



EGUsphere, referee comment RC3  
<https://doi.org/10.5194/egusphere-2022-485-RC3>, 2022  
© Author(s) 2022. This work is distributed under  
the Creative Commons Attribution 4.0 License.

## **Comment on egusphere-2022-485**

Anonymous Referee #3

---

Referee comment on "Carbon emission and export from the Ket River, western Siberia" by Artem G. Lim et al., EGU sphere, <https://doi.org/10.5194/egusphere-2022-485-RC3>, 2022

---

### 1) GENERAL COMMENTS

Lim et al. report a high-quality data-set of CO<sub>2</sub> and CH<sub>4</sub> concentration measurements in the Ket River in Siberia obtained during high-water and low-water. This is a very useful contribution to on-going efforts to collect data to better evaluate the carbon emissions from inland waters because the studied river drains a remote and nearly undisturbed (pristine) watershed dominated by peat bog and taiga forest. Unfortunately, the analysis is (in my opinion) not well structured and the authors might want to spend some extra time on thinking through how to present and analyze the data, and profoundly re-structure the paper and streamline the present content.

For instance, the authors computed the fluxes of CO<sub>2</sub> with a gas transfer velocity parameterization for lakes; this gave (unsurprisingly) very different results from the fluxes of CO<sub>2</sub> measured with floating chambers. This was predictable and in my opinion not very useful, just distracting. Regarding formal aspects, the authors should spend some extra time producing high quality figures. Figure 2 is extremely confusing and does a very poor job at presenting this data-set that required a lot of effort to acquire. Figure 3 shows some nice patterns of pCO<sub>2</sub> and CH<sub>4</sub> concentration in terms of seasonal variations (high-water vs low-water) as well as in terms of stream size (main-stem vs tributaries). A more straightforward and attractive presentation and discussion could be built on these simple patterns. Instead, this nice and potentially interesting information is diluted in a lot of rather unnecessary elements such as computations of fluxes with inadequate gas transfer parameterizations and correlations with not very useful variables such as total bacterial counts (see comments below).

## 2) MAIN COMMENTS

L37 and L218: I'm unsure that the term "continuous" applies to measurements of CO<sub>2</sub> to this study. My perception of "continuous measurements" is that water is continuously pumped through an equilibrator system connected to a CO<sub>2</sub> detector (or equivalent setup) and then the data are logged at regular intervals (1 min or less) (Abril et al. 2014; Crawford et al. 2016b; 2017 Borges et al. 2019). This means that the measurement of CO<sub>2</sub> is not interrupted for long periods (and runs for a few hours to a few days) while the boat is sailing. The authors made discrete samples with the boat stopped at a given spot. Albeit they made numerous measurements this should qualify as discrete sampling and not continuous. This is not just a semantic issue; the authors made 764 pCO<sub>2</sub> measurements over the distance of the boat route (834 km) as stated L 218. This roughly corresponds to one measurement every 1 km. This is still quite coarse to describe extremely dynamic river systems. As an example, Borges et al. (2019) showed very marked cross-channel gradients of CO<sub>2</sub> in the mainstem Congo River, corresponding to a spatial scale of the order of 1 km (using what truly qualifies as "continuous").

L150: The authors measured CO<sub>2</sub> fluxes between water and air with floating chambers. Lorke et al. (2015) have shown that anchored chambers enhance turbulence under the chambers and artificially enhance fluxes, thus providing erroneous estimates. Please specify if the chambers used in the present study were anchored or free-drifting. If the chambers were anchored then the data should be used with extreme caution, especially for the flood period when presumably the flow was higher. In my opinion, these chamber measurements are not necessary, and fluxes should be computed from gas transfer velocity using an adequate parametrization applied to spatial data, please refer to Liu et al. (2022).

L154: The authors also computed the CO<sub>2</sub> fluxes between water and air from CO<sub>2</sub> concentrations and the gas transfer velocity. The cited references (Guérin, et al., 2007; Wanninkhof, 1992; Cole and Caraco, 1998) provide parameterizations for lakes that are inadequate for computing the gas transfer velocity in running waters. The authors provide these 3 references, although it was unclear to me which one was actually used in the computations. The gas transfer velocity in streams and rivers can be derived from stream flow and stream slope, that in turn can be derived from spatial data; please refer

to Liu et al. (2022).

L 216: The authors state that there are no spatial variations in CO<sub>2</sub>. I suggest to mention here that CO<sub>2</sub> in tributaries was higher than in the main stem. This corresponds to a “systematic” pattern of variation. Also, I suggest that the authors extract the Strahler order of the sampled streams and rivers and analyze if there are differences by stream size. It is quite frequent that lower order streams show higher CO<sub>2</sub> values and higher order (Butman and Raymond 2011; Borges et al. 2019), although not always necessarily the case (Borges et al. 2018). Stream size could also be analyzed in terms of catchment area, in addition to Strahler order. Stream size can be used also for upscaling concentrations and fluxes, refer for example to Borges et al. (2019).

