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Interactive comment on "Retrogressive thaw slumps temper dissolved organic carbon delivery to streams of the Peel Plateau, NWT, Canada" *by* Cara A. Bulger et al.

Anonymous Referee #1

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This manuscript represents an important step in our understanding of permafrost thaw dynamics from the Canadian Arctic. As the authors point out, the permafrost regions of the Arctic are not all similar, and region-specific work such as this are critical to our understanding of the impact of climate change on the Arctic as a whole. The key finding of the manuscript, that permafrost thaw slumps may in fact reduce DOC delivery to Arctic streams, is timely and should be of interest to permafrost biogeochemistry researchers in general. I have some minor concerns detailed below, but I don't think they will greatly impede publication of this manuscript. The manuscript is well written, and the tables and figures are well presented and generally very clear.

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Discussion paper

One general comment is, and this could be addressed in the discussion for example without necessarily the need for extra data, what is the significance of this main finding to the overall carbon cycle/budget for such landscape features? The authors measure total suspended sediment (TSS) loss from the study features, but give no indication of the carbon content (I guess it wasn't measured). If DOC export is reduced due to adsorption to fine-grained sediments, these sediments are also mobilised and exported, and must carry some carbon. In Figure 3A we see that TSS increases downstream of these thaw features (unlike DOC), so can anything be said about the fate of the carbon that is locked up in this flux?

» Specific comments:

L. 17 (and/or introduction L. 66-69) – I recommend defining retrogressive thaw slumps specifically early on, do they differ from a normal thaw slump (active layer detachment, slide for example), and is the "retrogressive" characteristic of this type of slump especially different from thermokarst processes in general?

L. 49-51 – some resilience in the region is also possible in response to gradual permafrost thaw (e.g. Dean et al. 2016 doi: 10.1007/s10533-016-0252-2).

L. 53-56 – is DOC the primary substrate in soils or during aquatic transport/storage? I don't think there is a clear consensus on this point, and it's not clear exactly what your point is in this sentence. Yes, DOC can degrade to produce CO_2 (and CH_4) in Arctic aquatic environments, and this has been highlighted by recent studies (e.g. Spencer et al. 2015 GRL; Drake et al. 2015 PNAS; Mann et al. 2015 Nat Comms). But that doesn't mean DOC in the aquatic zone is the primary source of CO_2 released from streams and lakes. Most of the CO_2 released from streams is generated in the soil zone and transported laterally (Hotchkiss et al. 2015 Nat Geosci; Marx et al. 2017 doi: 10.1002/2016RG000547. So, I think it's important to not be too throw-away with this point, and be a bit clearer about how this aspect of Arctic aquatic carbon cycling relates to the study presented here.

L. 86-89 – would be good to compare this to pan-Arctic thermokarst estimates (e.g. Olefeldt et al. 2016 doi: 10.1038/ncomms13043).

L. 110-112 – please provide a reference to support this, or make it clear that this statement is hypothesis at this stage.

L. 126 – please be quantitative and give an area estimate, rather than saying "large portions" of the Arctic. Earlier the authors emphasise that the Peel Plateau is different, hence the uniqueness of this study. Please clarify the aims and intent regarding this point.

L. 180 – so these were exceptionally wet years? ~100 mm greater than the monthly averages? What is the significance of this enhanced precipitation to the DOC and TSS dynamics described in this study compared to other years?

L. 193 – how were the sites chosen to be representative? Was this done with remote sensing, or based on the experience of the authors? Either is fine, just to clarify.

L. 207 – suggest change to "... geomorphology were previously described by..." Or is the Malone et al. data actually used here?

L. 334-335 – would be good to clarify here and in the discussion, that only DOC concentrations are considered here, not fluxes. Dilution from the high rainfall reported for the study period could play a part. That said, the flux values from the FM3 site in Fig. 6 support the findings as presented.

Section 4.2 – might be useful for compare the geochemistry to pristine waters from the NWT (e.g. Dean et al. 2016 doi: 10.1007/s10533-016-0252-2).

