

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

Anonymous Referee #1

Received and published: 29 June 2018

General comments: The paper “Gas transfer velocities of CO₂ in subtropical monsoonal climate streams and small rivers” appears to be something of a companion piece to “Riverine CO₂ supersaturation and outgassing in a subtropical monsoonal mountainous area (Three Gorges Reservoir Region) of China” <https://doi.org/10.1016/j.jhydrol.2018.01.057> published in the Journal of Hydrology. In this current submission, the authors present k calculated from floating chamber flux measurements and using models, and discuss the implications of the differing approaches to k for making regional scale flux estimates. Using chambers to determine CO₂ fluxes, the authors then use pCO₂ to derive the gas transfer velocity. These flux-derived k values are compared to modeled k values. It is good to see the spatial aspects of the gas transfer velocity addressed. However, I do not feel that there is an

Printer-friendly version

Discussion paper



adequate consideration of the uncertainty in the estimates/calculations provided.

For the flux-derived k values, there is little provided in terms of uncertainty assessments. $p\text{CO}_2$ was not measured, but rather was computed based on pH, alkalinity and temperature. This would have large uncertainties that then propagate into k estimates. Golub et al. (2017, doi: 10.1002/2017JG003794) note that “freshwater researchers must make significant efforts to standardize and reduce errors in $p\text{CO}_2$ predictions”. I encourage the authors to undertake a more systematic uncertainty analysis for their $p\text{CO}_2$ values and propagate this error into uncertainty estimates for k .

Further, the authors here excluded deriving k values for samples that did not have a very large gradient in CO_2 across the air-water interface. The authors chose 110 μatm as the threshold for excluding data, but this was presented without any indication of choice of threshold, making it appear rather arbitrary. Given the pH of the rivers sampled and the $p\text{CO}_2$ that was at times undersaturated, this appears rather problematic in that it introduces bias that carries through to the regional estimates provided.

The authors in this paper refer to their k values as “observed”, but these are in fact derived, and so need to have uncertainty better characterized. Upscaling from X floating chamber measurements to a river network draining 58000 km^2 . How many flux measurements were made with floating chambers is not clearly stated, but it appears to be about 100 all made during summer 2016. Going from summer measurements for ~ 100 points to annual estimates for 58000 km^2 also requires some consideration of error propagation and bias. Fluxes were only retained when the floating chambers yielded linearly increasing CO_2 against time, which again biases against low flux locations. Of the attempted flux measurements, what fraction was discarded?

Finally, a minor point is that the authors state several times that theirs is the first determination of k for subtropical streams and small rivers. I would point the authors towards global syntheses on CO_2 evasion as well as individual studies that include k estimates.

Minor comments The figure S1 does not show the sample locations within the Daning

BGD

Interactive
comment

Printer-friendly version

Discussion paper



or Qijiang basins. These may be the same locations as Figure 1 in Li et al. (2018) Journal of Hydrology doi: 10.1016/j.jhydrol.2018.01.057?

There are a number of grammatical issues throughout the paper that the authors should address.

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

BGD

Interactive
comment

Printer-friendly version

Discussion paper

