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Interactive comment on “Spatial variability and temporal dynamics of greenhouse gas (CO₂, CH₄, N₂O) concentrations and fluxes along the Zambezi River mainstem and major tributaries” by C. R. Teodoru et al.

Anonymous Referee #3

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This study assesses the spatial and temporal variability of various greenhouse gas concentrations and fluxes in the Zambezi River basin. Recent work has revealed the important role that inland waters play as processors of carbon in the global carbon cycle and that inland outgassing fluxes often exceed fluxes to the ocean. In this context, the work done by the authors is very important, as they have taken high-precision measurements in a relatively understudied region. This manuscript is also potentially important as the authors find gas concentrations below the assumed value for tropical rivers, which could have implications for future assumptions about tropical rivers.

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However, the study is extremely descriptive and there is little clear interpretation of what is driving these fluxes or why they are lower than typical values. The Discussion section in particular has a heavy focus on descriptive statistics, has long dense paragraphs, and will need to be re-focused on interpretation. That said, I'm excited by the work, and strongly encourage the authors to present as clear and concise as possible.

Specific comments below:

2.1 Overly long and descriptive, details do not add to the reader's understanding of the study or the stated goals of the manuscript. For example, why is there so much background on land-cover if it is never mentioned again in the paper?

Fig. 3a,b Horizontal axis is slightly confusing. There are multiple rivers but different sources. Maybe rephrase to "Distance from mouth"?

Fig 3c. Would be most helpful to see this as mol vs mol, not % sat O₂. Also interesting to see if the slope is 1.3 as that is the value used to convert O to C values.

4.1 This section is very long and much of it could be slimmed down and moved to the results section. The interpretations of the pCO₂ levels should be condensed and related back to the main goals of the study.

There are several interesting interpretations here (outgassing due to large waterfalls) + the importance of floodplain input of CO₂, but they are lightly buried in the descriptive nature of this section.

The primary production rates are measured several times in this section, but are not included in the results or a table. The same goes with the respiration rates mentioned in the methods section.

P16512L2-4 The mechanism behind the high pCO₂ isn't really described here. Could link this "false" floodplain created by the damming back to the elevated CO₂ levels seen in the natural floodplains.

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P16412L23 – P16413L7 This seems to be the most interesting finding of the paper, and needs to be expanded upon. As it stands, it is nearly buried by descriptive statistics.

4.2 The purpose of this section seems to be to determine whether the DIC levels in this river can be explained by weathering. However, it is difficult to follow as written and does not add much to the overall manuscript. As before, this section is mainly results. There are some interesting findings, but the interpretations must again be condensed and tied back to the original goal of the study. The isotope values do not add to the study as it stands.

4.3 This section is mostly results and seems unnecessary. The authors state that the overall effect of the diel variation on riverine variability seems small. The data can be included in the supplement if the authors are concerned w/ diel variability.

4.5 This section gets at the stated goal of the paper(calculated fluxes). This section actually contains a lot of information, but I think that again, most of it could be moved to the results.

P16423E3: I do not see the need to include this exponential fit. It is unlikely to hold true in any different system and would likely be specific to this unique sample site(and at the times sampled).

4.5 This section could be very interesting, but the errors associated with some of these values might be too high to accurately calculate a mass-balance. The authors mention that when they include floodplain fluxes, their values are more consistent with global estimates of riverine export. Could these values have been included in the mass balance in the beginning? This section has the potential to be interesting and important, despite the large error in several measurements. However, the authors need to relate this to the overall goals of the project and tie their interpretations in with the rest of the results.

Fig. 10: Caption should read “diel variation” not “dial”

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