L. 346 – I wouldn't consider Ca2+ or Mg2+ to be conservative in general, it would be good to justify whether they are conservative in the current study system.

L. 387-392 – with a rough mass balance, would it be possible to estimate the downstream DO14C values?

L. 399-401 – this is a very important point, showing that the fluxes show the same pattern as concentrations (see comments on L. 334-335). Maybe emphasize this linkage of the FM3 findings to all the study sites.

L. 421 – the Drake et al. and Mann et al. 2015 citations don't really fit here. These studies focus on degradation dynamics not mobilisation. Also, the Drake study used permafrost soils from Alaska. Please check the other references in this sentence.

L. 433-435 – DOC:ion ratios are key here to support the DOC flux vs. concentration issue I pointed out earlier. Why not emphasize these ratios more? (note comment regarding L. 346).

L. 439 – what field evidence specifically?

L. 447 – why not data from the other sites too?

L. 449-454 – much of this should really go in the methods.

L. 452 – TSS shouldn't be considered conservative in my opinion: entrainment and deposition processes would be occurring, so how can conservative behaviour be justified? Or is that the point here, that it isn't conservative? Hard to follow the reasoning here. Perhaps if the approach used here were more clearly outlined in the methods section, and the results presented more clearly (e.g. in a table), this might help clarify this point.

L. 469 – where is it sequestered? Within the exported TSS load, or in the depositional environments within the study systems? If sorbed to mineral complexes, is the carbon sill bioavailable? What does this process mean for the overall fate of the carbon released from the thaw slump features? This is an important aspect of the discussion that is currently missing.

L. 491 – what do these values actually say about the condition of the DOM? This could also be added to the results section.

L. 504-511 – would it be possible to quantify the relative contributions from these end members (e.g. Winterfeld et al. 2015 doi: 10.5194/bg-12-3769-2015)? Would make a nice addition to the manuscript.

L. 565 – "across multiple measurement points" – not all points were used?

L. 579-585 – this sentence begins with "This result clearly highlights. . ." but the rest of the sentence is long and unclear. Please rewrite to clarify the sentence's clarity.

L. 582-583 – Where is this evidenced in the present study?

L. 597 – inter-regional or slump type? If referring to the results of the present study vs. previous/pan-Arctic findings, then state this more definitively.

L. 602-606 – how important is thaw slump derived DOC to the Arctic carbon budget? Is it large enough to justify its inclusion in ESMs at this stage? Or is its inclusion into smaller-scale process models more justified? This information could also be incorporated into the introduction for context.

L. 610 – is this explored in the present study?

» Technical corrections:

L. 42-43 – it's good practice to cite specific chapters in the IPCC reports rather than the whole report or the summaries as used here.

L. 49 – remove the "s" from "thaws"

L. 93 – Should this be "thaw-" or "thermokarst-" affected regions, rather than "permafrost-affected regions"?

L. 141 – how brief is brief? Please give numbers.

L. 225 – Helicopter! Cool.

L. 232-240 – how long was there between sampling and analysis, generally?

L. 257-265 – please provide precision/sensitivity estimates for each analysis.

L. 300 – define AIC acronym when used for the first time.

L. 423 – missing space between comma and permafrost.

L. 457-460 – reference?

L. 483 – I would add the following references from the region near the current study: Street et al. 2016 doi: 10.1002/2016JG003387; Quinton and Pomeroy 2006 doi: 10.1002/hyp.6083).

L. 586 – remove semi-colon.

L. 612 – missing an "as"

L. 612 – should be Vonk et al. 2015b?

L. 619 – suggest changing "effects" to "impacts" otherwise too many effects/affects in this sentence.

Table 2. Is the "t" from the Wilcoxon output?

Please add the figure captions to the figures next time (personal preference).

Figure S1. These pictures are impressive, why not include in the main manuscript? Is it possible to include a rough scale in the photos also?

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