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Interactive comment on "Physical-controlled CO_2 effluxes from reservoir surface in the upper Mekong River Basin: a case study in the Gongguoqiao Reservoir" *by* Lin Lin et al.

Anonymous Referee #1

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This paper describes CO2 concentration and flux measurements made over a ∼1 year period upstream, downstream, and within profundal and littoral regions of a run-of-river (short residence time) reservoir in the Mekong River Basin. While the role of reservoirs in carbon and greenhouse gas budgets is an important and timely topic, I think this paper needs significant restructuring and re-framing before publication in Biogeosciences. Firstly, I don't think the argument that few CO2 efflux measurements have been made in China is substantiated (see global map in Figure 2 of Deemer et al. 2016). The authors even cite a number of other studies of carbon dioxide dynamics in Chinese reservoirs. I think the authors could emphasize the importance of understanding these dynamics in the Mekong basin given all the reservoir development that

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Discussion paper

is slated for the region (maybe cite Zarfl et al. 2015 Aquatic Sciences). The authors could also do a better job of describing the unique hydrology/climate in the Mekong Basin since the diverse readership base may not be familiar with the characteristics of dry vs. wet seasons in this region. Secondly, I think the authors should be careful in their discussion of global carbon budgets vs. reservoirs as greenhouse gas emitterâĂŤ specifically, there is no mention in the paper about the potential role of methane as a GHG source and it is somewhat implied that CO2 might be the dominant emission pathway even though it is generally accepted that methane is often the dominant GHG source on an CO2 equivalent basis. Thirdly, I think the authors need to better integrate the diel sampling component of their study into the way that the other results are analyzed. The authors don't mention the temporal sampling scheme employed during their 16 sampling campaignsâĂŤwere sites always sampled in the same order? Over what range of times? Are we confident that variation in fluxes measured is more a function of spatial variation than temporal variation? Fourth, while I think that hydrology may be a dominant control on reservoir CO2 emissions in this reservoir (e.g. it seems a completely valid and plausible hypothesis), I don't think the authors present enough evidence in support of this mechanism to present it as a result (e.g. in the abstract of the paper). Reservoir hydrology co-varies with other seasonal variation in temperature and the authors present no systematic approach for differentiating other possible controls. Finally, it is difficult to interpret the zonation groupingâĂŤthe authors should consider incorporating a statistical assessment of significant differences between sites. For example, were the riverine samples from both sites more similar to each other than to other sites? Or was one riverine site emitting CO2 at much higher rates than the other? Reservoir inlets are often hot spots for biogeochemical activityâĂŤare we sure that these riverine sites are fully riverine and that their hydrology isn't influenced by the dam? In addition to these scientific concerns, the manuscript needs to be edited for proper English. There are grammatical issues and vaguely written statements that could benefit from a third-party editor.

Line By Line Edits Page 1 Line 12: change "cycle" to "cycling" Page 1 Line 14: did

the authors use a statistical approach to see if reservoir emissions were significantly different by season? Page 1 Line 17: I don't think the analysis presented here conclusively linked CO2 emissions to physical mixing. Page 2 Lines 3-5: Carbon dioxide is generally thought of as the largest contributor to total carbon emissions, but methane is generally the largest contributor to total greenhouse gas emissions on a CO2 equivalent basis. I think the authors should be careful to make this distinction clear. Page 2 Line 18: By "biogeochemical processes of phytoplankton" do you just mean photosynthetic uptake? Page 2 Line 24: The way you have phrased this sentence makes it sound like all the studies you are citing were conducted in the Three Gorges Reservoir, but Pacheco et al. 2014 was in Brazil. Also, I don't see Tao 2017 listed in your references section. Page 2 Line 25: Do you mean watershed? Not waterbody? Page 3 Line 5: Why is information about Xiaowan Reservoir relevant here? Also, perhaps this is a good place to mention the construction of Miaowei Dam (which is noted in your Figure 1). Was the dam completed after your sampling ended in Dec 2016? Was the system hydrology affected at all by the fact that a dam was being constructed upstream during your study? Page 3 Line 9: Is this a hydropeaking (load following) reservoir? It might be nice to see water level data from the reservoir given the current discussion of water level fluctuation you have incorporated into your discussion. Page 3 Line 16 (and throughout): You use "mainstream" when I think you mean "mainstem". Page 4 Line 2: Consider reformulating the equation to take out unit conversion factors (which seem a little distracting and un-necessary). Page 4 lines 26-28: The authors discuss dam hydrology as if they don't know what type of spill practices are employed in the reservoir. Isn't this information available? Also, the height of reservoir spill (epilimnion versus hypolimnion) could be mentioned in the study area section. Page 5 line 3: Why do the authors feel that the dataset is limited? Is there reason to think that sometimes the running waters from inflow are not more aerobic than the reservoir water? Page 5 line 11: Change this sentence to something like "With the exception of one sample, the reservoir was consistently supersaturated with CO2, indicating its role as a CO2 source to the atmosphere" Page 5 Lines 20-24:

A plot that shows water level and point CO2 measurements over time might be helpful hereâĂŤI got a little lost in this description of the results. Page 7 Line 1-2: Where do the authors show this analysis? Right now there is no mention of a statistical analysis of drivers and no corresponding table or figure. Page 7 Lines 4-21: So, given these results, are you confident that the CO2 efflux measurements you made are still predominantly representing spatial (rather than temporal) variation? Also, it sounds like physical differences (rather than biology) may be driving the differential emissions you see during the day versus at night? Would you agree? Page 7 Line 24: How do you define a pristine river channel? Was R1 at all influenced by the construction of Miaowei Dam? How do you differentiate free-flowing river from reservoir inlet? Page 8 Lines 11-12: I don't think Figure 7 really shows this. Page 9 Line 8: Not sure "constraint" is the right word. Page 10 Line 17: Why "potential"? Page 10, Conclusion: No discussion about why emissions were so high from the river in the dry season. Was this pattern consistent in both river sites? Page 10, Line 31: What pattern are you referring to? Figure 6: Continuous versus incontinuous diel sampling was not explained in the methods.

Please also note the supplement to this comment:
https://www.biogeosciences-discuss.net/bg-2018-244/bg-2018-244-RC1-supplement.pdf

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