

Interactive comment on “CO₂ emissions from German drinking water reservoirs estimated from routine monitoring data” by H. Saidi and M. Koschorreck

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Reply to reviewer 1:

We would like to thankfully acknowledge Referee 1 for providing valuable comments that will significantly contribute to the improvement of our paper. It was indeed our principal goal to get a first estimate of CO₂ emissions from German drinking water reservoirs – not reservoirs in general or a worldwide perspective. This was mostly driven by the fact, that the literature is somewhat biased towards boreal lakes and boreal and tropical hydropower reservoirs. Doing this we faced some practical problems (mostly linked to data availability) like: how to do temporal interpolation? How to deal with heterogeneous data frequency? Is it a problem if no on-site wind data are avail-

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able? We thought getting such data from drinking water reservoirs and addressing these practical issues would be interesting enough to be published as a paper. Our data do not really allow a discussion about the role of drivers, global upscaling or the general biogeochemistry or function of drinking water reservoirs or drinking water treatment. From the reviewers comment we conclude that our objectives are probably not interesting enough to attract an international audience. We could include some further analysis of possible drivers like trophic state or catchment characteristics into our paper. However, such further data analysis would need a complete new story and a completely new manuscript. It was our hope to have this first paper covering methodology of flux determination and providing a nation-wide upscaling. We would prefer to publish the detailed discussion of drivers like water quality or catchment characteristics in a future second paper.

The obvious problem that the resulting fluxes were calculated from pH values and are thus not independent from pH was raised by all reviewers. Actually it is clear that the pH is mainly controlled by pCO₂ in these waters. It is also clear that the flux is also calculated from and thus, not independent from wind speed. However, our results show that inter-reservoir variability was primarily governed by pCO₂ (and thus, pH) and not wind. This is because variability in pCO₂ was higher than variability in wind speed. We think that this is useful information and not meaningless, as stated also by the other reviewers. It shows that the exact quantification of k₆₀₀ is probably less important when comparing different reservoirs. We consider this point also important, because, the correlation with pH was extremely good and because although a similar dependence was observed by several other studies, this point has rarely been addressed. Studies which correlate CO₂ fluxes with lake or catchment variables frequently come up with the observation, that pH was the best predictor of the CO₂ flux. We think it is worthwhile to discuss the usefulness of this finding with respect to upscaling. As correctly stated by reviewer 2, the exact shape of the flux-pH curve depends on alkalinity and k of the particular systems. Thus, our relationship of course cannot be directly transferred to other systems. In a revised manuscript we would do a more detailed comparison with

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similar curves from the literature to explore the transferability of our results. A weak point is indeed that we did not properly address the possible role of organic acids. In a revised manuscript we would try to do some error estimates based on our DOC data, pH and literature information. We would like to keep the pH relation in the paper, but since it was criticized by all reviewers, we would put less emphasis on it.

Another important point, also raised by the other reviewers, is the quality of our wind data. Of course it would be nice to have site specific calibration of the wind speed–k600 relation, wind data measured on each reservoir and direct pCO₂ measurements. We are trapped here somehow between detailed case studies which have all these data and global studies which sometimes even use a mean wind speed or wind data obtained from models. Since there are no on-site long term wind data available for all our reservoirs, we see no better method than the one we applied. We consider this not a serious problem, because we could show that considering short term wind fluctuation would not change our results dramatically. We may have a systematic over estimation of the flux due to an over estimation of wind speed due to the location of our weather stations. The uncertainties related to wind data are already addressed in the discussion section. In a revised version of the manuscript we would try to better quantify the possible error resulting from our wind data. However, an in depth discussion of this issue is probably not very productive given the generally observed poor parametrization of U₁₀ at low wind and the non-consideration of thermal advection. The regulation of k600 needs to be studied in more detail in case studies, which then could address also points raised by the reviewer, like influence of fetch or reservoir size.

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