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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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Response to referee #1

We greatly appreciate the comments given by the reviewer and have addressed the questions raised by referee#1 below.

Overall questions

Overall I believe that the manuscript mis-interprets the relevance of the findings with respect to the importance of DIC. Hence it is incorrect to suggest that DIC is of greater importance in Arctic ecosystems because of this study. The catchments are low DOC systems compared to many Arctic or sub-arctic landscapes.

We agree that the results cannot be extrapolated to arctic ecosystems in general. We have modified the formulations in the text in cases were this is unclear such as L. 29 P7964 and L. 23-24 P7970. In order to put our results into a larger context we have also summarized literature data on DOC and DIC concentrations from permafrost affected areas in high-latitude areas. The comparison indicates that our DOC values are comparable to other studies but that our DIC values seem to be in the lower range.

Specific comments

7954 L1 – not clear what “hydrological cycling” means in this context?

We have changed the general wording “hydrological cycling” to “the flow of water through the landscape”

7954 L25 – what about CH4 release....

We agree and have included CH4 in the text.

7955 L17 – specifically active layer thickness?

Yes, as such, “increase in the active layer” has been changed to “increase in active layer thickness”

7958 L17-19 – sampling frequency varies from 2/3 times per week, to weekly, to monthly. Difficult therefore to calculate meaningful flux values.

We agree that calculations of flux values always are difficult. The sampling scheme for the concentration measurements are however based assumed variations in stream and water flow conditions. Thus, more intense samplings were done during the spring flood when we have a high variation and the least intense during winter baseflow conditions when the variation in concentrations and flow is relatively low. The sampling intensity can always be more intense but will also always be an issue of what is doable. The intensity of sampling that we use is comparable to many other studies and we think that calculating the flux values adds valuable information although there is uncertainty introduced in the calculations due to the sampling intensity. We do, however, have daily DIC values for stream 5 taken by an ISCO sampler for about 3 1/2 months (21 May to 7 September). A comparison of the DIC flux from the grab sampling and the ISCO sampling gives flux estimate that differs less than 2%. We have thus added this data as a figure and additional text to illustrate that the sampling intensity is not the critical factor for the flux estimates.

7959 L3-16 – a lot of assumptions were used to estimate flow. Have any of these been tested (e.g. area weighting)? How good were the discharge rating equations?

Yes, clearly there are several assumptions in place in order to estimate flow records across all 6 catchments. These assumptions, while necessary due to the difficulties faced when working across cold regions and northern landscapes, add uncertainty to flow estimates and subsequent C flux estimates. We have considered the potential impacts of these uncertainties on flux estimates (and our interpretation of the results) in response to the other reviewer comments (see response to reviewer #2 and #3) and clarified the text accordingly.

We have good confidence in the flows obtained for stream 6 as these were collected through the national flow observation network in Sweden (by SMHI). This confidence is the main motivation for the scaling used to "fill in the blanks" with regards to winter flow measures. Rating curves for the remaining streams were good (relative to site conditions). All curves were assumed to be power laws (as is typical for natural streams) based on 4 to 6 observed flows per stream. Fits for the power law rating curves to the flow observations had R2 values between 0.86 and 0.99 (this has been highlighted in the text).

Introduction to the manuscript is ok, although rather too many processes and linkages are described and it would benefit from being more closely linked to the study aims.

We have re-written the introduction in response to this and other comments. There is now a better focus and clear link to the study aims.

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Some relevant literature on spring snowmelt C and water fluxes is omitted. E.g. papers by Buffam 2008 Sc Total Environ , Dyson et al Biogeochem 2010, Dinsmore et al 2011 Sci Total Environ.

We are familiar with the papers above and that they are all related to boreal ecosystems in Sweden and Finland. We have deliberately tried to focus on literature from ecosystems more related to our study area i.e. sub-arctic/arctic environments although we are aware of the large extent of studies that have been performed on similar questions in the boreal region.

Study sites– no information is provided on soils or geology. This is a major omission since one of the focuses of this manuscript is DIC.

We agree and have added information on the soils and geology of the sites.

What was the % coverage of organic soils in the various study catchments? – again relevant because this is a manuscript on DOC.

We have been more specific in the site description giving information on the percentage coverage of organic soils and also stressed more explicit that the catchments are all dominated by upland soils. We have also given information on the humus layer depth found in the area.

7963 L1-9 – In my view $P<0.10$ is not a significant relationship.

We agree and have reformulated the text so that only values of $p=0.05$ are considered as significant.

Annual DOC load and annual runoff are not independent variables. Why not explore relationships between discharge, DOC and DIC??

Yes, DOC load is not an independent variable from 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 source

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renewal and depletion (see Jantze et al., 2013). Specifically Jantze et al. (2013) explore these relationships and show that the flow-independence is the result of the small characteristic DOC respiration-dissolution time scale, in the range of 1 year for Abiskojokken, relative to the average travel time of water through the subsurface to the stream. For DIC, the load is highly flow-dependent, due to the large characteristic weathering-dissolution time, much larger than 1 yr, relative to the average subsurface water travel time to the stream. This rate relation keeps the DIC concentration essentially flow-independent, and thereby less fluctuating in time than the DIC load. We have made this reasoning clearer in the revision of the manuscript. In addition, we have elected to shift focus (in response to several other reviewer comments) in this revision to the dynamics of the C concentrations across the six streams and the long-term chemistry data. (Jantze, E.J., Lyon, S.W., Destouni, G. (2013), Subsurface release and transport of dissolved carbon in a discontinuous permafrost region, Hydrology and Earth Systems Sciences, in press.)

