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Interactive comment on "CO_2 emissions from German drinking water reservoirs estimated from routine monitoring data" *by* H. Saidi and M. Koschorreck

Anonymous Referee #2

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This study investigates CO2 emission from German drinking water reservoirs, based on calculations of pCO2, estimations of the gas transfer coefficient, and the surface area of all German drinking water reservoirs.

While the study seems quite straightforward as to the data and methods, I have several severe concerns with this study. In its present state, I do not think it constitutes a valuable addition to Biogeosciences.

First, none of the aspects, findings or conclusions in this study are new. The only added knowledge is calculations of CO2 emission from a particular kind of reservoirs from one region, and fluxes were comparable to other reservoirs and other regions. Maybe because of that, the introduction does not manage to convincingly outline the

purpose of the study, and a hypothesis is lacking. This very much limits, in my opinion, the usefulness of this study.

Second, several of the conclusions and interpretations are not supported by the data. For example, the discussion of the importance of hourly CO2 emission dynamics is only based on hourly wind data (from met stations quite distant from the reservoir, i.e. not at the reservoir), but no hourly resolution in pCO2 data (assumption of constant pCO2 to calculate hourly flux). Another example is the interpretation that input of CO2 with river inflow is a more important CO2 source that internal production, which can not be made unless a C budget is established or internal production measured.

Thirdly, wind speed, which was used to estimate k from the Crusius & Wanninkhof 2003 empirical relationship, was measured at stations that were quite distant from the reservoirs (and maybe even on the top of mountains, as mentioned in the discussion). Hence, the resulting k may not be very representative of the k at the reservoir surface, making the resulting CO2 emission fluxes questionable. This applies particularly to the calculation of hourly CO2 emission; if the wind speed data are not representative for the site of the reservoir, the calculation of hourly fluxes is meaningless.

Lastly, the study concludes that since pH strongly correlated with CO2 emission flux, it can be used as a proxy for upscaling. Since the authors calculated pCO2 from pH and alkalinity, the correlation is simply a result of chemical equilibrium, and the correlation is based on Y variable that depends very strongly the X variable. This is statistically questionable. In addition, CO2 emission is estimated from pCO2 (in this study calculated from alkalinity, pH and temperature) and k; in this study, alkalinity and k were rather constant, leaving a strong influence of pH on CO2 emission. Any significant changes in either alkalinity or k would therefore very much weaken the relationship between pH and CO2 emission. This makes its use for upscaling very limited. Also, as the authors state, pH has very strong leverage on calculated pCO2 within a certain pH interval, and any uncertainties in pH measurement result in corresponding uncertainty in pCO2, further questioning the use of pH as a proxy for upscaling CO2 emission. I

therefore think that the statement to use pH as a proxy for upscaling CO2 emission is not substantiated, and may even be misleading researchers during future studies.

In addition, there are a few other aspects that in my opinion are problematic.

P1L20. This sentence invokes the impression that carbon budgets were established, which was not the case.

P2L17. The thin boundary layer approach is used to calculate CO2 exchange with the atmosphere, not for upscaling.

P2L24. While I agree that the majority of inland water pCO2 data in the literature are calculated and not directly measured, I think using the term “usually” is not appropriate. Direct measurement is vastly preferable to calculation of pCO2, given the strong effect of pH on the results, and particularly at low alkalinity

P5L24. It seems what was done here was interpolation rather than upscaling.

P6L10. Can these reservoirs in Germany really be assumed to be ice-covered during whole winter? Many lakes in Germany never freeze over. Maybe this argument could be backed up with water temperature data?

P6L19. The range in pCO2 is very wide, spanning 7 orders of magnitude. This seems, from my experience, unrealistic, and points towards that there may be outliers that could be attributable to the pCO2 calculations. Was there a cutoff for low alkalinities applied?

P8L11. The calculated median CO2 emission is comparable to the cited values, given the uncertainty in calcualtions and assumptions, not high.

P9L15. In many reservoirs, sedimentation is high, and degradation of sediment organic matter might be an important source of CO2 in reservoirs, maybe particularly during low flow (i.e. low DIC input from catchment) in winter. Low CO2 in spring does not indicate absence of under-ice accumulation since outgassing at ice-out is very rapid

and may not be captured by the sampling program.

P9L30. These calculations illustrated how important the gas exchange velocity k is when calculating CO2 emission flux. So indeed, flux is highly dependent on k, contrary to what is stated in this paragraph. It is evident from this paragraph of the discussion that with the available input data, the calculation of hourly CO2 emission is not warranted. Therefore, the entire section 4.2. is not relevant.

P11L9 and L 13. These statements are not supported by data.

P12L15-26. This statement is highly questionable, given the pH has such a strong leverage on calculated pCO2 (a very small change in pH can give a big change in pCO2), and that pH, in fact, is difficult to measure with high accuracy, particularly in soft waters. In addition, the observed relationship between pH and CO2 emission of this study is caused by chemical equilibria and near-constant k and alkalinity; in situations where k and alkalinity vary more strongly, the relationship would become much weaker. Giving a recommendation to use pH for upscaling could be highly misleading for future studies.

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