**Biogeosciences
Discussions**

Interactive
Comment

Interactive comment on "Greenhouse gas emissions
(CO$_2$, CH$_4$ and N$_2$O) from perialpine and alpine hydropower reservoirs" *by* T. Diem et al.

Anonymous Referee #3

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Review of the manuscript "Greenhouse gas emissions (CO2, CH4 and N2O) from perialpine and alpine hydropower reservoirs" by T. Diem, S. Koch, S. Schwarzenbach, B. Wehrli, and C. J. Schubert

Ivan Bergier has already provided a review. In general I agree with his comments that there are some problems with the study (I have tried to not repeat his comments). However, some results are relatively robust and my advice is to focus on these parts while excluding or tone down the more problematic parts. I will try to specify what I mean below.

General comments This manuscript includes concentration measurements of N2O,

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CO2, and CH4, as well as 13C signature estimates for CH4 in 11 reservoirs at different latitudes and at different seasons. Measurements were made along depth profiles in the water and sometimes in the sediment, and in inflow and outflow water. Based on these measurements several statements are made. Those I find convincing are:

1. Emissions of N2O were low or absent

2. CO2 was the primary greenhouse gas emitted.

3. Diffusive fluxes of CH4 from sub-alpine and alpine reservoirs are small compared to lowland reservoirs and reservoirs in boreal and tropical environments.

A rewritten version of the manuscript could focus on this.

Some other parts with unclear support or with methodological problems (in addition to the pints made in the other review) are:

a. Ebullition is claimed to be non-important except in one lowland reservoir. The ebullition part seems to have methodological problems. Apparently ebullition was measured just during a few hours in each reservoir. This is inadequate because ebullition is episodic with large fluxes during short events and periods without ebullition between those events. Hence, the potential ebullition events may have been missed in most reservoirs but captured by coincidence in the lowland reservoir. Ebullition is also strongly depth dependent with more ebulltion form shallow regions and it is unclear at what depth ebullition was measured in the different reservoirs. Measurements at the deepest point will hardly be comparable since the reservoirs varied widely in terms of depth. Hence the ebullition part has to be clarified or should be omitted.

b. It is stated that sediments did not contribute CH4 to the water or to emissions in many reservoirs but that CH4 coming with the inflow water supported all the emission. I do not understand this. Even if most CH4 is oxidized in surface sediments before entering the water (as indicated by the 13C signatures and the relatively low concentrations) the contribution from the sediments may contribute enough to yield the supersaturation

which drives most of the emissions. I do not understand how this explanation can be ruled out. It does of course not mean that CH4 from the deepest level is most important - I would rather expect lateral transport from marginal sediments in each layer. A large contribution by the inflow also seems strange unless the inflow volume is large relative to the reservoir volume and the reader is not given enough information to evaluate the statements made regarding this (see also specific comments regarding table 2).

In general I also lack information about chemistry of potential importance. This includes water chemistry (e.g. DOC, absorbance or color, total P and N), but also catchment area characteristics such as if the bedrock was rich in carbonates potentially explaining the DIC/CO2 concentrations/emissions in the reservoirs.

It is unclear if units per m2 reflect whole reservoir area integration or just per m2 of the water column where samples were taken. This is very important and totally determine if weight is given to basin morphometry or not..

When fluxes are given as mg m-2 d-1 it would be good to specify if you mean mg CH4/CO2 m-2 d-1 or mg C m-2 d-1.

The final general comment is that I think the manuscript dwells too much on specific situations in specific reservoirs. While important method aspects are not detailed enough, some results are presented in more detail than needed. Why not focus on the broad robust patters that could be generally valid and give less attention to specific details (unless they lead to important conclusions of course).

Specific comments

P3703 L5-8: Was it tested if some gas was lost from the water during the sampling process when taking up water in a water sampler from substantial depths? It seems likely that pressure changes could cause significant degassing. If this was not tested, surface concentration measurements should be more robust than measurements from deep water.

P3703 L19-22: I do not understand this error analysis. Please, clarify.

P3710 L17-19: "Assuming the area of intense bubble flux to be approximately one third of the reservoir, preliminary ebullition fluxes for Lake Wohlen would be 300ś230mgCH4 m-2 d.1" Why was this assumption made? How can it be supported and evaluated? (but see also general comment about ebullition).

P3713 L10-14: Some of these relationships are unclear. How did the relationship between CO2 flux and date look like? Was pH also related to date? Why would date be related with elevation (which presumably is constant over time)...or do you mean water level instead of elevation? Show all important relationships to give the reader a chance to understand them better.

P3715 L12: I could not find Suomis et al. 2004 in the reference list.

P2714 L4-6: "There is no production of N2O in 5 the deep alpine reservoirs we sampled and we assume calculated fluxes are probably overestimated." How is his known? Has N2O production been measured? Concentrations do not say anything about rates or dynamics.

Section 5.2.1

This discussion about the loss at turbines is unclear. First it is said that the average loss from the three reservoirs where a loss was detected were 46 %. Later on it is claimed that the average loss from the turbines from sub-alpine and alpine reservoirs were 51 %. I do not understand and this has to be clarified.

A way to double check the maximal amount that can be lost in the turbines would be to assume that the water reach equilibrium concentration in the turbines (i.e. the supersaturation is lost and saturation is yielded after the turbines. Thus, given the concentration and the water discharge through the turbines the maximum amount that could be degassed can be calculated.

Section 5.2.2

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Second sentence: Why would the water residence time cause a high input of OM? The residence time in itself is not important unless you mean a high inflow of water having high POC or DOC content. What were the OM levels in water end sediments compared to the other reservoirs? What were the sources?

Last sentence: I think ebullition and water concentrations may not always be coupled. Frequent bubbling may cause increased concentrations (McGinnis 2005), but between the bubbling events surface water concentrations depend on many other things such as piston velocity.

Table 2: The choice of different units makes it difficult to evaluate the table content. I suggest expressing concentrations also as μg m-2 reservoir surface corresponding to the flux unit and also to provide residence time calculations regarding the CH4 in the layer being enough well mixed to exchange CH4 with the atmosphere. This is needed to support the statements that river inflow can support all the emission in many reservoirs (which seems strange unless the inflow volume is large relative to the reservoir volume).

Table 3: It has to be specified what kind of emission was considered on the different studies. Diffusive emission only?

Interactive comment on Biogeosciences Discuss., 5, 3699, 2008.

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