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Interactive comment on "Different regulation of CO_2 emission from streams and lakes" *by* S. Halbedel and M. Koschorreck

Anonymous Referee #3

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General comments:

The manuscript compares the evasion of CO2 from 4 streams and 2 reservoirs located in Central Germany, and it explores the factors controlling these rates. The topic is relevant for science in general and for the journal Biogeosciences and its audience in particular. Nonetheless, I have some general criticisms which are listed below:

The language is quite poor and confusing in many parts of the manuscript, and thus it would greatly benefit from a revision by a native English speaker. In addition, the manuscript seems too long in many parts, especially the results and discussion. The authors should provide more straight-forward messages based on their main findings and avoid too much speculation.

The title does not completely correspond with the contents of the paper. First, the relative importance rather than the factors per se seem to regulate CO2 emissions from the study sites. Second, the study investigates reservoirs and not lakes. Third, the results are restricted to a relatively small geographical area with specific characteristics. Therefore, I suggest changing the title to: “Regulation of CO2 emissions from lotic and lentic ecosystems in a Central European catchment”.

It is very unclear which data (e.g. collection site, number of observations, sampling time) are pooled and used for the comparisons shown in the different figures. This should be made very clear in the methods part of the manuscript. Right now it is too confusing. In relation to this issue, it is surprising that no results from the statistical comparisons among systems are shown throughout the manuscript (e.g. Wilcoxon tests).

I have great concerns about the way k is estimated in the reservoirs by assuming a constant wind velocity. This seems rather unacceptable. The authors should try to improve this by using additional data, or at least try to estimate the uncertainty associated with this assumption and incorporate it in their discussion.

The conclusions about metabolism being the main driver of CO2 evasion and the low importance of groundwater inputs seem a bit speculative based solely on the results from this manuscript.

The upscaling of CO2 emissions to the whole catchment is very important; however, it should be improved and more details on the methods used should be provided in the methods part of the manuscript. In relation to this, it seems rather poor to assume a mean wetted with of 4m for all streams in the catchment. There are many approaches that could be used to improve this (see some of the references of the manuscript). Second, the authors should provide measures of uncertainty and discuss this issue in the context of their study and other studies. Finally, the authors should at least discuss the issue of not including measures of spatial variability in their emission estimates,

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especially in the reservoirs (there is growing literature on this issue).

Specific comments:

P10023, L12-13: These references refer to measurements of reaeration in streams and not specifically to CO2 emissions. P10024, L22: Delete “be” P10024, L24: “turbulence” rather than “turbulences”. P10025, L11: I suggest avoiding “GHG” in this manuscript as much as possible since the only gas investigated is CO2. P10025, L11: Tone down this sentence. There are previous studies that have measured CO2 emissions form streams and lakes in the same cathment (e.g. Buffam et al. 2011 GCB 17: 1193-1211). P10027, L14: GFF filters usually have an approximate pore size of 0.7um. P10027, L22: Why were different methods used for estimating CO2 concentrations on the streams and the reservoirs? Which is the best method? What are the problems associated with each method? How comparable are both methods? P10029, L4: The mean wind speed of what? When and where these measurements made? Is there a meteorological station nearby from which wind measurements could be taken? I have great concerns about using this value for evasion calculations. P10030, L3: I could not find the results from the Wilcoxon tests anywhere in the manuscript. P100030, L24-25: These emission rates are just for the studied stream reaches and for the whole reservoirs? Please specify. Does it make sense to compare in this way these systems of different size? P100031, L1-2: Can you provide a reference for this assumption? P10034, L7: I suggest using the median instead of the mean consistently throughout the manuscript, especially if you are using non-parametric statistics and your data are not normally-distributed. P10034, L15-16: Confusing sentence. What do you mean? P10034, L17-23: These arguments against the contribution of GW are not completely convincing. Please expand. Also, the results do apparently not show a significant relationship between metabolic rates (GPP, ER) and CO2 emission rates. That would be a good argument for the important contribution of metabolism to CO2 evasion. There was also no significant correlation between CO2 emissions and DOC. Thus, the conclusion about metabolism being the main driver of CO2 evasion seems a bit speculative based

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solely on your results (see general comment). P10035, L22: According to previous sentences your values were high and not low compared to those by Allin et al. (2011). Please check. P10038, 15-25: Expand this part on upscaling and include parts in the methods section (see general comment). Table 1: Add standard deviations. Clarify if the measures are just for stream reaches and whole reservoirs? Figures 2, 3, 4 and 6: There are not just averages in this graph. Define the boxes (e.g. median, quartiles, outliers, etc.).

Interactive comment on Biogeosciences Discuss., 10, 10021, 2013.