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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 #1

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Review of Saidi and Koschorreck CO2 emissions from German drinking water reservoirs estimated from routine monitoring data submitted to BG

In this paper, the authors use temperature, pH, and Total alkalinity (TA) monitoring data combined with wind speed data in order to calculate CO2 emissions from drinking water reservoirs in Germany. These reservoirs, represent all together a modest surface area of about 110 km2 for the whole Germany, that is, equivalent to that of Lake Müritz in Germany, but five times lower than lake Constance. One of the conclusions is that CO2 emissions from these drinking water reservoirs are negligible. The authors also attempt to relate the calculated CO2 fluxes with measured variables, and conclude that pH can be a good proxy for CO2 emissions.

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Discussion paper

I have two different types of criticism for this MS: one concerns the objectives of the study that is not well defined, and the speculative character of the interpretation of biogeochemical processes of these specific systems. The other one concerns the use of correlations between parameters that are not independent, but that in fact were calculated one from the other (as for instance the CO2 flux is calculated from pH) and some imprecisions on important aspects of aquatic chemistry.

First, the emissions being very small, this research would interest a large audience only if the MS could provide a detailed and original description of the question of drinking water in a broader biogeochemical context. The MS lacks from a clear definition of the objective of the study (other than using an available dataset, which at the moment appears as the only motivation for this work): is the topic here "drinking water CO2 emissions", is it "all reservoirs CO2 emissions"? "All inland water emissions?" When reading the MS it was not clear why focussing on drinking water reservoirs: if they are not significant CO2 source, at least are they special reservoirs? It also took me some time to understand that in Germany all these reservoirs receive only surface water and that the water is stored in reservoirs before being treated to make drinking water. With such high DOC values I guess this is not drinking water, and drinking water cannot contain live phytoplankton either as assumed in the MS. What's the interest in temperate region in storing surface water before treatment ? Most importantly, is it a general procedure found elsewhere? How do CO2 emission vary in reservoir as a function of the nature of the surface water used? What happens to water when pumped between the stream and the reservoir? How does this compare with other countries that use groundwater for drinking? And finally, how will water treatment affect the CO2 emissions? In the introduction and the Discussion, comparison is made indifferently with other reservoirs (hydroelectric or irrigation), or with natural lakes, without considering the specificity of each type of systems in terms of carbon source, or other biogeochemical drivers. This makes the problematic confusing. No time course of measured pH and TA and calculated pCO2 are shown, no relationship is found with water residence time, stream water characteristics etc... This makes the MS poor in terms of scientific

interest.

Second, the authors perform correlation between non-independent parameters that where calculated from each other. They calculate first the pCO_2 in the water (using pH and TA); and, second, FCO_2, the CO_2 flux at the water-air interface (using the water-air pCO_2 gradient and the wind-speed parameterization of Crucius and Wanninkhof 2003). No direct measurement of FCO_2 (that would have allowed the computation of K and thus an independent comparison with wind speed) were performed in this study. Later in the MS, correlations are found for instance between FCO_2 and pH, leading to the conclusion by the authors that pH can be defined as a "proxy" of FCO_2. This is a truism. Indeed, given the broad range in pH values, such observation is almost trivial, because low pH generates high calculated pCO_2 (that might in part be overestimated due to organic alkalinity), and thus cannot be used to predict anything, and does not constitute a scientific advance. Indeed, this correlation might be driven in parts by the presence of organic acids that generate an overestimation of the calculated pCO_2 at low pH. The maximum value of calculated CO_2 concentration was 11990 micromol L-1 (Page 6 L 19), that is a pCO_2 more than 300 000 ppmv. Can such high values be affected by the bias described by Abril et al. (2015)? How much the occurrence of the bias in pCO_2 calculation does affect the correlation between calculated pCO_2 and pH ? and the global estimation of the CO_2 flux as well.

There are also several uncertainties and imprecision in the way the gas transfer velocity was calculated, sometimes using wind speed data measured in the mountain to calculated gas exchange in reservoirs located in the valley more than 800 meters below (P9 L24-27) and no fetch effect (size of reservoirs) is considered in the parameterization as a function of wind speed.

Figures 3 and 5 have little interest as they correlate parameters that are not fully independent.

Detailed comments In the intro P2L8-12 Why do drinking water reservoirs have a low

trophic state? First paragraph mixes drinking water and other reservoirs. What are their differences?

P2L24. In fact direct measurements of CO_2 concentrations exist. The problem of pCO_2 calculation in low pH low alkalinity waters should be mentioned in the intro. P3L3. The concentration of CO_2 was not monitored. Monitored parameters were pH and TA.

P3L8 "by applying simple regression analysis... or by the CO_2 concentration." You don't need to make such "simple regression" because FCO_2 is explicitly a function of CO_2 concentration and wind speed. L13 "in other regions... catchment". DIC input from catchment is important whatever the region. P8L13 "possible explanation is the high impact of stream quality on the drinking water reservoirs, caused by typically low water residence time". This statement is vague. This should have been discussed with much more details. CO_2 emissions should have been analysed in terms of surface water origin and residence time of water in reservoir. L16 : "because of better quality, drinking water reservoirs are preferably located in upstream areas with higher stream pCO_2. This is supported by our observation of higher CO_2 concentrations occurring often in small reservoirs". This is very speculative: are small reservoirs necessarily connected to small streams and large reservoirs to large rivers ? I would make an alternative speculation: CO_2 concentration is higher in small reservoir because they have lower fetch, which limits gas exchange. Do the authors have evidence for their interpretation being less speculative than mine?