### 3) SPECIFIC COMMENTS

L 34 : I suggest to define « medium-size rivers »

L 34 : I suggest to remove « poorly » or replace by « largely » but « poorly unknown » is awkward.

L 40: I suggest to mention the months-years of sampling

L40: I suggest to replace "CO<sub>2</sub> concentration" by partial pressure of CO<sub>2</sub>.

L40-41: I suggest to mention the differences in pCO<sub>2</sub> between base flow and flood period.

L41-43: I suggest to provide the range of the CH<sub>4</sub> concentrations values rather than the ratio to CO<sub>2</sub>.

L 47 : I suggest to specify if this is this spatial or temporal "variability" ? or both ?

L 49 : The hypothesis of lower path soil-water CO<sub>2</sub> inputs during summer is based on what ? During summer-time numerous processes contribute to increase CO<sub>2</sub> in rivers compared winter such as higher temperature stimulating microbial metabolism, longer residence time and lower gas transfer velocity (lower river flow), in addition to changes in flow paths of soil-water flows (Borges et al. 2018).

L51: "lateral" usually refers to exchange between river and riparian zones (e.g. floodplains). Term "downstream C export" might be more adequate. I suggest to specify if this downstream C export refers to inorganic, organic or total carbon and if dissolved or dissolved+particulate.

L67: define abbreviation pCO<sub>2</sub>

L69: This statement does not reflect current state of CO<sub>2</sub> studies in rivers. There is a fast growing very large amount of studies reporting directly measured CO<sub>2</sub> measurements either discretely (Alin et al. 2011; Borges et al. 2015; Amaral et al. 2018; 2022; Leng et al. 2022), continuously at fixed sites (Crawford et al. 2016a, Schneider et al. 2020; Gómez-Gener et al. 2021), and continuously underway (Abril et al. 2014; Crawford et al. 2016b; 2017; Borges et al. 2019). And this is also the case for studies in “under-represented or ignored regions” as stated, and for more than a decade (Alin et al. 2011).

L 71-72: This is correct and there are some studies available (Abril et al. 2014; Crawford et al. 2016b; 2017 Borges et al. 2019). It could be useful to briefly mention if there is and what is the added value to make continuous “regional high spatial resolution measurements” of CO<sub>2</sub> compared to discrete measurements, based on past published papers.

L73-74: Please clarify what do you mean by “High latitude regions are important”. With respect to total CO<sub>2</sub> emissions at global scale, rivers in high latitude regions are not important according to the study of Liu et al. (2022) who show that “tropical rivers are responsible for 57% of the global emission, more than temperate and Arctic regions combined (30 and 13%, respectively)”.

L113: there's some sort of typo here “ 0.6..-0.9 °C”

L 148 : For a journal such as Biogeosciences I think it is insufficient to refer to other papers for basic methodological information. I suggest to provide details on the gas used

for the headspace, on the calibration gases, on the detection limit, precision and accuracy. It could also be useful to mention the typical time interval between sampling and analysis.

L129-139: Similarly for CO<sub>2</sub> please provide information on precision. Is the stated accuracy given by the manufacturer or was this determined by the authors? Also specify how the Vaisala instrument was calibrated. Did you trust the factory calibration or did you carry out calibration in the lab? Was the probe checked for signal drift before and after the cruise against standards ? Did you measure atmospheric CO<sub>2</sub> with the Vaisala probe during the cruises as a check of good functioning ?

L 144 : how was the water sampled and transferred to the serum vials ? With some sort of sampling bottle ? Niskin or equivalent ?

L165: I suggest to define the "NIST" abbreviation

L189-193: Please specify if the land cover data correspond to the whole catchment area upstream of the sampling point or if this corresponds to the riparian vegetation just adjacent to the sampling point.

L 216 : I suggest to remove word « emission ». You cannot pre-suppose an emission, some rivers on some occasions can be sinks of CO<sub>2</sub> (Crawford et al. 2016b).

L 246: I'm not sure this "warning" is useful since the authors used a parameterization for lakes, and this was not a very good idea to start with.

L 295 : It's quite unusual to look into the effect of catchment lithology on fluvial CO<sub>2</sub> and CH<sub>4</sub> concentrations. Lithology will affect the HCO<sub>3</sub><sup>-</sup> content and DIC content, but with little direct impact on CO<sub>2</sub> levels and certainly not on CH<sub>4</sub>. I suggest the authors restrict this analysis to DIC (or remove altogether this analysis that is just a distraction).

