Comment on egusphere-2022-485
Anonymous Referee #1

Referee comment on "Carbon emission and export from Ket River, western Siberia" by Artem G. Lim et al., EGUsphere, https://doi.org/10.5194/egusphere-2022-485-RC1, 2022

Review of ‘Carbon emission and export from Ket River, western Siberia’ by Lim et al.

General comment

In this work, Lim and colleagues reported the spatial and seasonal dynamics of C export and emissions from the Ket River mainstem and major tributaries by combining continuous in-situ measurements and discrete sampling. Although high latitude regions are an important component of the global carbon cycle due to their large carbon stocks, carbon emissions and export from permafrost-affected regions, especially those in Russia, are poorly studied due to logistical constraints and inaccessibility. In view of the changing climate and thawing permafrost, this study is timely important in quantitatively assessing the spatial and seasonal patterns of dissolved carbon export and emissions in this permafrost-affected river basin and thus provides important insights into future riverine carbon cycling. This research work fits well with the scope of the journal Biogeosciences. But there are several major issues to be properly addressed during the revision stage.

My first major comment is on the observed stable behavior of CO2 in the Ket River basin. The authors have tried to explain the stable behavior of the CO2 dynamics (pCO2 and Fco2) by relating them to various physiochemical parameters. But it seems none of the physiochemical parameters is sufficiently strong to drive the pattern although they show pronounced spatial and seasonal variations, as shown in Table 1 and Figs 2 and 3. This is
contrary to studies in other climates/regions. I am wondering whether these potential drivers are working in different (opposing) directions and have counteracted each other. The authors may need to think about this seriously, and re-examine the cause-effect relationships. Many of the current discussion statements are lack of evidence and speculative.

My second major comment is on the calculation of the annual flux of CO2 emission and lateral C export. With very limited C sampling results covering a short period (Fig 1b), the annual flux estimates are prone to large errors. For example, CO2 emissions during ice melting periods are exceptionally strong after a long period of CO2 accumulation. But such emissions are not included or accounted for in the estimation. Likewise, the lateral fluxes based on monthly average discharge are likely with huge uncertainty. E.g., the strong DIC concentration differences between the flood and baseflow (Table 2) suggest significant dilution effect and changing flow paths.

Overall, this manuscript was well written, but the structure could be further improved by moving the discussion statements from the Results section to the Discussion section. A further language editing is also needed before its resubmission.

**Specific comments (with line number):**

L42-43: 100 to 150 times?

L64: even for these regions, the estimates are still with great uncertainty.

L80: delete ‘remain’

L95: essentially speaking, the two sampling campaigns represent the two extremes (highest flow and lowest flow, respectively). A question then is whether it is reasonable to use these extremes for annual flux estimation (emission and downstream export)?
L108: what is hydrocarbon exploration? I don’t understand this.

L113: delete ‘.’ after -0.6. also, references are needed to this paragraph describing the background information.

L119: Have the authors finished the cruise (1300 km in total) and sampling within 3 days? Sounds an impossible task.

L125-126: what’s the sampling frequency for the day/night circle?

L152: change ‘location’ to ‘locations’. Also, it would be helpful to briefly describe the measurement procedures, instead of referring readers to published papers for details. These papers might not be accessible to some of the journal readers.

L154: what are the standard approaches? Please clarify and provide details.

L156: For flowing streams and rivers, the major driver of the gas transfer velocity is flow velocity, not wind speed.

L181: The DIC concentrations in base flow is even higher than the DOC concentrations (table 1). But here the contribution of carbonate C to total C is only 0.3%. this looks problematic. please double check.

L195: what is the spatial resolution of the biomass and soil OC content datasets?

L219: a lack of systematic change? Note the pCO2 changed by a factor of 2 when tributaries with high CO2 concentrations join the mainstem.

L241-247: these are not results, move them to the discussion section.

L297-298: would the precipitation quickly infiltrate into soil and become groundwater?
as the measurements were performed at the flood peak, this may have caused overestimation.

how were these %s determined?

why the co2 flux pattern is different from the pco2 pattern?

Another possible reason is because the measurements were actually not performed in the true headwater streams. All the sites, include the tributary ones, are located along the mainstem and not in the headwater region as shown in Fig. 1.

If allochthonous C inputs are the dominant source, pCO2 should have a clear relationship with distance to terrestrial C inputs, i.e., there should be higher pCO2 in tributaries than in the mainstem.

change ‘at’ to ‘in’.

For these comparisons (similarity and differences), it is quite difficult to follow. Putting them into a table may help. Also, the authors need to make a critical and comprehensive discussion, rather than a general sentence on the possible reasons. This is quite speculative.

This ignorance may have caused great errors to the annual estimates. Emissions of CO2 during ice melting is exceptionally strong and make a disproportionate contribution to the annual flux estimate.

unclear description of the Ob River.

change ‘thus’ to ‘this’

any evidence to support this argument?

Fig 2: for b&c, change the x-axis to 0-900 for consistency and easy understanding.
Fig 4e: much higher pco2 during the daytime than the nighttime? Why?

Fig 5d: very low r2, what is the p-value?