Author comments (ACs) to the referee comments (RCs) on the manuscript "High-frequency productivity estimates for a lake from free-water CO_2 concentration measurements", by Provenzale et al. (bg-2017-412) – RC2

The authors would like to thank the referees for the time they invested in reading and assessing the manuscript, and for the comments they provided. We believe that the feedback from the referees has allowed us to substantially improve our work. The authors would also like to thank the editor for the time and consideration.

RC2

The manuscript "High-frequency productivity estimates for a lake from free-water CO_2 concentration measurements" presents a method to assess NEP in aquatic ecosystems. While interesting, the method requires an independent measurement of (1) the flux of CO_2 between the atmosphere and the water surface, (2) usage of high-frequency in situ CO_2 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 CO_2. 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 wind speeds. 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.
We thank the referee for the good points. We tried to follow the suggestion to describe what the NEP rates represent relative to the other seasons and to estimate the uncertainty in the NEP rates. For the first part, the manuscript now reads: "In our case, for example, the NEP rates and hence the parameters are representative of the late summer. In lake Kuivajärvi, where diatoms are abundant, it can be expected for the productivity to have a peak in the spring and another smaller peak in the autumn, at the turnover. More measurements at those times would be needed, in order to understand whether the parameterization is still valid under those conditions."
For the second part, see the answer to the later comment about NEP uncertainty.
We would like to point out that the ideal conditions were identified in 40 days, not 10, as is specified in the manuscript.

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 CO_2 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 CO_2 is uniform in the mixed layer, but this assumption does not appear to have been tested for confirmation.
We added a sentence about the potential benefit of co-locating chamber measurements and in situ measurement. However, we would like to stress that this manuscript is focused on a direct way to measure the CO_2 concentration in the water and on the equations used to calculate the NEP from

these measurements. The flux between the lake and the atmosphere is needed in order to close the mass balance, but the methodology used to measure it (and hence a comparison of the possible methods) is beyond the scope of this paper. We tried to make this clearer in the manuscript. The manuscript now reads: "The calculations could be improved with a better EC data set. Different methods could also be adopted to estimate the flux between the lake and the atmosphere. Chamber measurements could be used, but the time resolution could be an issue. They would need to be performed regularly. They could, however, be used to integrate the EC data set for example. Surface renewal models could also be used (e.g. Heiskanen et al. (2014)). For further information on the comparison between different flux measurement methods, see Erkkilä et al. (2018).". Regarding the uniformity of CO_2 in the mixed layer, for years 2010 and 2011 we had a second CO_2 probe at 0.5 m, whose readings matched the ones from the probe at 0.2 m. We added this information to the manuscript: "For years 2010 and 2011, another CO_2 probe was located at a depth of 0.5 m, and its readings were consistent with those from the probe at 0.2 m, hence showing homogeneous CO_2 concentrations in the mixed layer.".

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 F_a term for each day could be useful.
We agree. We calculated the uncertainty on the average values of F_a. We decided not to use the standard deviation, since the 30-min EC data are characterised, as often happens, by large scatter. Instead, we recalculated the averages randomly selecting only half of the available data, and then we repeated the process 100 times. We then checked how far apart the calculated average values were. We added this to the manuscript, which now reads: "We also estimated the uncertainties on the daytime and nighttime average values of F_a. We decided not to use the standard deviation, since individual 30-min data EC data are characterised by significant scatter. Instead, we recalculated the daytime and nighttime averages randomly choosing only half of the data in the sample, and repeated the process 100 times. Then we checked how far apart the minimum and maximum average values we obtained were, and used that as uncertainty.".

The authors state (P6 L13) that the CO_2 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?
We agree that it was not written clearly in the manuscript. It is not an assumption, but an observation, from analysing the available EC data for the studied years, and the data from years with more complete EC data sets. We rephrased it in the manuscript, which now reads: "Under these circumstances (i.e. warm and sunny summer days without strong wind events), the CO_2 flux is expected to have similar daily cycles across the studied days, as is shown by the available EC data and by the EC data from years with more complete data sets.".

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.
We agree. We changed "excellent" to very good in the abstract, and provided some metrics ($R^2 \geq 0.71$).

P1 L19: change to ". . .in gaseous form (primarily as CO_2)."
We changed it.

P4 L5: What is the permeability of silicone to CO_2 relative to the diffusion rate of CO_2 in water at the temperatures experienced in this study?

Laboratory tests on the same set-up were run for the original paper (Hari et al. (2008)). When the silicone tube was transferred rapidly from a water bath with low CO_2 concentration to one enriched in CO_2, the response time of the whole system was < 5 min (Hari et al., 2008).

P6 L29: It would be helpful to present more information describing how p_{max}, b and r_0 are determined. Which equations were used to solved for these three unknowns?
The parameter values are obtained fitting the model to the data. We added this sentence to the manuscript, which now reads: "their values can be obtained fitting the model to the data.". Also see, in the manuscript, "After calculating the NEP, we plotted the NEP versus irradiance curves. We then fitted the model (Eq. (5)) to the NEP data with the least-squares fitting method, in order to check the agreement between the data and the model and in order to estimate p_{max}, b and r_0."

P7 L26: "The curves in Figg. 2-5 have the expected trends, and this confirms that. . ." – this sounds like confirmation bias.
"confirms" was changed to "suggests".

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 CO_2 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?
We agree. We removed "accurately", and we addressed this issue in the Discussion session. The manuscript reads: "Our analysis was hindered by issues in the EC data set: due to inherent EC limitations and technical problems, the data set had many gaps and average daytime and nighttime F_a values had to be used. The relative uncertainty on them was, on average, 50%. This uncertainty propagates to NEP through Eq. (3), and therefore to the parameter values as well. However, it does not undermine the good agreement between the model and the data, given that the average F_a values were calculated putting together all the periods of the same year. Therefore, each NEP data point has the same uncertainty and the same weight in the 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 p_{max} and r_0 are more sensitive to variation seems to be a statement that wasn't formally tested through sensitivity analysis.
We performed a statistical test to check whether the variations in the parameter values between the years were statistically significant. See P8 L7-10 in the original manuscript for the description of the test. The changes in the value of b are indeed large but so is the uncertainty on the value of b itself, which makes these changes not statistically significant.

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

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."
The method does allow the NEP to be parameterized as a function of PAR and water T, through the calculation of the model parameters. The model parameters change between the years, and their changes are not fully explained solely by the changes in water T and PAR, indicating that they depend on other variables as well, such as the algal community composition for example. We

rephrased it to make it clearer. The sentence from P7 L29 now reads: "From what is said so far, the changes of PAR and water temperature alone cannot fully account for the changes in the model parameters. The long-term variations of the parameters probably have other drivers too, such as the composition of the algal communities".

P11 L7: I would not describe eddy covariance over a water surface as "relatively inexpensive".
"relatively inexpensive" refers to the CO_2 probes. The next sentence in fact reads: "The method requires at least a concomitant estimation of the CO_2 flux from the lake to the atmosphere. In our case the EC technique was used, which is expensive and can be laborious in the data processing phase. However, chamber measurements or surface renewal models could be equally good options.".

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?
The large separation is explained by choosing a PAR threshold between "night" and "day", and then having different F_a average values for night and day. We made it clearer in the manuscript, which now reads: "Note that both in Fig. 3 and Fig. 4 and especially for years 2010 and 2014 there is a large separation between NEP across the chosen PAR threshold between night and day. This is caused by having to resort to daytime and nighttime average values for F_a.".