Biogeosciences Discuss.,
https://doi.org/10.5194/bg-2017-412-RC2, 2017

Interactive comment on "High-frequency productivity estimates for a lake from free-water CO_2 concentration measurements" *by* Maria Provenzale et al.

Anonymous Referee #2

Received and published: 19 December 2017

The manuscript "High-frequency productivity estimates for a lake from free-water CO2 concentration measurements" presents a method to assess NEP in aquatic ecosystems. While interesting, the method requires an independent measurement of (1) the flux of CO2 between the atmosphere and the water surface, (2) usage of high-frequency in situ CO2 sensors, and - at least in the present approach – (3) conditions under which lateral advection fluxes (both within the water column and in the atmosphere) are limited. Overall, the methodological approach and science appear sound, but of limited utility due to known issues with eddy covariance and chamber methods for determining water to atmosphere fluxes of CO2. That is, the atmospheric turbulence required for eddy flux determination is often not present at night, while the stratification required for the water column (i.e. to satisfy the assumption of no lateral fluxes in the lake) would be violated under higher windspeeds. Thus, the overall measurements are constrained to a methodological "sweet spot". The authors do not explore the potential limitations of only making measurements during these ideal conditions, which were identified for 10 summer days over a number of years for a lake in Finland. While the results for the measured days are interesting, there should be some effort to describe what these NEP rates represent relative to the other seasons (early open water after thaw, etc.), as well as efforts towards uncertainty estimates of the NEP rates and how this uncertainty cascades into the least-squares regressions for modeled parameters.

The authors present in essence a case study of implementation of a method presented by Hari et al (2008), with the suggestion that the method has been overlooked and has not been used for NEP because of limited testing (P 2 L35 – P3 L1). This logic seems a bit circular, and misses the point that there are few eddy covariance studies over aquatic systems. The authors suggest that determination of the atmospheric flux of CO2 could be made with chambers rather than by eddy flux, but do not discuss limitations of chamber measurements, which are not insignificant. Some discussion on how chamber measurements and in situ measurements could be co-located would be useful. As well, the study makes the assumption that [CO2] is uniform in the mixed layer, but this assumption does not appear to have been tested for confirmation.

It seems problematic that the authors calculate NEP based on time varying dC/dt, but use mean values for daytime and nighttime fluxes of the atmospheric flux (essentially static values). Perhaps there is additional information in the eddy flux data that could be used to propagate uncertainty in NEP calculations? For example, the standard deviation of the Fa term for each day could be useful. The authors state (P6 L13) that the CO2 flux is expected to have similar daily cycles across the analysed days, but it is not clear that the magnitude of the fluxes should be similar across days. What is the basis for this assumption?

Specific comments P1 L11: Here, the model fit is described as "excellent", while later it is described as “very good” on P7 L28. Providing some metrics that would qualify as excellent should be included in the abstract.

P1 L19: change to “. . .in gaseous form (primarily as CO2).”

P4 L5: What is the permeability of silicone to CO2 relative to the diffusion rate of CO2 in water at the temperatures experienced in this study?

P6 L29: It would be helpful to present more information describing how pmax, b and ro are determined. Which equations were used to solved for these three unknowns?

P7 L26: “The curves in Figg. 2-5 have the expected trends, and this confirms that. . .” – this sounds like confirmation bias.

P7 L29: “This clearly indicates that the method used here allows the NEP to be accurately parameterized as a function of irradiance and water temperature.” What seems to be missing here is uncertainty assessment on NEP. If NEP is not well constrained (since it is calculated from Eqn 3 assuming static rates for the daytime and nighttime CO2 fluxes between the lake and the atmosphere), how can the model fits be characterized without consideration of the uncertainty in the “measured” NEP vs. the modelled fit?

P8 L10: “The value of b does not change significantly between any of the years” – but Table 1 and 2 show it to vary by 50% between years. This seems rather significant. The later statement that pmax and ro are more sensitive to variation seems to be a statement that wasn’t formally tested through sensitivity analysis.

P9 L14: “We hope in the future to further develop the method” – this kind of statement doesn’t belong in a Discussion section.

Compare P9 L6-7: “the changes of PAR and water temperature cannot fully account for the changes in the model parameters” with P7 L29 “the method used here allows the NEP to be accurately parameterized as a function of irradiance and water temperature.”

P11 L7: I would not describe eddy covariance over a water surface as “relatively inexpensive”.

P17 Fig 3 for 2014: What explains the large separation between NEP values for low values of PAR and the jump in NEP just as PAR increases a bit?

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