

Interactive comment on “Gas transfer velocities of CO₂ in subtropical monsoonal climate streams and small rivers” by Siyue Li et al.

Anonymous Referee #3

Received and published: 17 July 2018

General comments

The manuscript of Li et al. presents measured CO₂ fluxes, transport coefficients based on CO₂, and calculated pCO₂ data of running waters in a subtropical monsoonal climate zone. These data are complemented by among others water chemistry parameters such as DOC, DTN, DTP, as well as hydrogeomorphology data (e.g. water depth, flow velocity). They provide data and insights about transport coefficients for a so far understudied region and highlight the spatial variability and subsequent uncertainty for regional upscale estimates.

By investigating the key parameter for CO₂ flux estimates - the transport coefficient - in an understudied region, Li et al. address a very relevant topic. Narrowing down the uncertainties of regional upscaling estimates of riverine CO₂ fluxes is of wide interest,

[Printer-friendly version](#)

[Discussion paper](#)



hence this study would make a good contribution to the literature and the subject matter is thus of interest to Biogeosciences readers.

However, in my opinion, the manuscript has some problems:

(1) The terminology used in this manuscript is quite confusing to me. It seems to me that "streams", "rivers", "river networks" are used interchangeably (without definition and consistency), which makes it hard to follow the red line of the story. The terminology needs to be clarified and unified.

(2) The sampling design is not very clear to me. All investigated running waters seem to be in the Three Gorges Reservoir (TGR) region, but in addition two larger streams (Daning and Qijiang) were sampled. In the results and discussion, these investigated running waters are combined, sometimes split, which makes it hard to follow (in the main text and tables). In my opinion, these three "regions" need to be presented in a unified way (always separated or combined, possibly both in each table and figure), and presented more clearly in the text.

(3) One of the main messages is the presentation of transport coefficients in a subtropical monsoonal climate zone, which is interesting, but I can imagine that there is a large difference in the wet and dry season. However, all the measurements were done in the wet season. I think this issue should be clearly acknowledged and discussed.

(4) There are two technical issues: (i) The measurements with the floating chambers are poorly described. The only information Li et al. provide is that the floating chambers were "deployed". If the flux measurements are done in an anchored or free floating manner is critical (see e.g. Lorke, A., Bodmer, P., Noss, C., Alshboul, Z., Koschorreck, M., Somlai-Haase, C., Bastviken, D., Flury, S., McGinnis, D. F., Maeck, A., Müller, D., and Premke, K.: 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>, 2015.). Hence, this issue needs to be addressed clearly. (ii) It seems that all the flux and pCO₂ measurements were done dis-

tributed during the day. The fact that there is a diurnal cycle of CO₂ was not considered (see e.g. Pascal Bodmer, Marlen Heinz, Martin Pusch, Gabriel Singer, Katrin Premke, Carbon dynamics and their link to dissolved organic matter quality across contrasting stream ecosystems, Science of The Total Environment, Volume 553, 2016, Pages 574-586, ISSN 0048-9697, <https://doi.org/10.1016/j.scitotenv.2016.02.095>), and values directly compared. This issue should at least be discussed.

(5) Developing models to estimate transport coefficients is meaningful, but the process of the model development is poorly described. Additionally, which data were used for the models, and which not is confusing to me (goes along with my comment (2) above).

(6) From what I see in these data, there are several running waters undersaturated with respect to CO₂ (Fig. S2 and Fig. 1), and hence a sink of CO₂. This aspect is totally neglected and the investigated running waters are generalized as CO₂ sources to the atmosphere. In my opinion, this aspect of influxes of CO₂ is very valuable and should be properly discussed.

(7) As far as I see, there is some arbitrariness regarding data handling/processing. The cut-off at 110 μatm (line 198) for the air-water CO₂ gradient for k600 calculations, as well as “When several extremely values are removed. . .” (line 303), needs to be described/demonstrated/justified much more clear.

(8) CO₂ fluxes were measured, while pCO₂ and transport coefficients were calculated. This should be clearly stated throughout the manuscript to be transparent.

(9) I am not a native English speaker, but I think that the manuscript should be revised for the English language. (see exemplarily in the specific comments and technical corrections below)

I think this is a valuable study, but the combination of the points mentioned above make the manuscript hard to follow and the conclusions and main messages drawn in the current state of the manuscript too general. In a revised version, the study would get

[Printer-friendly version](#)[Discussion paper](#)

more shaped, more detailed and informative, and the conclusions and main messages can be specified and more related to the investigated region.

