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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 #2

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General comments

The manuscript explores the differences in parameters controlling CO2 evasion from streams and reservoirs and upscales this to assess the importance of each within the landscape. The topic is both relevant to the journal and of high scientific importance. There is growing recognition that evasion from water bodies is an important landscape flux that needs to be quantified, and the role of streams vs lakes/reservoirs is an interesting and important question given the generally higher emission rates from streams yet the usually smaller water surface area.

The title is slightly misleading, it appears to be a comparison of streams and reservoirs rather than lakes, although it is noted that the pre-dams are suitable model systems for lakes I don't feel its right to fully classify them as such. Presumably the shape of

the pre-dams is different from natural lakes e.g. significantly deeper at one end, the bottom substrate, I guess a lot of these factors are also dependent on the age of the dams? More information is required on the reservoirs.

Although generally well laid out, the manuscript and the discussion in particular I felt could be made more succinct without losing any information by simply tightening up the language and avoiding superfluous sentences. There are a number of places (some highlighted below) where the wording in sentences was unclear or the phrasing was usual. The manuscript would benefit significantly from having its use of English checked. There are a couple of key references I felt were highly relevant but missing, these have been listed below.

Specific comments

I am unsure of the accuracy of using a mean wind speed from manual measurements to calculate K in the reservoirs (see comments below). And following on from this I think it would be more accurate to consider differences in CO2 concentrations rather than evasion rates. Differences in K between streams could be considered separately but the reservoirs cannot be included as no actual measurements have been made. I do not see any benefit in stating that CO2 evasion is correlated with CO2 concentration when the concentration is itself one of only 2 parameters actually used in the evasion calculation, they are not independent so it is a circular argument and not statistically correct.

I also think it is important that when upscaling of evasion measurements is done it is always accompanied by an error term which it is not in this manuscript. I acknowledge the error is large and difficult to calculate but some attempt is essential if these values are to be published and systems compared.

Much of the discussion concerning the pre-dam surface concentrations (and subsequently fluxes) regards stratification. Presumably depth is important in this (though admittedly my expertise is more focussed on stream systems), in which case the bot-

tom profile of the reservoir is likely to be important, especially if it is sloping from very shallow at one end to deep adjacent to the dam. This needs to be discussed and the profiles described where possible given that sampling was only carried out at the deepest points in the reservoirs.

10025 Ln 15: It is not clear what a 'typical central European setting' is, more detail is needed or the sentence should be omitted

10026 Ln 1: It is unclear what is meant by 'rural' and how it differences from 'pristine'... I think you mean 'agricultural'? Furthermore I don't know that true 'pristine' catchments exist in central Europe given anthropogenic air pollution and enhanced N deposition, it's a technicality but I think the wording is important. Maybe 'unmanaged' or something similar would be a better term.

10026 Study sites: I recognise the information can be found elsewhere and references have been given but I think things like landuse are important enough to be described in this paper as well. I think a bit more information should be included here. Also include some meteorological information, i.e. you mention later that winter emissions are not included due to ice cover so information of the ice cover season should be included, min and max annual temperatures and precipitation also.

10026 13: biweekly or monthly from what date until what date... its unclear over what period this study is carried out and how many samples the analysis is based on

10026 15: What depths were samples collected and what depth was the reservoir

10026: Why was the same method for headspace collection not used for the reservoir as the lakes, an important difference seems to be equilibration at water temperature in the streams for 1 minute compared to equilibration in a lab for 30 minutes for the reservoir samples... has any test been done to calculate the difference due to method. Also was pressure at the different water depths accounted for when calculating reservoir concentrations in the profile?

10027 Streams: I am unclear how many times samples were collected from the streams. Were they just sampled twice (noon and before sunset on same day) for each season in 2011? It should be made much easier to find information of what was sampled, from when to when the sampling occurred, and at what sampling frequency.

10027 Ln 14: GF/F 45 μm pore size... is this correct? Usually GF/F refers to 0.7 μm pore size

10028: More detail needed, e.g. detection limits, name of DOC/TIC analsyser.

10028 Equation 1: In text the authors refer to the CO2 concentration in the surface water as C2water yet in the equation it is CO2water and the air equilibrated water is C0 in both text and equation, I cannot make sense of this notation... revise. Also no account of solubility is included in the flux calculation whereas most studies calculate flux based on water-air gradient times solubility times gas transfer coefficient?

10029: The wind speed equation assumes a certain fetch to allow wave/turbulence development. Given that reservoirs are usually long and thin in shape is there a sufficient fetch across all wind directions to achieve the k predicted by the equations? Also more information is needed describing where the wind speed was measured, was it measured at all reservoirs or just one? And why has the average been used, would it not be more accurate to use individual wind speed measurements with the associated concentration gradient and take the average emission over time rather than disassociating individual k and concentration measurements? I assume the lack of continuous wind speed introduces a significant uncertainty into the evasion calculations and presumably a significant underestimation given the shape of the windspeed relationship. Are no meteorological stations available nearby that manual measurements could be correlated to improve this accuracy? Perhaps even having wind speed data from nearby would allow an analysis of the wind speed frequency distribution, knowing the shape of the data distribution would allow a basic mathematical calculation of the error introduced when the wind speed equation is used?

10030 Ln 14: P-value for r = -0.79... is this significant?

10030: Throughout this paragraph results are repeatedly described as higher or lower but no statistics are presented for the reader to know if these are significant results or just observations.

