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Interactive comment on "Evaluating stream CO_2 outgassing via Drifting and Anchored flux chambers in a controlled flume experiment" *by* Filippo Vingiani et al.

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We thank the anonymous Reviewer for her/his considerations/comments. We will be happy to improve the manuscript based on the suggestions and comments provided by the referee. A more detailed answer to each specific comment follows.

Comment: L26 greenhouse gas emissions Answer: Thank you. We will correct it.

Comment: L33 might be helpful to show Eq. 1 here Answer: Ok, we will fix it.

Comment: L37 Yes, k can vary in space and time, which is a very important characteristic. I suggest expanding the aspect of spatiotemporal heterogeneity. Maybe the

authors can add some examples or numbers to give us a better understanding how much k can vary in space and time in rivers? This could be then also used in the discussion of spatial k600 of drifting k600. Answer: Thank you for this suggestion. We will expand the issue of spatiotemporal heterogeneity of K in the revised text.

Comment: L40 Are there k models for rivers other than from Raymond et al. 2013? If yes, do they also use wind, current and slope? I'm surprised to see only one reference here. Answer: In L40 we quoted the paper from Raymond et al. 2012 (not Raymond et al. 2013). In this paper the authors summarized many of the available equations relating gas transfer velocity to the hydraulic geometry. We will replace "Raymond et al. 2012" with "Raymond et al. 2012 and references therein"

Comment: L55 a floating "flying" chamber design with flexible chamber walls has also been successfully used by Rosentreter et al. 2017 and Jeffrey et al. 2018 Answer: We will add the suggested references. Thank you.

Comment: L58 Yes, local CO2 sources such as groundwater inputs change surface water CO2 concentration, but how would they interfere with local k? Answer: Local groundwater inputs rich in CO2 do not affect k. We argued that the chamber method allows direct point measurements of gas fluxes and these latter could be helpful to observe the spatial heterogeneity of gas fluxes in a stream sourced by pointy CO2 groundwater inputs. We will better clarify this point in the revised text.

Comment: L69-70 This may be exaggerated. For example, the study by Rosentreter et al. 2018 compared k of CO2 in mangrove surrounded creeks, lakes, main river channel, and a bay and in direct comparison to a dual tracer experiment and found good agreement between the two methods (5% discrepancy). Lorke 2015 compared drifting vs anchored chamber measurements. Jeffrey et al. 2018 compared chamber measurements in different sections of an estuary. etc... so this has been discussed before and also quantified. Was there a fan attached inside the two chambers? Did you test for evenly distributed air circulation inside the chamber? Did you test for temperature artefacts inside the chamber? Was the temperature constant during chamber incubations? Answer: All the studies mentioned in this comment (Rosentreter et al. 2018, Lorke et al. 2015, Jeffrey et al. 2018) discuss or quantify the uncertainty associated to the estimate of k via the chamber method but using the water CO2 concentrations obtained via other techniques. In our manuscript we discuss and quantify the uncertainty associated to the estimates of k derived only from CO2 observations inside the chamber. This will be further stressed in the revision of the paper. A fan was not used inside the chamber and the temperature was always almost constant during the chamber incubation. This information will be added in the Method section.

Comment: L126 what CO2 sensors? Please add brand, model, and accuracy of CO2 sensor and CO2 analysis. Answer: The CO2 sensor specifics are indicated in L101:104.

Comment: L127 Roughly, how long did you conduct chamber incubations (runs) for? minutes, half an hour? an hour? Answer: Chamber incubation time for steady deployments ranges between 12 mins to 45 mins. This will be specified in the revised methods.

Comment: L134-135 Is this a problem? Even if chamber concentrations inside were not atmospheric, you can still use the change of concentration for estimating k, no? If you measured CO2 every 30sec over the duration of the chamber incubation, then you have a start and end concentration over time (F) that you applied in Eq.2 and Eq.3. Meaning only the difference between start and end concentration is important (slope) and not the concentration itself. I'm curious to hear if the authors agree or disagree. Answer: The equilibration condition at the moment of incubation does not affect per se the measure of k. Thus, we agree on the possibility of estimating k also in the case the chamber is not perfectly equilibrated to the atmospheric value at the moment of incubation. In general, ensuing a perfect equilibration before incubation is important to have a realistic representation of the CO2 fluxes occurring at the water-atmosphere interface. Moreover, if the chamber is not equilibrated to the atmosphere you might

have a non-homogeneous CO2 concentration inside the chamber at the beginning of the incubation and this could generate a potential bias in the typical exponential curve during the incubation (i.e. Eq. 2 and 3 could be no longer valid). Also, ensuing equilibration to the atmospheric value before incubation guarantees the maximization of the concentration gradient and this leads to a clearer CO2 signal.

Comment: L139 Were the atmospheric concentrations outside close to 400 ppm? Answer: Yes, we will explicitly indicate it around L139.

Comment: L193 do you mean increasing "linear regression"? If yes, what was your threshold r2? Answer: The first data quality check we used was to consider only the curves that showed a monotonous increase (or decrease) in CO2 concentration inside the chamber volume during the incubation process. This was just a visual data quality check. Then we used the NSE coefficient to discriminate the ones with the highest performance. This wil be clarified in the revised Methods.

Comment: L250-251 this sentence could be deleted as this is also mentioned in the Table 3 caption. Answer: We will delete it. Thank you.

Comment: Figure 4b shouldn't this be k600, not k? Answer: No, the figure is referred to a single deployment (there is no need to standardize the k to k600 in this case).

Comment: While this study greatly contributes to our understanding of appropriate chamber design and conditions (drifted vs anchored) of the chamber method in general, I wonder how good this chamber method is in predicting the CO2 flux in comparison to other k methods and empirical k models? For example, were CO2 fluxes measured in the flumes better predicted by k600 derived from the chambers measurements in this study than predicted from k600 models (e.g. Raymond et al. 2013, Ulseth et al. 2020)? Or more practically, would the authors recommend to use FF chamber anchored mode over the k600 model by Ulseth et al. (2020) based on energy dissipation for estimating CO2 fluxes in rivers? Do the empirical models under or overestimate the flux? Answer: We thank you for your comment. In this paper, we do not have a direct comparison between the chamber method and other possible k methods. Hence, we are not able to state that the chamber method might be better than other k methods. In general, we observed k estimates from FF chamber in line with empirical models from Raymond et al. 2013 and Ulseth et al. 2020. We are not able to argue that our chamber estimates under or overestimates the flux with respect to these models. Your questions are all valuable, and they provide exciting hints for further research. We are conscious that to have measure of k from other methods might improve the paper and give a further support to the validity of our chamber method. We hope to have the possibility to investigate this further.

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