adding a 'knowledge gap' statement (current L165-169); clarifying the last paragraph to include a clear statement of objectives (current L170 onwards; see uploaded PDF); and making some additional editorial changes to streamline the text. We have retained two paragraphs in the Introduction that discuss retrogressive thaw slumps: both generally (paragraph 2), and on the Peel Plateau specifically (paragraph 3). While we have streamlined these paragraphs somewhat, they do add a fair bit of bulk beyond a 'normal' Introduction. However, we feel that this text is important to include in the Introduction, because this feature type may not be familiar to biogeochemists that study permafrost thaw, and understanding the Peel Plateau landscape is critical for understanding the study objectives.*

**Specific comments:**

For specific comments see the annotated and attached pdf-file.
*Thanks for this file. Below we've listed line numbers where modifications were <u>not</u> made as requested in the annotated PDF, or where we felt some additional comment might be useful. Except for the items listed below, all changes were made as suggested. Line numbers in bold below refer to the commented PDF submitted by Dr. Fritz.*

-**line 24:** ***Regarding question about whether the process we discuss is caused by adsorption to clays.*** This would certainly be our hypothesis! However, at this point, this has not been explicitly tested, so we have not added additional text to the abstract.

-**line 24-27** is highlighted but I can't find a comment associated with this text, so no changes were made.

-**line 44:** "forecast" (rather than "forecasted") was retained; as a result we also haven't made the other grammatical changes in this sentence.

-**line 70:** We add Lantuit et al., (2012) but not Lantuit and Pollard (2006); the Kokelj reference provides a broad perspective on thaw slumps from across the western Canadian Arctic, so we've chosen to add only one reference from Herschel Island at this point in the text, to also add this more specific perspective.

-**line 77:** We keep "materials", rather than "material", and therefore have also not modified the remainder of this sentence (to the singular).

-**line 82:** an insertion is indicated, but I could not see text associated with this, so no change was made.

-**line 355:** ***Regarding the following comment: This also means that thawing ice-rich Pleistocene strata which contain large ground ice contents dilute the DOC signal from organic-rich horizons (see Fritz et al., 2015 [The Cryosphere] and Tanski et al. 2016 [GBC] for typical DOC concentrations in different ground ice types).***
Agreed! We haven't modified the text here (to avoid adding interpretation or discussion of results to the Results section), but have added text to clarify the importance of ground ice later on in the Discussion, including a specific reference to these two citations (current L743-744).

-**line 342 (Section 4.2):** While we added some DOC values as suggested for sections 4.1 and 4.4, we haven't added major ion data here. We've chosen to keep the focus of the text in this section on relative change in major ion concentration between upstream and downstream sites.

**-line 438:** ***Regarding the comment that the 'dilution effect' or low DOC concentrations that we see for slump outflow from some slumps is caused by low DOC ground ice.***
This ties to our response to the comment about L355 above. As mentioned above, we agree that this is worth clarifying in our text. In this section (5.1) we are discussing more patterns in DOC across slumps; the mechanisms for these patterns are more thoroughly dealt with in section 5.3. To address this comment, we have pointed the reader to Section 5.3 at this point in the text (current L601), and added a specific consideration of DOC and ground ice in that section (current L743-744), including reference to the Fritz and Tanskii citations.

**-line 462:** ***Regarding the comment "This sentence does not provide much useful information and should be somehow combined with the next sentence to carve out the differences of the landscapes and stratigraphy".***
This section of the Discussion has been re-worked slightly, in part in response to comments that were also made by Reviewer 1. We split this sentence in two to allow the second half to tie more clearly to the sentence that follows (current L626-629).

-**line 461:** "Lentic" was retained, rather than switching to "limnic"

-**line 552**: *Regarding additional hypothesis about the DOC: temperature link being caused by temperature effects on headwall thaw.*
This point in the text discusses the relationship between temperature and DOC at upstream (i.e., pristine) sites. As a result, we have not added this hypothesis, which is applicable only to slump-impacted sites. We do discuss our findings for slump-impacted sites in the following paragraph of the manuscript. To address the reviewer's point, we have modified our wording to clarify that this component of the text deals with upstream-of-slump sites only (minor changes for clarity throughout the first paragraph of Sn 5.4).

-**line 611-614**: text is highlighted, but no comment is present, so no change was made.

-**line 615**: we retain "for example" to allow us to indicate that it's not just the Peel Plateau that is different from other regions of the Arctic.

**-line 617:** ***Regarding the comment: "These are not the best references to active layer deepening and its effects. Look at the CALM papers in GTN-P"***
The Kokelj and Vonk citations are provided to reference the statement that "non-linearity can also be expected to extend to different types of permafrost thaw …" (ie, the finding of non-linear response). As a result, we have retained them here, but we also add a CALM paper (Romanovsky et al., 2010) to make sure we also include a reference that documents the process of active layer deepening, specifically.

-**Table 3:** ***Regarding question about why only a subset of our slump features have 14C measurements.***
We went back to our sites two years after the main study to collect 14-C measurements to help us better refine this story. We only have measurements from 4 of our 8 slumps because some of these features are difficult to access (helicopter only).