

Interactive comment on “Biodegradability of dissolved organic carbon in permafrost soils and waterways: a meta-analysis” by J. E. Vonk et al.

Anonymous Referee #2

Received and published: 27 August 2015

GENERAL COMMENTS The authors conducted a meta-analysis of measurements of biodegradability of the dissolved organic carbon in soils and flowing waters of the Arctic, the data included measurements of their own. This work represents an important contribution, not only for Arctic systems but also in general, as quantification of biodegradable DOM is a common question/practice for researchers investigating ecosystems worldwide. The manuscript is mostly well written but it seems like BDOC is used interchangeably for biodegradability of DOC and biodegradable DOC throughout the document, the authors should clarify this. The intention of the paper is great but it will strongly benefit from more data. An important point to make would be the difference between what is really measured in these biodegradability assays: potential biodegradable DOC as opposed to true biodegradation in situ. Removing the sample (especially soil) from the environment can have important implications in the results;

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this is somewhat analogous to the old conversation around measurements of denitrification.

SPECIFIC COMMENTS P8357L3: what do you mean by increasing flow paths? Increasing number of flow paths? P8357L13: not true for all DOM P8357L25: some of these factors do not affect the biodegradability of the DOC per se, they affect the results in measurements of BDOC, maybe add “the quantification” before “. . .BDOC, including. . .” P8359L11: the subcategory for streams (>250 km²) seems too broad. Streams of 250 km² watersheds are rivers, maybe better to drop the class names. P8360L18: the incubation period is not 28 days for all the data points right? Eliminate “28 day”. P8260L19: perhaps add an equation to show the calculation; it could help with the standardization of the method. P8360L28: were the data normal? What was done otherwise? P8361L26-P8362L4: add references to specific figures (Fig 2 a, b, c. . .) P8363L12: remind us also which data subset you used for this. P8364L11-L16: higher BDOC in longer experiments is not surprising, how is this relevant? The paragraph is confusing, recast. P8365L23: not clear the point of the authors here. As the authors mention in the previous paragraph there is a big difference in aquatic BDOC between no permafrost and discontinuous and continuous permafrost. P8366L9: not really a decreasing trend, just a difference between large river and all other categories. Reword. Section 4.2.2: how well represented are headwater streams (zero or first order streams)? There is growing evidence of highly biodegradable DOM entering headwaters (e.g., Berggren and del Giorgio 2015 JGR biogeosciences), how would the lack of representation of low order streams affect these trends? P8366L21: should large rivers be included here? “. . .day for large streams, rivers and large rivers. . .” Section 4.2.3: it is not clear why there was a correlation with all the larger streams and rivers but not with the sites classified as “streams”. P8367L10: it is not clear what decoupling the authors are referring to. Section 4.4: Trying to measure BDOC in samples with initial low concentration of DOC (1 mg C/L or less) can be hard since samples start getting closer to the quantification limit of the OC analyzers. It would be useful to include a comment and potential solutions about this in this section.

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TECHNICAL CORRECTIONS Figures 3-5 are cited before Figure 2; P8360L5: “We added” instead of “We prepared”; P8361L24: correct, Fig 2a; P8361L25: correct, Fig 2c; Figure 2: Add Y-axis label; Figure 4: Add the number of data points in each boxplot.

Interactive comment on Biogeosciences Discuss., 12, 8353, 2015.

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