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Interactive comment on "River inflow and retention time affecting spatial heterogeneity of chlorophyll and water–air CO_2 fluxes in a tropical hydropower reservoir" *by* F. S. Pacheco et al.

Anonymous Referee #2

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General Comments:

Overall the authors present an analysis of field collected data designed to emphasize the importance of spatial heterogeneity on the calculation of carbon fluxes from a tropical reservoir. The authors take an interesting approach by attempting to constrain the drivers of both Chl and CO2 as a function of physical (riverine influence) and chemical parameters. With that said, the authors admit the hydrologic modeling was limited in its ability to constrain a small dataset for this analysis.

One aspect that seems to be missing from the discussion involves the importance of sediment carbon fluxes. The authors admit that there are additional citations that mea-

sured sediment CH4 and CO2 near the dam, but this work should be further addressed within this manuscript to round out the discussion. In fact, without this information, the conclusions regarding the importance of CHl on CO2 are limited. In addition, are there physical effects of increased turbulence in the dry season as the river water plunges? Could this actually increase the release of CO2 and CH4 from sediments? Perhaps these questions cannot be answered, but a more well rounded discussion of the additional influences on CO2 fluxes is warranted.

Finally, the authors do want to emphasize the importance of spatial as well as temporal studies for addressing GHG fluxes from reservoirs. However their data as presented do not necessarily show this importance. Intuitively there would be a higher standard deviation using the spatial data versus a single location (table 4) but this does not seem to be the case. When you look at the SD, the data do not suggest much of a difference. The lack of differentiation between the two datasets should be addressed within the discussion. Should median values be used?

Specific Comments:

In general the manuscript should undergo an additional proofread, as there are many simple grammatical errors present. This is perhaps a result of English not being the primary language of the authors, however the presentation of material is held back. I have presented many examples (but not all) of the grammatical errors to be addressed observed in the first few pages. Overall there were many articles missing from the text and there were many places where the verb tenses were confusing.

8533: 11 – s on factors 14 – s on conclusion 21 – potentially 'a' more important source. . .

8534: 24 – differences 'in'. . . 29 – large nutrient loads. . .

8535: 11- high density of spatial data 11 – our hypothesis is. . . 14 – the second hypothesis in not really a hypothesis, but more of a comment related to the investigation

outlined in the first hypothesis. 20 – you can simply say elevation of 440m as this is a standard against mean sea level. 21 – what is the coppan system? Do you need to even reference wet-warm and dry cold?

8536: 3 – I do not know what demographic density means. 4 – restricted by rainfall?

Generally – do you need the description of how the PHYTO-ED works? This seems extraneous to the manuscript.

8537: 16-20, This section seems to have result more than methods presented. The RMSE of the spatial fit seems to be more appropriate for the results. 26 – state water temperature

8539: eq 2 appears to be incorrect. Please check the algebraic rearrangement from k600 – k. This should require all calculation for the (k) and hence the fluxes calculations be double checked.

8541: 20 – you mention MODIS data but you need to be more specific. Was this 1km MODIS Aqua, Terra, what is the product and reference the dataset. 8542: In general, if Chlorophyll is used to determine the transition zone locations, then presumably this zone changes in size throughout the year, as do all of the other zones. How do the authors handle this in the analysis specifically? Perhaps just additional detail on this in the methods since it is included in the discussion.

8545: 24-25 high pCO2 from riverine sources may not only come from reduced phytoplankton due to turbidity. High CO2 is prevalent in almost all river water, and may come from many difference sources, including terrestrial respiration. This should be made more clear.

8546: 17-19 this discussion would benefit from more detail that might have been presented within the Ometto et al 2013 reference. It appears that additional carbon data is available and would be useful to the reader within this manuscript. Specifically, if there are complementary data on the outflow CO2 and CH4 concentrations.

20-22 - especially because there is no data – the authors should use terms like ‘could’ instead of seems to.

Interactive comment on Biogeosciences Discuss., 11, 8531, 2014.