10030 Ln 22: How were surface areas calculated? This is a major part of the upscaling and subject to significant error so should be acknowledged. Also it is unclear how many measurements these calculations are based on. I understand the need to upscale but it is vital that any calculation such as this include an error term which I suspect for these values is extremely large. A large error term does not diminish the results in any way, it simply highlights the difficulty in measuring this flux and the need for more work to be done in the area and I don't think the numbers should be published without it.

10031 Ln 10: With reference to Hassel CO2 concentrations it appears as though concentrations are actually similar to other sites in both spring and summer and it is only the fall concentrations that makes it significantly different from the others?

10031 Ln 12: the Rappbode did not have the highest evasion in spring, this was again the Hassel?

10032 Ln 6 (and elsewhere): when describing correlations or regressions include both r/r2 values and P values.

Table 2: CO2 concentration is used in the evasion calculation, the parameters are therefore not independent and do not meet the assumption of regression or correlation statistics. Furthermore, as k is held (almost) constant across reservoirs would it not make more sense to consider what drives differences in CO2 concentration directly rather than evasion. Differences between stream k values could be considered separately. A simple sensitivity analysis could then be carried out to see how changes in concentrations vs changes in k values contribute to the final evasion flux and the drivers of evasion considered this way.

10036 Ln 12: I don't think the data in this study allows conclusions to be made on the drivers of reservoir fluxes, only the drivers of reservoir surface CO2 concentrations. This comment should be considered throughout the discussion.

10037 Ln 19: Here the authors consider what controls the temporal variability in fluxes however the only analysis is based on spatial variability. It is not clear what this is based on as spatial and temporal drivers are likely to be very different.

10038 Ln 2: no correlation between Q and FCO2... again there is confusion over whether the authors are considering spatial or temporal variability which are likely to be driven by different things.

10038 L15: Why use this linear relationship rather than the actual concentrations and a summary of measured k values. Mean/median k could be used with known concentrations and the min and max used to produce a range of potential upscaled values?

Figure 7: As it is presented I do not think this figure provides any additional clarity or information more than simply writing 3 sentences. I assume the size of the dots and arrow relate to CO2 evasion but no axis or legend is available to show this.

Boxplots: I suggest adding letters to boxplots to show streams/reservoirs that are statistically similar or different e.g. use of statistic such as Tukey's family test.

Technical comments

10023 Ln 10: Mio t yr-1 ?

10027 Ln 13: I don't understand what the term 'with the wave' means, please clarify

10028 Ln 2: Are 'equilibrated air' and 'headspace gas' referring to the same thing, in which case why are different terms used?

10030 Ln 15: k value similar but not the same

10031 Ln 19: I don't understand what is meant by the term 'surface values'

In many cases the wording is unclear or the English not right which makes it difficult to read.... below are examples but I feel the whole manuscript would benefit from being reread and checked for its use of language.

10022 Ln 17: 'also physical factors control' should be 'physical factors also control'

10023 Ln 17 'Thereby seems the majority of the CO2 in lakes and streams to originate from organic terrestrial sources' should be 'It therefore seems that the majority of the CO2 in lakes and streams originates from terrestrial organic sources'; also it does not seem that this sentence follows on from the previous correctly?

10023 Ln 18: 'Originated' should be 'Derived'

10024 Ln26: 'and affects therefore also emission' should be 'and therefore also affects emission'

10025 Ln 5: 'We assume that the different regulation mechanisms are different relevant for lakes or streams'... this is incorrect English, its not clear what is meant.

10027 Ln 21: Do you mean 'regularly' rather than 'consistently'?

10030 Ln 10: 'which had significantly the highest CO2 concentrations in general', this is not good use of English

10031 Ln 4: 'A more detailed picture is given if looking on the seasonal changes' this is not good English

10032 Ln 27: 'Thereby had Hassel the highest TIC concentrations', this is not good English.

10034 Ln 14: 'Outreach', this is not the correct word and therefore the sentence is confusing.

10037 Ln 29: Change 'is generally under suspicion' to 'is generally considered'

Suggested References:

[Billett and Harvey, 2012] contains evasion rates and CO2 concentrations from a range of UK streams using the propane method, very relevant for comparison to the rates calculated here.

[Lundin et al., 2013] compares evasion from streams and lakes in a subarctic catchment

[Dinsmore et al., 2013] considers seasonal variability in CO2 concentrations from a UK stream

[Dawson et al., 2004] considers downstream changes in CO2 linking them to landscape characteristics

Billett, M. F., and F. H. Harvey (2012), Measurements of CO2 and CH4 evasion from UK peatland headwater streams, Biogeochemistry, 1-17.

Dawson, J. J. C., M. F. Billett, D. Hope, S. M. Palmer, and C. M. Deacon (2004), Sources and sinks of aquatic carbon in a peatland stream continuum, Biogeochemistry, 70(1), 71-92.

Dinsmore, K. J., M. F. Billett, and K. E. Dyson (2013), Temperature and precipitation drive temporal variability in aquatic carbon and GHG concentrations and fluxes in a peatland catchment, Global Change Biology, 19(7), 2133-2148.

Lundin, E. J., R. Giesler, A. Persson, M. S. Thompson, and J. Karlsson (2013), Integrating carbon emissions from lakes and streams in a subarctic catchment, Journal of Geophysical Research: Biogeosciences, n/a-n/a.

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