

Interactive comment on “Methane and nitrous oxide sources and emissions in a subtropical freshwater reservoir, south east Queensland, Australia” by K. Sturm et al.

Anonymous Referee #1

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The presented manuscript describes measurements of CH₄ and N₂O surface emissions, water column concentrations and sediment-water-fluxes of a sub-tropical reservoir. The measurements were made at two sites during one sampling campaign (March 2012). The authors show that there was CH₄ and N₂O evasion from the reservoir to the atmosphere. I agree to this statement, but I have concern regarding the representativeness of the measured values and hence, the quality of the results. The following points lead to this concern:

- The spatial variability was not resolved adequately, since only 2 sampling sites were used, one for the deeper and one for the shallow water zone. Therefore spatial replication is missing. This point is extremely important since a co-author of this paper

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showed in another publication that large errors can arise if the variability is not taken into account (Grinham et al., “Quantification of ebullitive and diffusive methane release to atmosphere from a water storage”, Atmospheric Environment 2011).

- Since measurements were made during one campaign, they represent only point measurements. In reservoirs, it was shown that several environmental factors can affect flux rates significantly, e.g. temperature or water level changes. And since the water level in this reservoirs changes, e.g. due to strong precipitation events as indicated in the Methods section (p. 19789, line 2), it can be expected that the flux rates also show strong temporal variations.

The methods used to measure the fluxes and concentrations are well established techniques, except of the porewater GHG measurements. The use of Falcon tubes in combination with centrifugation needs to be better described and it is necessary to show, that there was no leakage of gases, since Falcon tubes are not designed to be gas-tight.

The text of the manuscript is well written, but the following points need to be described:

- P. 1949, l. 13-24: How many measurements were made when and where? A table could help to illustrate this to the reader. - P. 19490, l. 15: Which piston velocity was used? A reference should be added. - P. 19491, 1-9: How was the equilibration made? This should be described in more detail or a reference should be added. This should be also made for the porewater concentration measurements. - P. 19493, l. 6-8: How was ebullition considered? Also in incubated sediment cores, gas bubbles can be released and affect the flux rates. - P. 19495. L 1-11: The hypolimnion CH₄ concentrations are higher at the deep site than the porewater concentration at the lower site. Does this indicate that there must be higher porewater concentration at the deep site? (Otherwise there would be a flux from the hypolimnion to the sediment.) This should be discussed.

The figures of the manuscript show the values, but since a logarithmic y-axis was chosen, e.g for 2, the large differences between days cannot be inferred good enough.

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The use of a linear scale could help to illustrate the differences better. In the discussion, it would help also to discuss these differences with respect to the precision of the measurements indicated by the error bars.

Interactive comment on Biogeosciences Discuss., 10, 19485, 2013.

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