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Reply on RC3

Sergey N. Vorobyev et al.

Author comment on "Fluvial carbon dioxide emission from the Lena River basin during the spring flood" by Sergey N. Vorobyev et al., Biogeosciences Discuss., https://doi.org/10.5194/bg-2021-109-AC3, 2021

REVIEWER: This study presents a very interesting dataset. There is a significant lack of data on GHG emissions during the spring flood of Arctic rivers, so the data collected and presented is very insightful. Because of this I recommend putting in some extra work to make the most of the data, streamline this paper and make the conclusions stronger.

RESPONSE: We thank the reviewer for positive evaluation of our work and we revised the data presentation and interpretation as recommended.

REVIEWER: Comment to line 170: You change to comparing to k_{600} values from literature, which is not the same as k values but you do not define k_{600}.

RESPONSE: We thank the reviewer for pointing out this inconsistency. In this study, we used the value of k_t (a median gas transfer coefficient) of 4.464 m d^{-1} measured in 4 largest rivers of Western Siberia (June 2015) of the Ob', Pur, Pyakupur and Taz rivers (Karlsson et al., 2021).

To standardize $k_t$to a Schmidt number of 600, we used the following equation (Alin et al., 2011; Vachon et al., 2010):

$$k_{600} = k_t\ (600/Sc_{CO2})^{-n}$$

where Sc_{CO2} is CO_2 Schmidt number for a given temperature (*t*, °C) in the freshwater (Wannikhof, 1992):

$$Sc_{CO2} = 1911.1-118.11t + 3.4527t^2 - 0.041320t^3$$

and exponent *n* is a coefficient that describes water surface (2/3 for a smooth water surface regime while 1/2 for a rippled and a turbulent one), and the Schmidt number for 20°C in freshwater is 600. We used *n* = 2/3 because all water surfaces of sampled rivers were considered flat and had a laminar flow (Alin et al., 2011; Jähne et al., 1987) and the wind speed was always below 3.7 m s^{-1} (Guérin et al., 2007).

Alin, S. R. *et al.* Physical controls on carbon dioxide transfer velocity and flux in low-gradient river systems and implications for regional carbon budgets. *J. Geophys. Res.* **116,** G01009 (2011).

Guérin, F., Abril, G., Serça, D., Delon, C., Richard, S., Delmas, R., Tremblay, A., Varfalvy, L., 2007. Gas transfer velocities of CO2 and CH4 in a tropical reservoir and its river downstream. J. Mar. Syst., **66,** 161–172. https://doi.org/10.1016/j.jmarsys.2006.03.019

Jähne, B., Heinz, G. & Dietrich, W. Measurement of the diffusion coefficients of sparingly soluble gases in water. *J. Geophys. Res. Ocean.* **92,** 10767–10776 (1987).

Vachon, D., Prairie, Y. T. & Cole, J. J. The relationship between near-surface turbulence and gas transfer velocity in freshwater systems and its implications for floating chamber measurements of gas exchange. *Limnol. Oceanogr.* **55,** 1723–1732 (2010).

REVIEWER: Comment to line 176-177: Why did you use air concentrations from Mauna Loa Observatory and not closer stations such as Cherski or Barrow? What pCO_2 air concentration values were used to calculate the fluxes?

RESPONSE: The use of world medium CO_2 concentrations for gas transfer fluxes from water surfaces is the most standard approach in this field and we did so for consistency with numerous previous works. In this study we used pCO_2 = 402 ppm. It represents the average of 129 stations all over the world (World Meteorological Organization, 2009: Technical Report of Global Analysis Method for Major Greenhouse Gases by the World Data Center for Greenhouse Gases (Y. Tsutsumi, K. Mori, T. Hirahara, M. Ikegami and T.J.Conway). GAW Report No. 184 (WMO/TD-No. 1473), Geneva, https://www.wmo.int/pages/prog/arep/gaw/documents/TD_1473_ GAW184_web.pdf) taken from The World Data Centre for Greenhouse Gases (WDCGG) which is a World Data Centre (WDC) operated by the Japan Meteorological Agency (JMA) under the Global Atmosphere Watch (GAW) programme of the World Meteorological Organization (WMO). WDCGG (World Data Centre for Greenhouse Gases) (kishou.go.jp) https://gaw.kishou.go.jp

Specifically, for the year of this study (2016) the world monthly average CO_2 concentration is as following (https://community.wmo.int/wmo-greenhouse-gas-bulletins):

Year	Month	pCO_2
2016	1	403.34
2016	2	403.84

2016	3	404.35
2016	4	404.45
2016	5	404.16
2016	6	403.07
2016	7	401.51
2016	8	400.66
2016	9	401.39
2016	10	402.99
2016	11	404.43
2016	12	405.39