L297-298: This is also quite unusual. I would envisage seasonal variations precipitation to explain seasonal variations of CO<sub>2</sub>, but not spatial variations during a given period, in this case base flow. Correlation does not necessary imply causation, some correlations are spurious or indirect. There's a possibility that this is relate to stream size, as precipitation at catchment scale, also captures catchment surface area in an area of relatively homogeneous precipitation. I suggest to remove altogether this analysis that is just a distraction.

L 346: The paper of Gómez-Gener et al. (2021) gives a reasonably good account of diel variations of pCO<sub>2</sub> in temperate rivers but reports measurement in an extremely limited number of sites in tropical rivers. So this study does not allow to make generalizations on "tropical rivers". There are other studies in tropical rivers that have shown that diel variations of CO<sub>2</sub> are undetectable such as the Congo (Borges et al. 2019) because aquatic pelagic primary production is low (Descy et al. 2018) due to strong light attenuation the water column by DOM.

L363-367: This is a reasonable explanation. However, "homogeneous landscape" and "strong allochthonous sources of organic carbon" can still lead to variations of CO<sub>2</sub> per stream size, with small systems showing higher values than large systems as predicted conceptually (Hotchkiss et al. 2015) and verified at basin-scale (e.g. Borges et al. 2019).

L 381: I suggest to remove the word "interesting". This is self-evaluation, let the readers decide what's interesting. Same applies to word "notable" L 361.

L 477-515: Section "Concluding remarks" provides a summary of the paper and thus duplicates the content of abstract. This section could be removed or streamlined.

In Figure 2, I suggest to show the « continuous » pCO<sub>2</sub> measurements data points as a discrete symbols (dots) rather than a line.

Figure 2 is incredibly confusing and in my opinion undermines the large sampling effort. I suggest to make separate figures for pCO<sub>2</sub> and FCO<sub>2</sub> and not try to show all of the data together in single plot. Please provide a graphical representation of the pCO<sub>2</sub> during the flood period. If I understand correctly the symbols, the blue diamonds in plot A) are for the FCO<sub>2</sub> and not pCO<sub>2</sub> in the tributaries. But Table 1 shows that pCO<sub>2</sub> was measured in the tributaries during the flood period. I also suggest to remove the "continuous FCO<sub>2</sub>". The term is misleading since it's FCO<sub>2</sub> computed from "continuous" pCO<sub>2</sub>. Also since the figure mixes FCO<sub>2</sub> measured with the chambers and computed with a gas transfer velocity and that the values are very different, the impression given by the figure is very confusing.

Figure 5 : pCO<sub>2</sub> should be in the Y-axis and the potential predictors/descriptors (SUVA, land cover) in the X-axis.

The correlation of pCO<sub>2</sub> and TBC in Fig. 5B is weak and not very informative. The TBC

only informs on the presence of microbes and not their activity. Also, if CO<sub>2</sub> comes from soil-water as suggested by the authors then it is not produced in-stream and we should not expect a correlation with TBC. This cannot go both ways.

#### 4) REFERENCES

Abril, G., Martinez, J.-M., Artigas, L.F., Moreira-Turcq, P., Benedetti, M.F., Vidal, L., Meziane, T., Kim, J.-H., Bernardes, M.C., Savoye, N., Deborde, J., Albéric, P., Souza, M.F.L., Souza, E.L., and Roland, F. (2014) Amazon river carbon dioxide outgassing fuelled by wetlands, *Nature*, 505, 395-398, <https://doi.org/10.1038/nature12797>

Alin, S. R., M. F. F. L. Rasera, C. I. Salimon, J. E. Richey, G. W. Holtgrieve, A. V. Krusche, and A. Snidvongs (2011), Physical controls on carbon dioxide transfer velocity and flux in low- $\Delta$  gradient river systems and implications for regional carbon budgets, *J. Geophys. Res.*, 116, G01009, <https://doi.org/10.1029/2010JG001398>.

Amaral JHF, AV Borges, JM Melack, H Sarmento, PM Barbosa, D Kasper, ML Melo, D de Fex Wolf, J S da Silva, BR Forsberg (2018) Influence of plankton metabolism and mixing depth on CO<sub>2</sub> dynamics in an Amazon floodplain lake, *Sci. Total Env.* 630, 1381-1393, <https://doi.org/10.1016/j.scitotenv.2018.02.331>

Amaral JHF, JM Melack, PM Barbosa, AV Borges, D Kasper, AC Cortes, W Zhou, S MacIntyre & BR Forsberg (2022) Inundation, hydrodynamics and vegetation influences carbon dioxide concentrations in Amazon floodplain lakes, *Ecosystem*, 25(4), 911-930, <https://doi.org/10.1007/s10021-021-00692-y>