Specific comments

Abstract:

Line 20: Indicate how many river networks (see general comment (2))

Line 24: As far as I understood not when all data were included. Please be more specific here.

Lines 30 – 33: This is not really new. Maybe you can specify this statement for the investigated region?

Introduction:

Line 41: Bastviken et al., 2011 totally focuses on CH₄. I suggest replacing this reference with a more suitable one.

Line 42: But you did not present "new accurate measurement techniques" in your study, what are your reasons to mention this in the introduction?

Lines 50 – 58: This equation is pretty standard knowledge and can just be described in words here. The equation can be moved to the methods.

Line 63: The standardized transport coefficient (k_{600}) should be explained here.

Line 80: You set the scene of seasonal precipitation, but in the study, you only measure in the wet season. This is contradictory. This issue should be discussed.

Lines 84-89: Kind of repetition and partially contradictory to the text in lines 43-49. These two paragraphs should be unified and clarified.

Lines 92-99: The relevance of the study (first time in this region, etc.), the objectives and how these objectives were approached should be written more clearly. At this point, the input parameters for the model development is totally unclear.

Materials and methods:

Line 105: In my opinion, Figure S1 should go to the main text. There are no sampling points for Daning and Qijiang, which is confusing to me.

Lines 105-109: Please add a reference for this statement.

Lines 110-118: Please see my general comment (2): Please restructure this, make clear how many running waters were sampled where, the size of the sampled running waters (Strahler stream order is fine), and why in these three regions. Otherwise, it is hard to follow your storyline.

Line 141-142: What is "PP"?

Line 148: I don't really understand what you mean by this sentence, please revise.

Lines 155-156: This sentence is confusing to me, please revise.

Line 158: Do you mean CO2SYS? If yes, please add the corresponding reference.

Line 167: What was the brand of the tubing?

Line 170: What is DC?

Line 173: Please see my general comment (4) (i)

Line 177: The units are confusing to me. Why is there two times pressure and temperature? Please double check if the units match up in the end, to me they do not.

Line 187: Please be more specific: k was calculated by reorganizing Eq (1)

Line 192: Sc to the power of 0.5? This seems weird. What do you mean here?

Line 198: Please justify the cut-off at 110 μatm . Maybe add a figure to the supplementary material.

Line 203: I read about water depth and current velocity the first time here. These measurements need to be described before.

Printer-friendly version

Discussion paper



Line 213: The pH is quite high. This in combination with influxes of CO₂ requires at least a short discussion about chemical enhancement.

Line 214: Please see my general comment (6)

Lines 218-222: This paragraph belongs to the discussion section.

Lines 223-227: This paragraph should be revised because it is not very clearly written. Please add the p-values to the text in case of significances.

Lines 235-237: What is the meaning of this ratio? Please add a few words what the reader can get from this information.

Line 242: These models and how you developed them should be explained better (in the method section).

Lines 246-248: I do not understand this sentence. What do you mean by "binned"?

Discussion:

Lines 270-274: How does this paragraph support the discussion of your study?

Lines 286-288: No significances... But still, you developed the models considering all data? This is not at all clear to me. Did you split/separate the data set for the models? This is not clear in Table 2. Please see my general comment (2). I think these data/models are valuable, but at the moment they seem arbitrary and should be better explained. This would help to give them more weight.

Lines 286-309: I see a lot of results here, which are presented in the discussion for the first time. The part presenting pure results should be moved to the results section.

Line 303: Please justify the removal of "extremely values". Maybe add a figure to the supplementary material. If there is no objective criteria and justification, I do not see why data should be removed.

Lines 327-333: Why discussing k values and not k600 values? I think this needs to be

[Printer-friendly version](#)

[Discussion paper](#)



unified/consistent throughout the manuscript.

Conclusion:

Lines 358-360: Very general, but actually, the regions had to be separated/ split, no?

Lines 368-369: I think you should focus the conclusion on the investigated region.

Tables:

Table 3: b) Why not presenting k600 values here which can be directly compared with other studies?

Figures:

Fig. S1: Was always everything sampled at each point? "Samples" should be replaced by "sampling point"

Fig. S4: There is no reference to this figure in the main text.

Technical corrections

Line 22: Delete "were"

Line 44: add "by" or "via" before "floating chambers"

Line 59: Replace "precisely" with "well"

Line 127: "consisted" instead of "consists"

Line 139: "pH sonde" "was" instead of "is"

Line 225: "Daning" instead of "Danning"

Interactive comment on Biogeosciences Discuss., <https://doi.org/10.5194/bg-2018-227>, 2018.

BGD

Interactive
comment

Printer-friendly version

Discussion paper

