Biogeosciences Discuss., 10, C5299–C5302, 2013
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Biogeosciences
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Interactive comment on "Catchment-scale carbon exports across a subarctic landscape gradient" *by* R. Giesler et al.

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Received and published: 24 September 2013

Response to referee #2

We greatly appreciate the comments given by the reviewer and have addressed the questions raised by referee#2 below.

Major Comments:

There are no description of soils or parent geology and how this varies across the landscape. This is a glaring omission and goes to the heart of the DIC question. How interpretable are the results without this information?

We agree and have now added information about this in the manuscript.

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Interactive Discussion

Discussion Paper

I am unsure of the space-for-time argument. These sites have a very limited spatial extent and I see no clear argument as to how their spatial arrangement can be used as an argument for changing temporal patterns. Perhaps I am missing something here? The catchments do appear to have some trends with time, but that is not a space for- time argument. Without information on the disposition of permafrost / soils among these catchments, I do not see how this is possible. I would suggest the authors recast the manuscript to provide a more clear and convincing rationale.

We agree that this is a weak part that was not fully developed and have omitted the space-for-time argument in the manuscript.

The comparison of total flows (in m3) is meaningless, particularly when using this to compare with mass fluxes which are not independent from total flows. I think the authors can work to address and eliminate the large differences in area that clearly influence mass loads and attempt to focus on changes in patterns of normalized C fluxes.

Clearly DOC load is not an independent variable from annual runoff since load is a product of flow and concentration. However, both DOC concentration and load are essentially flow-independent because their dynamics are instead dominated by the annual renewal and depletion relative to the water travel time through the landscape (see Jantze et al., 2013). For DIC, the load is highly flow-dependent, due to the large characteristic weathering-dissolution time relative to the average subsurface water travel time to the stream. Still, we appreciate the motivation behind this comment. As such, taken together with the collective reviewers' call for more heed with regards to flow uncertainty, we have elected to downplay the hydrological parts of the study and, instead have re-worked the manuscript to more specifically stress the unique parts of our data. These are the dynamics of the C concentrations across the six streams and the long-term chemistry data. We feel this addresses the main concern of this comment.

There is insufficient information about the hydrology of this 'water year'. I note the large differences in total runoff (in mm) in Table 1. What is the cause of such large

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normalized variability? Does this go to the quality of the discharge data, which is not addressed particularly. I am quite nervous about the interpretation of the results based on the uncertainty in the flows.

The hydrology associated with this 'water year' can clearly be seen in Figure 6 as this figure displays the past 20 years of annual streamflow for Abiskojokken relative to the single year where observations were made across all 6 catchments. This year, thus, is a relatively low flow year based on the past 2 decades of observations. We have made this connection more clearly in the revised text.

Further, to put the "large" variability in total runoff into perspective, we can consider recent work (Lyon et al., 2012) on head water runoff variability in the nearby Krycklan boreal system where areas explored span a range from a few hectares up to about 70 km2. In that system under wet conditions, runoff can vary from about 0.5 mm/day up to 2.0 mm/day. Assuming an even distribution across the year (which is clearly not the case), the runoff from the 6 catchments in this study would range from 1.0 mm/day to 2.6 mm/day. While this range can in part be due to measurement errors or flow estimate uncertainties, this range of runoffs not impossible and could be attributed, for example, to differences in wetland or forest coverage influencing evapotranspiration rates or the influence of topography on hydrologic response (as is the case in Krycklan (Lyon et al., 2012). Of course, the goal of this present study is not explicitly to delve into the hydrologic response differences across these catchments; however, that is the focus of follow up work utilizing a synoptic sampling strategy similar to that outlined in Lyon et al. (2012). Based on preliminary analysis of that study where runoff is measure for various headwaters of Abiskojokken, the specific discharge can vary from less than 1 mm/day to more than 6 mm/day during low flow conditions.

As addressed in the response to the previous reviewer's comments, there is potential uncertainty in and error introduced by the flow estimates that does need better addressed. We have, thus, explored the potential impacts of this error on load estimates by simulating the potential influence on export of +/- 15% error. While flow estimation

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errors can range from less than 5% to more than 40% (Guerrero et al., 2012), we consider this as a conservative error estimate associated with flow measurements given previous work (Lyon et al., 2012). This allows for error bounds to be approximated on results and provides a framework to consider uncertainty. We have presented this in the study and adjusted the discussion (highlighting the potential for uncertainty) throughout. Guerrero, J-L, Westerberg, IK, Halldin, S, Xu, C-Y & Lundin, L-C 2012, 'Temporal variability in stage-discharge relationships'. Journal of Hydrology, vol 446., pp. 90-102

This work builds on considerable past research, but I am uncertain as to its unique contribution. Exploration of terrain characteristics and simple regression analysis with regressions really only particularly strong with the (not independent) mass fluxes and total runoff provides little new insight. The paper re-states ideas of flow paths, and there is some evidence based on the DIC-flow path length relationship, but again, if the authors are looking to provide improved insight into what causes the spatial variability among the catchments, it is not clear to me.

We have re-worked the manuscript to more specifically stress the unique parts of our data which we believe are the dynamics of the C concentrations across the six streams and the long-term chemistry data. This is also emphasized in the new title and abstract. We still believe that the exploration of the more hydrological parts is valuable for the manuscript but they are less emphasized (consistent with previous review comments).

Overall, while this data is of interest, I believe the authors should rework the manuscript to focus on the unique contributions of this work and how it informs our understanding of carbon and hydrology in discontinuous permafrost environments.

We agree with the comment and have thus re-worked the manuscript (see above).

Interactive comment on Biogeosciences Discuss., 10, 7953, 2013.