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Interactive comment on "Spatial variability and temporal dynamics of greenhouse gas (CO_2, CH_4, N_2O) concentrations and fluxes along the Zambezi River mainstem and major tributaries" *by* C. R. Teodoru et al.

Anonymous Referee #1

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This study assesses the spatial and temporal variability in the concentrations and fluxes of the three main GHG found in the Zambezi river. Based on state of the art techniques and measurements, the authors calculate mass-balance for CO2, CH4 and N2O across the Zambezi river, which drains 1.4 millions km^2 of African territory. Global estimates of GHG emissions from aquatic ecosystems in last 5-10 years have been constantly going up, either due to different methodology or due to the inclusion of systems or regions that had traditionally been underestimated. Yet the estimated aquatic emissions cannot constantly increase if the global C budget is to be resolved. In this context, the results

of this study have important implications for global extrapolation exercise because 1) they report high-quality data on aquatic biogeochemistry for an under-studied region of the globe and 2) concentrations and fluxes of GHG are typically lower than what has been reported in other tropical regions.

This study, however, is very highly descriptive. While overall I believe that its descriptive nature fits rather well this data-intensive manuscript, I think the main claims tend to be buried among masses of secondary details, and that readability and potential impact suffer from it, especially in the "Discussion". Below I provide suggestions that mainly aim at improving readability and better emphasizing what I consider to be the main novel aspects of the manuscript.

Specific comments: Figure 1: It would be helpful to show the previously studied African areas (in terms of GHG dynamics), perhaps on the inset (could be bigger). Right now a number of studies are cited in the intro, but without reading all of them it is hard to quickly judge how the current ms represents a significant advancement over the study referred to (in terms of the magnitude of the spatial extent and the distribution of the areas covered).

Section 2.1: Very long section, but I rather enjoyed reading it

Figs. 3,4,5 and 9: Including a line that would represent the weighted average for all the sites included in this study would help placing those results in a larger context

Fig. 3c: It would be useful to present this graph in terms of mol for mol of CO2 vs O2

p.16 409L1: "T" missing in "starting"

Section 4.1: This section is very long and descriptive, and most of it is actually result. There is barely any interpretation in it. Parts of this section could be cleaned up by merging some results in the corresponding place in the "Results" section while focusing on interpretation here, and the implications for the main points of the study. Same applies to similar comments below.

P16412L2: s missing to “alteration”

P16412 L16 : P16413 L7: This is what I consider as the most novel aspect of the work, but it is completely lost among a nearly 6 pages long section

Section 4.2: I am not sure what this brings to the rest of the ms. I understand that the authors aim at describing the different sources of carbon for the Zambezi river, but DIC stable isotopes come out of nowhere that far in the manuscript. There is nothing in the introduction that sets up why we should care about DIC stable isotopes, and, again, most of this section is actually results. The authors should consider removing this section, or better placing it in the overall context of the paper. If the latter is done, I believe that this section should be condensed.

Section 4.3: Again, this is mostly results, and new figures keep being introduced that far in the ms. Why did the authors present these numbers in the discussion?

Section 4.4: I believe that readability suffers from having the discussion of the concentrations and fluxes of GHG so far apart form each other, with so much new content (i-e results) in between

p.16421 L 16-23: This is a rather critical claim, which would actually help explaining why this study measured typically lower fluxes than other tropical regions. It would further suggest that riverine fluxes estimated from chamber measurements around other rivers of the world may have been systematically over-estimated. I would expect to see the data here as this directly contributes to one of the main conclusions of the paper.

p.16423 L10-13: I have some difficulties with this equation. Conceptually, is not that an empirical way to estimate an average regional “k” for all the systems studied here? (i-e flux = concentration * “something”). I did not do the math but I suspect that the product of the different parameters, with proper transformation, would yield close to the average k for the studied sites.

I am not sure why someone would want to use this equation when you can simply

multiply measured (excess) CO2 by a realistic estimate of k for a given type of systems. It may be more useful to simply report the average k measured here if this is to be used for extrapolation purposes.

Section 4.5: Again, very long and descriptive, and mostly results, and nearly 4 pages of text without a paragraph. I got completely lost in reading this section and I could not identify the main points. What are the implications of those results, and why are they included in the discussion?

Interactive comment on Biogeosciences Discuss., 11, 16391, 2014.