

Biogeosciences Discuss., referee comment RC1
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Comment on bg-2020-477

Anonymous Referee #1

Referee comment on "Spatial and temporal variability of $p\text{CO}_2$ and CO_2 emissions from the Dong River in south China" by Boyi Liu et al., *Biogeosciences Discuss.*,
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The paper by Liu et al. presents seasonal $p\text{CO}_2$ concentrations and CO_2 fluxes from the Dongjiang River basin. They found that concentrations and fluxes were higher in larger rivers relative to smaller ones and in the wet season (summer) compared to the dry season. They also contextualized some of these broader findings with auxiliary measurements of DO, DOC, alkalinity, and pH.

The paper presents a good quantity of spatially and temporally resolved CO_2 data, adheres to established methods, and is generally well written. I think the data alone is a useful contribution however I think much of the discussion surrounding the drivers and explanation of CO_2 differences is either lacking or unsupported. I think that after some revisions of the discussion, the manuscript could warrant publication in *Biogeosciences*. Below are my primary criticisms, followed by line-specific minor comments.

1. The results show that $p\text{CO}_2$ (and in turn FCO_2) is higher in the larger rivers compared to the smaller rivers, which the authors interpret as resulting from proportional differences in C inputs (both CO_2 and DOC) and metabolism of allochthonous inputs. Since these are connected systems (i.e. the small rivers eventually flow into the larger ones), I'm a bit puzzled how CO_2 would increase downstream due to higher C inputs unless the study design somehow missed high CO_2 inputs from low order streams that directly joined the mainstream? Based on Figure 1, it appears that many of the smaller rivers were also at higher elevation. A bias towards higher altitude sites in the smaller rivers could explain the observed trends if these catchments had less vegetation/forest cover and therefore less C inputs (as both CO_2 and DOC). Indeed, the authors observed higher DOC concentrations in larger rivers, which they assume fuels higher respiration. Where does this DOC come from if it doesn't pass through smaller rivers first? I suspect there is some sampling bias at hand.

There are additionally more processes, such as photo-oxidation or titration of the carbonate equilibrium via organic acids (indeed you see increasing CO_2 with decreasing alkalinity), that could impact some the observed downstream increase in CO_2 . These aspects are not discussed in the manuscript and the authors conclude too strongly that they know the responsible drivers without data to support such claims. Since more highly

productive vegetation in the catchment could result in both higher CO₂ inputs and higher DOC that fuels respiration, I think it would be useful to explore the relationship between C concentrations (pCO₂ and DOC) and catchment land-cover (perhaps as a fraction of wet area, similar to Rocher-Ros et al 2019, L&O Letters or % forest cover).

2. The discussion of spatial and temporal patterns is blended together and needs to be disambiguated a bit. It is hard for the reader to make sense of these various overlapping trends. I would suggest starting with one (spatial), then the other (temporal) before finishing on how they overlap to result in the observed pattern.

3. Increased precipitation can both increase the transport of terrestrial C (including CO₂) and dilute it. How do you know which process dominates?

4. Throughout the discussion, the authors fail to reference their figures or tables in many cases that would make it much easier to observe their explanations.

5. Given the high resolution of the pCO₂ data, would it not be interesting to upscale outgassing for the whole basin? Perhaps it could be compared to DOC/POC export if those have been previously estimated (or even roughly estimated using your values). At the very least, I think the authors' data could be nicely displayed on a map (Similar to Figure 1 of Rocher-Ros 2019, Limnology and Oceanography Letters).

Overall, I think the discussion of the drivers of CO₂ variability is overstated. Specifically, there is no direct evidence of lateral soil CO₂ nor dilution effect caused by precipitation. There doesn't seem to be much of a difference in dCO₂ vs. dO₂ between large and small rivers (Figure 6), suggesting that metabolism is similar. At minimum, the current discussion would need to justify why simultaneously low DOC and CO₂ are not an artifact of altitude/land-cover.

Minor comments:

16-17 - what direct evidence of soil CO₂ and dilution is there to support this statement?

96 - Figure 1 could be supplemented with a landcover map. Many of the smaller rivers appear to be at higher elevations and I am curious if they are less forested.

103 - Figure 2's data might be better suited for a bar graph?

163 - I think the reference to equation 2 is incorrect here.

195 - There is no hydrologic data in Table 1. Discharge should be presented.

197 - Again there is no stream width or discharge data presented anywhere in the manuscript beside these lines of text.

202 - U10 is undefined.

275 - DOC and CO₂ can simultaneously be transported from terrestrial systems, which also might explain their correlation.

297-318 - This section is very overstated and not the only way to interpret these data. I recommend revising and rephrasing to reduce certainty and include alternative explanations.

381-382 - This is possible, but not certain.

390 - Respiration and photosynthesis can occur simultaneously.

405 - The units for pCO₂ are not consistent (some times uatm sometimes ppm). What about Borges 2015, nature geoscience that includes a significant amount of data for rivers in central Africa? Also Mann et al. 2014 JGR-Biogeosciences has additional pCO₂ data. Lastly, is the Mississippi River really a subtropical basin?

409-412 - Again, I don't think these conclusions are justified.

417- Still don't really see how depletion would only affect small streams and not the larger ones they flow into?