Biogeosciences, Response to reviewer comments, 19 June 2015

Dear Editors and Reviewers:

Below is a detailed description of how we have addressed each of the referee's comments. We have a complete, revised copy of the manuscript including revised figures to send or upload at any time. We were very pleased with the positive and constructive reviews and feel that the comments and suggestions have improved the manuscript. We would like to thank the reviewers for their efforts. Reviewer comments to authors are listed below in italics and the responses to reviewers are in bold.

Sincerely,

Julia R. Larouche on behalf of all the co-authors

REFEREE #1

Issue 1: The number of figures is too high compared to the length of the ms. Moreover in some parts of the results and discussion sections are difficult to follow since in a same phrase we have to look for several graphs in different figures. I don't have any suggestion about this but I think that the quality of the ms will improve if a different choice would be made. Probably by merging some the figures the paper will become more clear (e.g. 2 and 3).

Response 1: We agree with this first point by Referee #1. We removed Figure 2 and will describe the results in the text instead. The intention of this figure is to show differences among reference streams by geographic region to show the influence of regional differences independent of impacts of disturbance. We also moved Figure 5, which shows the inverse relationship between SUVA and BDOC, to the supplementary materials. We have considered the representation of this data in many ways and, we think the current figure format is the most appropriate way to represent the data given the nature of the questions (i.e. the various groupings of the data set for each question). We edited the labels on the x-axis of figure 3 (see below) to improve clarity. The new label makes it clear that there are two comparisons being represented: the first (the left side) is the impact of thermokarst (in the Feniak Region) and the second (right side) is the impact of thermokarst in a burned landscape.

Issue 2: The main hypothesis was not supported by data and, indeed, the authors made a good discussion on this subject. The authors claimed that the site characteristics (soil, age, water chemistry etc) are key factors for the lability/biodegradability of DOC. This conclusion is of course supported by the data. However I would think that this was an important issue even not doing this experiment. Therefore I suggest that the authors should clear point the importance of this conclusion trying to make it general thus presenting the new science obtained with this study.

Response 2: We acknowledge that it has long been known that landscape characteristics, particularly landscape age and surficial geology, exert control on various ecological factors (soil composition, vegetation type, water chemistry, microbial composition, etc.). Given the data we have (which are very difficult to obtain), there is an indication that landscape differences may be more important than disturbance impacts. Our data primarily allows us to make comparisons at the landscape scale, rather than the local scale. We started this research several years ago with the expectation that thermos-erosional features (thermokarst) and wildfire would have major, long-term impacts on stream ecosystems that could be observed at both the local and the watershed

scales. The data in this paper suggests that the impacts from disturbance may not be discernible from background 'noise' (e.g. landscape variability) if you expand the spatial scale of consideration or lengthen the time of consideration from the initial point of disturbance. We acknowledge that we have an imbalanced experimental design and do not want to over-interpret our results. The importance of our conclusion in general terms is that although the impacts of hydrologic nutrient and BDOC flux may be large and important for large thermos-erosional features immediately after they form (as shown by other studies), in the larger landscape context and over a longer period of time, the impacts *may* become subdued to the background, landscape-to-landscape differences. An important question moving forward for this type of research is whether a threshold exists of feature type and size combined with stream or river or lake size when a geochemical or biological impact may or may not be detected? Our data cannot answer that question; however, our data does indicate that stream ecosystems may be more resilient to the impacts of fire and thermokarst than previously expected.

REFEREE #2

General Response: We have addressed all of the comments provided in the supplemental pdf (see below) and have corrected all the grammatical and citation mistakes Referee #2 highlighted.

Comment 1 (Line 169): Can you provide the site numbers (as shown in Table 1) as a reference for these locations?

Response 1: Yes, we have added those site numbers to this section for clarification.

Comment 2: Looks like a note to self that was missed. Please do indication the types and concentrations of the nutrients additions used. Also please indicate the temperature of the incubations.

Response 2 (Line 180): We have added the appropriate information to the text. Increasing ambient concentrations by 80 µM NH_4^+/NO_3^- and 10 µM PO_4^{3-} and samples were stored in the dark at room temperature.

Comment 3 (189): Although only one sample was removed, the authors should provide more details with respect to the methods and decision making processes used to evaluate and identify the "suspicious" samples and to determine if they should be rejected.

Response 3: All of the DOC concentrations from each of the four replicates, for each time stamp, for each sample were carefully inspected. Samples that contained 1 of 4 incubation replicates that was considerably different than the others were flagged and further inspected. If the questionable value was more than two standard deviations from the mean then it was removed. The above information has been added to the text of the methods section to clarify this important point.

Comment 4 (Line 194): Were any standards used to verify the performance of the instrument? if so please detail these.

Response 4: The baseline was corrected using DI water at the beginning of each run to ensure there was no absorbance measured. The above information has been added to the text of the methods section to clarify this important point.

Comment 5 (Line 210): This citation (and the reference) is missing the year.

Response 5: The citation and the reference have been corrected.

Comment 6 (Line 212): Provide some indication of whether the data were generally normal or not - were the data pretty consistently normal or not, or quite variable?

Response 6: The data were for the most part consistently normally distributed with few exceptions – this important point was clarified in the text.

Comment 7 (Line 372): Which characteristics? Specify if you are referring to low elevation? or high? Its unclear.

Response 7: We agree that this sentence was unclear and have changed it to read:: 'The older landscape and lower elevation in the Anaktuvuk area may explain the higher concentrations of DOC, TDN and TDP and the lower % BDOC observed in the streams, regardless of the impact of fire or thermokarst'.

Comment 8 (Line 385 and 396): citation mistake?

Response 8: Yes, thank you. It should read (Jorgenson et al., 2010) and we had not included it in the reference list but only at the end of Table 1. We have corrected this error.

Comment 9 (Line 406): There is one site that has much higher suspended sediment than the others (Feniak) - the papers by Kokelj et al. 2005, and 2009 suggest that DOC-sediment adsorption reactions play a role in controlling DOC in slump affected lakes. It would be helpful if the authors discussed the potential role that differences in the suspended sediment in the various sites (impacted or not?) might play in controlling DOC amount and quality - it seems like the only control that was not considered - in what is otherwise a very comprehensive and well written section.

Response 9: We would like to thank the reviewer for making this sharp observation since it uncovered a mis-categorization in our database. The TSS value of ~120 mg L^{-1} from the Feniak region represents an effluent sample from the active layer detachment slide and not from the tributary in which the effluent flows into. Our intention is to report background water chemistry from just the stream sites - not from the thermokarst effluent – since our BDOC samples were taken from streams and not effluent. We have now removed that value from Figure 6 (now Figure 4) and ran the statistics again. There remains a relatively high value of ~98 mg L^{-1} from one of the Feniak sites (an impacted stream). We have added a few points in the Discussion regarding the potential role of suspended sediment in controlling DOC concentrations (Lines 418-428).

Comment 10 (Line 410): I highlighted a few places where there were some inconsistencies in the formatting for 'et al.," but I am likely to have missed some. Please do a search and replace for this to be sure your formatting is consistent.

Response 10: The formatting is now consistent.