

Biogeosciences Discuss., referee comment RC2
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Comment on bg-2022-69

Anonymous Referee #2

Referee comment on "Meteorological responses of carbon dioxide and methane fluxes in the terrestrial and aquatic ecosystems of a subarctic landscape" by Lauri Heiskanen et al., Biogeosciences Discuss., <https://doi.org/10.5194/bg-2022-69-RC2>, 2022

In this submission, the authors present an interesting assessment of the carbon fluxes (CO₂ and CH₄) in a subarctic region. The study area located in northern Finland covers 7 km² consisting of five different ecosystem types. The focus of the study was to evaluate the temporal variability of the ecosystem-atmosphere carbon exchange using two years of measurements (2017-2019). The authors found that the different ecosystems had significantly different responses to the weather conditions. Major differences were found in the behavior of the ecosystems during four particular periods: 1) a rainy growing season in 2017, 2) the warmer-than average early growing season in 2018, 3) a heatwave and drought in the summer of 2018, and 4) a cold spell in autumn 2018. Heiskanen et al. found the study area to be a net sink of CO₂ and a source of CH₄ during the study period. However, the uncertainties are large.

The manuscript can contribute to the understanding of the carbon cycle by evaluating the role of different ecosystem elements and their response to forcing factors. Increasing our understanding of the ecosystems' response to the weather conditions is particularly relevant in the high latitudes, as these regions are highly vulnerable to the changing climate.

I have two major concerns about this work:

- The analysis presented here is based on a data set including up to 90% of data that has been gap-filled (ca 90% of the data from the pine forest and ca 60% of the fen CO₂ fluxes were gap-filled, as well as ca 70% of the CH₄ fluxes). These data was then used to describe the temporal variability of the carbon exchange and to extrapolate the fluxes to a landscape level. In my opinion, gap-filling procedures are useful when the gaps are small and the uncertainties can be accounted for, which is not the case here.
- My second concern is regarding the extrapolation to a landscape level. The upscaling was made using the gap-filled data set from the terrestrial ecosystems and only a few days of measurements from the lake ecosystems. I doubt that these data can really

capture the temporal variability and, most certainly I do not think it can accurately represent the fluxes at a landscape level.

Other comments:

L85: "we address questions how" --> "we address questions on (or questions about, or similar) how..." (In any case, is this sentence really necessary? The main questions addressed are stated as 1) and 2) in the following lines).

L103-L104: For clarity, I would recommend using the same terminology in the text and in the Table. Consider throughout the manuscript.

L107: "high string formations can remain" --> " high string formations that can remain"

L124: maybe I am missing it, but I cannot find the data that leads to this 10% contribution of the mosses and lichens to the total above ground biomass in table A1. Please check the numbers and clarify.

L162-L164: Please give information about the impact of each screening criterion, i.e. percentage of rejection of each criterion, and the implications of rejecting these data in the final analysis.

L181: what was the measurement frequency of the Los Gatos gas analyzer? In general, details about CH₄ flux calculations are missing. Please clarify.

L184: Please provide a more detailed explanation of what the chamber measurements are and how were they performed, for the non-expert readers.

L221: "glasses" --> "gases"

L235: please define the acronyms EC, TC, and FI.

Sect. 2.2: Might be worth adding a table summarizing all the flux measurements, including location, gas, technique, period/dates of measurement, etc. As it is now, it is hard to keep track of which measurements were made, where and for how long.

L300: The authors show throughout the manuscript that the years 2017 and 2018 were significantly different. These differences were mainly caused by the meteorological conditions affecting the different ecosystems, including the lakes. I think a good explanation is needed about why the annual balances at the OS lake can be extrapolated from year 2017 to 2018, regardless of the differences observed in all the other ecosystems.

L324: Please define F_R , F_{GPP} , etc. in their first appearance in the text.

L410: Are these results really statistically different? This might be clear from the tables and figures in the appendix but, in my opinion, Figure 8 (and later on Fig. 9, 11-13) need to show some measure of uncertainty.

L511-514: I understand that the convective processes (turnover mixing) can be the driver enhancing CO₂ fluxes. However, this physical mechanism has a limited effect in such shallow water bodies. Additionally, I would expect the vertical mixing to be strongest right after the ice melt (i.e. May according to Table 4). As the time passes, stronger stratification typical of the summer months would be expected again. The reason why the authors see an enhancement of the CO₂ fluxes but not on CH₄ fluxes has to do, in my opinion, with the different production mechanisms and accumulation of the gases (which might then vertically transported by the turnover mixing). Further discussion is needed in this regard.

L520: Was it possible to detect any diurnal variability with the measurements presented in this study? If yes, it might be relevant to discuss.

Sect. 3.3. Please discuss the role of the lakes in the CO₂ fluxes at a landscape scale (i.e. in L631)

L662: I agree on the statement "C exchange...on the lake it depended on the amount of available carbon in the sediment and the length of the ice-free period". However, I do not think this is really discussed in Sect. 3.2.3. Please revise.

L682: "contribute positively the landscape resilience" --> "contribute positively to the landscape resilience"

