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Biogeosciences Discussions

Interactive comment on "Gas transfer velocities of CO_2 in subtropical monsoonal climate streams and small rivers" *by* Siyue Li et al.

Anonymous Referee #2

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The manuscript reports on transfer velocities of CO2 (K) in streams and small rivers for assessing the gas fluxes. CO2 released from lakes and rivers has been recently recognized as an important component in the global carbon cycle. The accurate estimation of CO2 flux is still challenging primarily due to the difficulty in obtaining an appropriate K value. The topic would be of great interest for the community of scientists working on carbon cycles and can be considered for publication. However, the current version need to revised (see below).

General comments

As emphasized by the authors, the study focuses on the subtropical monsoonal streams and small rivers which are characterized by large seasonal variations in climate and discharge. Hence, the K value in these rivers should also have obvious

seasonality. Unfortunately, the samples presented in this study were collected in the rainy season. The K value were calculated based on the one-time sampling campaign, which might result in a certain amount of errors on the annual flux estimation. Regarding this, the uncertainty of the sampling data and the calculations, as well as the reliability of the argument should be sufficiently discussed.

In my point of view, the variation in K value of the rivers studied are obvious and need to be discussed. In addition, the spatial difference of K values is only sorted out for the three river systems (Daning, Qijiang and TGR). I would suggest the authors examine the variations of K following the physical characteristics of rivers (such as the current velocity, slope and the water depth) or/and the river orders.

The pCO2 calculated in this paper is between 50-4830ppm, which indicates that the river pCO2 value is sometimes much lower than that of the atmosphere, that is, the studied rivers can sometimes absorb CO2 from the atmosphere. It seems that the annual CO2 flux for the whole basin was calculated in this paper based on the averaged K value from the observed results using floating chamber method. The question is that is it reliable to estimate both directions of the CO2 flux at the air-water interface (including river CO2 outgassed to the atmosphere and the atmospheric CO2 input to rivers) by using the same K value? Or what uncertainty will it cause?

This study measured DOC, DTN, and DTP, but the authors did not mention these measurements in the discussion section. What is the relationship between these variables and the K values?

Specific comments

L 94-97: I would suggest rephrase these sentences, since they cannot convey clearly the real contribution or scientific merit of this study.

L 111-112, 117: The classification method of the river order used here should be clarified. The number of a river order defined by different classification system may

represent different size or hierarchy of a river.

L 214-216: This statement is problematic. Clearly, the studied rivers are not always supersaturated reference to atmospheric CO2 as the pCO2 in rivers is between 50-4830 uatm.

L 274-285 These arguments need more solid evidence to support. As mentioned in the general comments, I would suggest that the authors focus on discussions on relationship between spatial change in K values and physical characteristics of rivers or/ and the river orders.

L 497-498 The water area is a very critical parameter for the calculation of CO2 flux in a basin, so the acquisition of water area is essential and should be described more in detail. For example, what is the resolution of the satellite image? In addition, the variation of surface area of water between wet and dry seasons should be considered.

Finally, I would suggest the authors polish the English grammar and writing, as well as the figs presenting.

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