Thus, taking the period of this study, end of May - beginning of June, the average value is 402 ppm which was used in our calculations. This value is consistent with that directly measured at the Tiksi station in 2016: 404±0.9 ppm (Ivakhov et al., 2019)

Ivakhov, V. M., Paramonova, N. N., Privalov, V. I., Zinchenko, A. V., Loskutova, M. A., Makshtas, A. P., Kustov, V. Y., Laurila, T., Aurela, M., and Asmi, E.: Atmospheric Concentration of Carbon Dioxide at Tiksi and Cape Baranov Stations in 2010–2017, Russian Meteorol. Hydrol., 44(4), 291–299, DOI: 10.3103/S1068373919040095, 2019.

REVIEWER: Comment to section 3.3: The discussion of the correlation of pCO2 with landscape parameters is not entirely consistent from the results to the conclusion. For example according to Table 3 pCO2 is correlated with riparian vegetation, but later on in the conclusion it is stated that it is correlated with the width of the riparian zone. So the riparian vegetation is a proxy for the width of the riparian zone? I note you did these correlations for the tributaries which gives interesting results, but how about for the main stem? It would be interesting to see since in the main stem pCO2 increases from south to north. The first sentence in the results section (L247-250) gives to understand that you did this but based on the captions of Table 3 and Figure 5 you only did the correlations with data from the tributaries- correct?

RESPONSE: The reviewer is totally correct. Yes, the riparian vegetation is a proxy for the width of the riparian zone. And yes, we run the landscape control correlations only for the tributaries. The size and huge diversity of the main stem watershed did not allow producing sufficient information on land cover of the Lena River and this can be a subject of another study.

REVIEWER: Comments to section 3.4: The calculations of the areal lotic C emission for the entire open water season are not entirely clear to me. Did you use different pCO2 and k values for the main stem and the tributaries? You state that 1 to 2 g C m-1 d-1 covers full variability of the large and small tributaries and the Lena River main channel (L291-293) but Tables 1-2 show that there is values lower and higher than this. Also L348 states that the range in the tributaries is (0.2 to 3.2 g C m-2 d-1) and L289 that the Aldan river had considerable higher emissions than Lena river main stem, how was this taken into consideration in the areal C emission calculation?

RESPONSE: This is very pertinent comment and we thank the reviewer for bringing it out. The k value for the main stem and tributaries was the same (4.464 m d^{-1}); it represents the average measured in 4 largest rivers of Western Siberia in June 2015 (Ob’, Pur, Pyakupur and Taz rivers, Karlsson et al., 2021). These 4 rivers are fairly representative for the Lena River and its tributaries, although the *k* value should be considered as highly conservative. The pCO_2 used for flux calculation (Table2) was directly measured in the full transect of the main stem and the tributaries. When providing the largest possible span of average emission values (1 to 2 g C m^{-2} d^{-1}), we used the median values of the main stem and tributaries.

We further revised the calculations following the comments of this and other reviewers. For this, we explicitly took into account the water area of the main stem (43%) relative to the total Lena basin and we introduced the partial weight of emission from three largest tributaries (Aldan, Olekma and Vitim) according to their catchment surface areas (43, 12 and 14% of all sampled territory, respectively). We summed up the contribution of the Lena river main stem and the tributaries and we postulated the average emission from the main stem upstream of Aldan (1.25±0.30 g C m^{-2} d^{-1}) as representative for the whole Lena River. This resulted to updated value of 1.65±0.5 g C m^{-2} d^{-1} which is within the range of 1 to 2 g C m^{-2} d^{-1} assessed previously. Note that this value is most likely underestimated because the emissions from the main stem downstream of Aldan are at least 10 % higher (Table 1, Fig. 1 B), and it could be so for the whole remaining part of the basin, not sampled in this work.

REVIEWER: In terms of the k values used: You answered to the comment from reviewer 2 that you use the k values 4.46 m/d from Karlsson et al., 2021, this is not clear in L167-169. It reads as if you used the value 4.6 m/d based on Serikova et al., 2018 and Karlsson et al., 2021. You do then in L218 state that 4.46 m/d from Karlsson et al., 2021 is used. I would suggest changing L167-169 so this is consistent.

RESPONSE: Good point; thanks for catching this. We will correct all numbers accordingly. One single value of k (4.464) was used throughout all calculations.