Borges A.V., F. Darchambeau, T. Lambert, S. Bouillon, C. Morana, S. Brouyère, V. Hakoun, A. Jurado, H-C Tseng, J.-P. Descy, F.A.E. Roland (2018) Effects of agricultural land use on fluvial carbon dioxide, methane and nitrous oxide concentrations in a large European river, the Meuse (Belgium), *Science of the Total Environment*, 610-611, 342-355, <https://doi.org/10.1016/j.scitotenv.2017.08.047>

Borges AV, Darchambeau F, Teodoru CR, Marwick TR, Tamooch F, Geeraert N, Omengo FO, Guérin F, Lambert T, Morana C, Okuku E & Bouillon S (2015) Globally significant greenhouse gas emissions from African inland waters, *Nature Geoscience*, 8, 637-642, <https://doi.org/10.1038/NGEO2486>

Butman D., P.A.Raymond (2011) Significant efflux of carbon dioxide from streams and rivers in the United States. *Nat. Geosci.* 4, 839–842, <https://doi.org/10.1038/NGEO1294>

Crawford J.T., D.E. Butman, L.C. Loken, P. Stadler, C. Kuhn, R.G. Striegl (2017): Spatial variability of CO<sub>2</sub> concentrations and biogeochemistry in the Lower Columbia River , *Inland Waters*, <https://doi.org/10.1080/20442041.2017.1366487>

Crawford J.T., E.H. Stanley, M.M. Dornblaser, R.G. Striegl 2016a CO<sub>2</sub> time series patterns in contrasting headwater streams of North America, *Aquatic Sciences* <https://doi.org/10.1007/s00027-016-0511-2>

Crawford, J. T., L. C. Loken, E. H. Stanley, E. G. Stets, M. M. Dornblaser, and R. G. Striegl (2016b), Basin scale controls on CO<sub>2</sub> and CH<sub>4</sub> emissions from the Upper Mississippi River, *Geophys. Res. Lett.*, 43, 1973–1979, <https://doi.org/10.1002/2015GL067599>

Descy JP, Darchambeau F, Lambert T, Stoyneva MP, Bouillon S, Borges AV (2017), Phytoplankton dynamics in the Congo River, *Freshwater Biology*, 62, 87–101, <https://doi.org/10.1111/fwb.12851>

Gómez-Gener, L., Rocher-Ros, G., Battin, T., Cohen, M. J., Dalmagro, H. J., Dinsmore, K. J., Drake, T. W., Duvert, C., Enrich-Prast, A., Horgby, Å., Johnson, M. S., Kirk, L., Machado-Silva, F., Marzolf, N. S., McDowell, M. J., McDowell, W. H., Miettinen, H., Ojala, A. K., Peter, H., Pumpanen, J., Ran, L., Riveros-Iregui, D. A., Santos, I. R., Six, J., Stanley, E. H., Wallin, M. B., White, S. A., and Sponseller, R. A. (2021) Global carbon dioxide efflux from rivers enhanced by high nocturnal emissions, *Nat. Geosci.*, 1–6, <https://doi.org/10.1038/s41561-021-00722-3>.

Hotchkiss, E. R., Hall Jr, R. O., Sponseller, R. A., Butman, D., Klaminder, J., Laudon, H., Rosvall, M., and Karlsson, J. (2015) Sources of and processes controlling CO<sub>2</sub> emissions change with the size of streams and rivers, *Nature Geosci.*, 8, 696–699, <https://doi.org/10.1038/ngeo2507>

Leng P, Z Li, Q Zhang, F Li, M Koschorreck (2022) Fluvial CO<sub>2</sub> and CH<sub>4</sub> in a lowland agriculturally impacted river network: Importance of local and longitudinal controls, *Environmental Pollution* 303, 119125, <https://doi.org/10.1016/j.envpol.2022.119125>

Lorke A., P. Bodmer, C. Noss, Z. Alshboul, M. Koschorreck, C. Somlai-Haase, D.

Bastviken, S. Flury, D. F. McGinnis, A. Maeck, D. Müller, and K. Premke (2015) Technical note: drifting versus anchored flux chambers for measuring greenhouse gas emissions from running waters, *Biogeosciences*, 12, 7013–7024, <https://doi.org/10.5194/bg-12-7013-2015>

Schneider CL, Herrera M, Raisle ML, et al (2020) Carbon Dioxide (CO<sub>2</sub>) Fluxes from terrestrial and aquatic environments in a high-altitude tropical catchment. *J Geophys Res Biogeosciences* 125: e2020JG005844. <https://doi.org/10.1029/2020JG005844>