

Comment on bg-2022-94

Anonymous Referee #1

Referee comment on "Lateral carbon export has low impact on the net ecosystem carbon balance of a polygonal tundra catchment" by Lutz Beckebanze et al., Biogeosciences Discuss., <https://doi.org/10.5194/bg-2022-94-RC1>, 2022

Review on the manuscript titled 'Lateral carbon export has low impact on the net ecosystem carbon balance of a polygonal tundra catchment', submitted for publication in Biogeosciences by Beckebanze et al. in May 2022.

The presented study evaluates the contribution of lateral carbon exchange to the net ecosystem carbon balance of a small Arctic polygonal tundra catchment underlain by permafrost. Monitoring vertical and lateral flux rates over a period of about 3 months in summer 2014 reveals the temporal development of $f\text{CO}_2$, $f\text{CH}_4$, DOC and DIC, and how the relative contribution of each fraction changes over the course of the growing season. These datasets demonstrate that the lateral export pathways only make up about 2% of the vertical budget. Even though they may be important at the beginning of the season, or for shorter term studies, the authors thus conclude that lateral components can be neglected when targeting long-term carbon budgets.

The manuscript is well written and structured, and all conclusions are fully supported by the result material presented. The topic is certainly relevant, since as the authors correctly claim in their text lateral carbon export is considered to be a major component of the net carbon budget in certain Arctic ecosystems, but comprehensive studies that aim at assessing it are currently still lacking. Even though the presented material is a limited-scale case study, and extrapolation of the findings may be difficult, this is definitely a valuable contribution for this field of research. Besides some minor comments, which are listed further below, I have a few comments that should be addressed before the paper can be accepted for publication:

1.) You show in your results that the DOC flux is comparatively high in the first few days of June. This observation is also discussed at length in the discussion section. Now, from your description it sounded like the area was flooded by the Lena in the days before the start of the study period. This suggests that the standing water on the site, with high DOC, was previously laterally imported into the catchment. So if this carbon was not locally produced, in what way should it still be considered for the NECB? This should at

least be considered in the discussion, and maybe you should tone down some of your statements regarding the elevated role of DOC early in the season.

2.) In section 4.1, you discuss the relationship between runoff, DOC/DIC, and precipitation. I think this discussion is not to the point. Since the source of DOC is obviously not the rain water, why should higher PRCP lead to higher DOC fluxes? Your results suggest that, besides the initial period following the flooding in spring, DOC is leached from the thawing active layer, at a rate that is likely to be rather correlated to the increase in thaw depth than to PRCP. If this is the case, more PRCP would rather dilute the DOC, and thus influence the outgoing concentration, but not the exported DOC. Please adjust the discussion accordingly.

Altogether, I consider my requested modifications as minor corrections. The manuscript is a valuable contribution to Biogeosciences, and I therefore recommend to accept it for publications after these small changes have been worked in.

MINOR COMMENTS:

l. 16f: I suggest to remove the last sentence from the abstract

l. 35f: I suggest to move the last sentence of paragraph 1 further down, to be the first sentence of paragraph 2.

l. 55: a better section header would be 'catchment characteristics'

l.77f: this sentence made me wonder how accurate a reference measurement with a bucket and a stop watch can actually be. So is this indeed helpful to correct the actual measurements? Maybe you can include this in the appendix where you treat the uncertainties of this part of the dataset in more detail.

l.99f: the implication of this gap-filling approach on your results should be treated as part of the discussion.

Section 2.5: please provide information on how many samples were actually analyzed to produce the DOC results for this study

Section 2.9: I found the description of the uncertainty treatment a bit vague in places. I therefore suggest to move some of the more detailed descriptions on uncertainty assessment from the appendix into the main text.

L.154: you mention the 'vertical water balance', but include water runoff in the list of components ..??

Figure 2: is water discharge the total discharge for the catchment, or the discharge normalized per unit area? If the latter, please adjust the units accordingly.

Figure 4: even though the background pictures are nice generally, the chosen format makes the interpretation of the shown material rather difficult. I suggest to remove background figures altogether here (it can be left in Fig.3), and change this into a single panel figure with a line/bar chart showing the temporal development of the different flux components. Since you give the monthly values already in the table in the appendix, such a figure could be improved by a higher temporal resolution, e.g. weekly averages.

II.251ff: It would be good to mention also briefly in the methods section that you decided to neglect POC for this reason.

II.254ff: this paragraph is kind of detached from the rest of the text, and particularly it is not connected to your own results.