

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

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Received and published: 8 January 2018

This paper documents the chemical conditions and concentrations of dissolved carbon dioxide in the Red River system of Vietnam. The data contribute to the "database" of concentration values for the globe, with one goal of further constraining the CO₂ source strength of inland waters. Therefore, the data are valuable on their own, especially given that they fill in geographic gaps for SE Asia. The main criticism of this paper is the use of a wind-driven gas exchange model. While criticism of gas exchange models are prevalent within the community of researchers, this example is especially problematic as it relies on unlikely drivers of turbulence (and thus gas exchange) in riverine systems. There is some evidence that gas exchange is enhanced by some wind patterns in very large rivers, however, gas exchange in rivers is not considered

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to be a major driver. Rather, it is turbulence generated by water flow that drives gas exchange rates in these systems. Therefore, the CO₂ emission estimates are not only biased, as recognized by the authors, but are likely to be highly inaccurate due to the model selected. It is hard to believe the results without some other line of confirmation. In addition to the criticism of the estimates of gas exchange, I did not find the discussion points to be well supported by the data especially given the limited time and geographic scope of the measurements. There is simply not enough evidence to support any of the inferred drivers of CO₂ variability in this river system.

Specific Comments:

48: what references support plate tectonics as major drivers of carbon fluxes in this system?

53: are changes in sediments the hypothesized drivers of changing carbon fluxes in this study?

184: if exchange is less related to wind, then what is the justification for using this model in the present study?

214: such low temperature variability leads to skepticism of this environmental parameter being a significant driver of CO₂ variability. In addition, the broad conclusion here is that water chemistry seems to be quite stable over time.

273: a lack of CO₂ diel variability, but a finding of diel exchange variability, is a direct function of the model. This diel variability in fluxes then, is simply due to changes in wind which I do not believe are likely drivers of gas exchange in most river systems.

276: this section reads more like discussion than results

346: in contrast, this opening paragraph of the discussion most likely belongs in the results section of the manuscript

359: what part of the study design allows for a significant investigation on the role of

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dams and gas exchange?

401: paragraph is too speculative

449: but the temperature variation was very small. How much could this have possibly contributed to the variation in CO₂ exchange?

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