REVIEWER: The dataset collected is very interesting and provides a lot of great insights. To me it is a bit of a missed opportunity to not utilise it more when estimating the areal lotic C emissions of the Lena basin. I would have liked to see how estimated areal CO2 emissions during the spring flood months, calculated with a k value corresponding the higher flow, a larger water surface area (281000 km2) and your slightly higher pCO2 values compare to the summer month, calculated with a k value corresponding the lower flow, a smaller water surface area (281000 km2 - 5022 km2) and previously published slightly lower pCO2 values. I note that you replied to reviewer 2 that decreasing the water surface area for summer reduced the result by less than 15% which is below the range of your uncertainty. It would be good to see this more explicitly in the publication, this is not clear in section 3.4. Is this what the number 0.67±0.15 (n = 47) (L281) indicates? It would also be interesting to know how much the Lena River main stem contributes to the areal CO2 emissions in contrast to the tributaries.

RESPONSE: We agree with a necessity of more rigorous and extensive estimation of aerial emissions. We added more elaborated calculations of aerial fluxes, taking into account the Lena River main stem (43% of the water area, as calculated in response to this request) and partial contribution of largest tributaries (according to their catchment areas), see our answer above.

We would like to note that while the summer period non-covered in this study (July-August and September) is characterized by slightly lower water surface areas, the water temperature and in-stream organic matter processing are higher than in spring and thus the overall CO_2 emissions during these months of the year might be sizably higher than those during the spring. We prefer to avoid extensive speculation on seasonaliy as it was not within the scope of the present work (a first snapshot assessment of C pattern in the Lena basin). However, we are confident that possible variations in water surface areas (including the contribution of very small streams, see section 4.2) do not exceed the range of uncertainties on emissions estimated in this study.

REVIEWER: Comment to lines 383-385: You compare your estimated C evasion to the DOC+DIC lateral export of the Lena River determined by other studies. Since you also collected DOC and DIC data I was wondering if how your data to compares to that of those studies?

RESPONSE: The lateral C export by the Lena River is based on regular (monthly to weekly) monitoring of dissolved C concentration and daily discharges at the terminal gauging station of Kusyur, some 700 km downstream the most northern sampling point of this study. The spatial variations of DOC and DIC concentrations obtained in this study cannot be used for calculating the lateral export. The reviewer is right when requesting to present a comparison of our data with those of other studies and we did so below.

Generally, the concentrations of DOC measured in the present study during the peak of the spring flood are at the highest range of previous assessments during summer baseflow (around 5 mg L^{-1}; range of 2 to 12 mg L^{-1}, Kuzmin et al., 2009; Cauwet and Sidorov, 1996; Lara et al., 1998; Lobbes et al., 2000; Kutscher et al., 2017).

The DIC concentration in the main stem during spring flood was generally lower than that reported during summer baseflow (around 10 mg L^{-1}; range 5 to 50 mg L^{-1}) but consistent with the values reported in Yakutsk during May and June period (7 to 20 mg L^{-1}, Sun et al., 2018). Sizable decrease in DIC concentration between the headwaters (first 500 km of the river) and its middle course was also consistent with the Alkalinity pattern reported in previous works during summer baseflow (Pipko et al., 2010; Semiletov et al., 2011). For the Lena river tributaries, the most comprehensive data set on major ions was acquired in July-August of 1991-1996 by Huh and Edmond's group (Huh and Edmond, 1999; Huh et al., 1998a, b) and by Sun et al. (2018) in July 2012 and end of June 2013. For most tributaries, the concentration of DIC was a factor of 2 to 5 lower during spring food compared to summer baseflow. This is highly expected result given strong dilution of carbonate-rich groundwaters feeding the river in spring high flow compared to summer low flow.

REVIEWER: In L243 I understood that you compared it to your own collected DOC and DIC data, or is this also a comparison with published results? In that case a reference in L243 would be good. In general there is a lack of further discussion of your DOC and DIC data.

RESPONSE: In this part of the text, we describe the spatial variability of DOC and DIC concentrations obtained in this study. We do not extensively discuss these data because there are no sizable diurnal variations. We did examine the DOC variability in the tributaries, and, in response to other reviewers, we tested a link between DIC (and pH) and CO_2 concentration in the main steam and tributaries. We did not find any sizable control and these results will be presented in the revised version. In response to this comment, extensive discussion of DOC and DIC results in comparison with literature data is presented in our response above and will be included in the revised manuscript.

REVIEWER: The grammar and sentence structuring throughout needs improving, this will greatly help with the overall cohesion and readability.

RESPONSE: We agree and will invest in revision of grammar and syntax of the text seeking a help of native English speaking scientist.

Reviewer: L 327 says there was no relationship but then in brackts says: ($p < 0.05$)

Response: the last term is not needed here

We will correct inconsistent use of units as noted by reviewer in the revised version.