

Interactive comment on “CO₂ partial pressure and CO₂ emissions from the lower Red River (Vietnam)” by Thi Phuong Quynh Le et al.

Anonymous Referee #1

Received and published: 19 January 2018

General Comments:

This paper provides some important quantification of CO₂ concentration, evasion rates, and temporal and spatial heterogeneity in an understudied Southeast Asian river system. Given the lack of data available on these systems, the concentration data presented in the paper is valuable on its own. I am concerned, however, at the large discrepancy between calculated and measured CO₂ given the lack of a reasonable explanation aside from calculation error. Furthermore, along with what the other reviewer wrote, the reliance on wind-speed as the only determinant factor for k₆₀₀ is subject to large errors in flux estimation. Knowing this, it is nearly impossible to assess how well correlated CO₂ flux is with any of the environmental parameters used in the multi-variate analysis at the end of the paper. I think if the authors figured out why

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their calculated values are off, used a more broadly accepted model to estimate k600 (or better yet, measured it directly), and simply presented the concentration and flux measurements from the Red River, it would be a valuable contribution to the literature.

Specific Comments:

-How exactly does the data presented in this manuscript relate to anthropogenic impacts? The authors suggest that the Red River is “strongly” affected by human activities but do not provide results that suggest humans have altered the amount or way by which CO₂ is evaded from the river. The “influence of dams” based simply on observing higher concentrations at that site is unconvincing – there is no direct evidence. Likewise, the ascribed influence of human population is equally weak

-There are more accurate ways to calculate k600 than from wind speed. As the other commenter suggested, instantaneous discharge, flow hydraulics, and even channel slope may provide better or more robust ways to model k600 compared to wind speed

-Although the authors suggest that their direct and calculated pCO₂ values were well correlated, they do not seem to correspond very well at all. The y-axis in Figure 3 makes it impossible to appreciate the noise in this relationship. More importantly, there is no plausible explanation as to why the slope is not close to 1, but rather that calculated pCO₂ was nearly 1/5 of that measured directly by their equilibrator

-The explanation that wind speed is driving the diel signature in CO₂ flux is circular. Wind speed is how the flux was calculated in the first place, so higher winds during the day will always yield a “higher flux” of CO₂ during the day. CO₂ might have a completely different diel pattern if modelled with something other than wind speed

-No specific reasoning given as to why temperature was positively correlated with pCO₂ concentration except enhanced weathering rates. Did the authors consider in-stream respiration?

-It's hard to evaluate the seasonal variation of CO₂ fluxes generated from only wind

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speed and concentration

-What are the +/- values in any of the tables? Standard deviation?

-There seems to be overall very little diel signature in any of the water chemistry data presented in the paper. All of the differences seem to fit within the error bars of each “average” measurement.

Technical Corrections:

17: I would avoid the use of the word “good” when describing a river system

19: Not sure what is meant by “carbon dynamic”

21-22: Do you mean “relative” rather than “in contrast”?

42-43: Unclear sentence summarizing Raymond 2013

113: How was alkalinity measured? There are no methods detailing this

277: Discussion mixed in with results, difficult to follow

286: More discussion in the results section

346: Results being presented in the “Discussion”

359: Results being presented in the Discussion

Interactive comment on Biogeosciences Discuss., <https://doi.org/10.5194/bg-2017-505>, 2017.

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