7964 L18-20 – I disagree. Both DIC and DOC fluxes are very low in these catchments, even though the % DIC contribution to the total C flux is high.

We agree that the fluxes of DOC are low but still comparable to other sites and even higher to some other sites in permafrost affected areas. We have also extended the text somewhat in order to compare our concentration values across permafrost-affected catchments. We also compare our results with a rather nearby forested catchment. Both average DIC values and the DIC export are actually higher than flux values found in extensively studied Krycklan catchment in the boreal forest some 600 km south of Abisko (Wallin et al., 2010, JGR, G02014).

7965 L10 – the author's interpretation of a geogenic origin may be correct, but no information at all is given in the manuscript on geology or soils. Hence this assertion cannot be evaluated by the reader.

We agree that using geogenic is not fully correct since we cannot separate between

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respiratory and bedrock derived DIC. We have reformulated this accordingly.

7969 L20-25 - I think the authors are again over-exaggerating the importance of the DIC flux in these systems. The overall DIC flux values are small.

We do not fully agree since our literature survey indicates that our DIC concentrations and fluxes may be in the lower range of does values found in Arctic ecosystems.

Section 4.4 is very general and there is little or no evidence presented to support these far-reaching conclusions.

We have partly re-written and shortened the this paragraph.

Table 1: I would have preferred to have seen the data area-weighted since there are huge differences in catchment area been the 6 sites.

By definition of runoff used here (which is equivalent to specific discharge), total runoff gives an area normalized characteristic. Further, max/min flow ratio is a scaled value representing dynamic range and, thus, largely independent of catchment area (compare to Total Flow).

Table 2 should come before Table 1.

We agree and have changed this.

Table 2: Is this "elevation" of the sample site? Is it mean "slope"? What is "flow pathway length"?. These terms are all rather meaningless unless they are accurately defined.

We have clarified this in the text. Elevation and slope are the mean values of the catchment. Flow pathway length is defined as the distance from any given point in the catchment to the outlet of the catchment following the flow pathway defined by the steepest surface (topographic) gradient.

Table 3: Presumably units are g C m-2 yr-1. These are all low C fluxes values compared to many boreal/northern systems.

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We have changed the unit throughout the manuscript. See above regarding flux values.

Suggest use L instead of dm-3.

We have changed this according to the suggestion.

Table 4: Pretty meaningless table – again I don't think it worthwhile expressing $P<0.10$ as a significant relationship.

We have removed this table and instead presented the results in the text. We agree that the way we expressed is not good and have changed it accordingly only using $P<0.05$ as the significance level.

Table 6: Omit as the relationships are predictable. Big question would seem to be - what controls DIC and DOC, not what the inter-relationship between Ca and Na are. The former is relevant to this manuscript, the latter not relevant.

We have followed the suggestion and removed table 6.

Fig 3: Need to know whether the trends are significant or not.

This is given in the figure but obviously not clearly enough. We have added p-values to the figure for clarity.

Fig 4: Not sure what the authors are trying to show in this diagram?

We were trying to show that there is a sustained increasing trend in winter DIC which is indicative of deeper flow pathways and thawing permafrost. We have, however, omitted the figure and instead added the information in the text.

Fig 5: These Si/DIC inter-correlations would be expected. Why not add regression curves and R2 values?

We have followed the suggestion and added regression curves, r2 and p-values.

Fig 6: Mass flux and water yield will almost always show a +ve correlation – again the graph is what you would expect. Not sure what this Figure adds to the manuscript?

The main addition that this figure adds to the current study is the highly linear relationship between DIC and discharge relative to the non-linear relationship between DOC and discharge (across all discharges).While some spatial variability likely exists also in weathering rates, for instance between higher altitudes and lower valley bottoms (e.g. Allen et al., 2001; Dixon et al., 2008), high correlation between DIC mass flux and water discharge (i.e. Fig. 6) has also been found in other studies, for diffuse solute inputs from geogenic sources, or for inputs from subsurface sources (Godsey et al., 2009; Basu et al., 2010). The additional dynamic DOC load effects over the range of annual flows from the various sites (particularly comparing the long-term Abiskojokken data to the 6 catchments) can be partly attributed, thus, to the variable nature of decomposition rates for organic material and the associated release rate for DOC, which are coupled to numerous factors such as temperature, vegetation type and soil moisture distribution (Clark et al., 2005; Hobbie et al., 2000; Lyon et al., 2011; Seibert et al., 2009; Sjogersten and Wookey, 2004; Winterdahl et al., 2011). The interpretation of dominant discharge-independent DOC dynamics is consistent with several studies highlighting explicitly the connection between sub-annual variations in soil water DOC (akin to the DOC release time scale) and stream concentrations (e.g. Seibert et al., 2009; Clark et al., 2005). See Jantze et al. (2013) and discussion therein.

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