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) – RC1

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.

RC1

Provenzale and co-authors describe a study in which they use high-frequency CO_2 measurements to estimate lake net ecosystem production (NEP) and describe variation in NEP using water temperature and light in the upper mixed layer. The authors constrained their study to well-stratified periods so as to satisfy assumptions of CO_2 transport in the lake. Michaelis-Menten model fits to the estimated NEP were very good and parameters varied between years, for reasons which the authors speculate (e.g. algal community composition). This study advances CO_2 metabolism research in lakes; however, I think the results and discussion sections need a lot of restructuring to effectively describe the important results of this study. I make several suggestions below that I think will improve the manuscript.
We thank the referee for the suggestions. We followed them to restructure the Results and Discussion sections.

Hanson et al. (2003, Limnology & Oceanography 48: 1112-1119) also estimated GPP, R, and NEP using CO_2 measurements for 2-4 days in each of their study lakes, so this isn't the first time CO_2 measurements have been used for metabolism studies other than Hari et al. (2008).
We added this information to the manuscript. The manuscript now reads: "our study is an important step towards testing and developing the approach so that it becomes more general, also given the scarcity or even lack of high-frequency direct CO_2 measurements for productivity studies (we are aware of only one other study where free-water CO_2 measurements were used for metabolism studies (Hanson et al., 2013).".

Do the authors have evidence that lateral transport of CO_2 negligible in their study lake? Are there stream or groundwater inflows to the lake that may transport CO_2? Even in lakes that are fairly isolated from the surrounding landscape, lateral transport of CO_2 can be significant. (see Vachon et al. 2016 doi: 10.1002/lno.10454), and some autotrophic lakes can have significant CO_2 outgassing, indicating a decoupling from NEP and CO_2 dynamics (Bogard and del Giorgio 2016 doi: 10.1002/2016GB005463). More details on lake hydrologic characteristics and the influence of lateral transport or lack thereof would be good. What is the lake water residence time? Are there significant inlet streams? Etc..
We added the hydrologic characteristics of the lake. The manuscript now reads: "Most of the inflow is through a permanent stream in the northern end, while the role of groundwater is small during summer. Temporary inflows appear at snowmelt, through several small ephemeral streams. The outflow is located at the southern end. The residence time was 522 days in 2011 and 655 days in 2013.".
As for the lateral transport, Dinsmore et al. (2013) studied the CO_2 concentration discharge in six sites, including lake Kuivajärvi. They concluded that most of the CO_2 discharge for this site happens at snowmelt or during strong rain events in the autumn. The assumption that lateral transport of CO_2 is, under our conditions, negligible is also confirmed by the fact that the CO_2 concentrations in the mixed layer only exhibit a diurnal cycle, and no long-term trend is observable (see the Figures in the supplemental information). We added this information in the manuscript,

which now reads: "The lateral transport of CO_2 had to be ruled for the sake of the calculations. A similar challenge is encountered in forest ecology studies as well, where the lateral transport in the air (advection) is also usually neglected. We are of course fully aware of the lake being a 3D dynamic system. Besides, since this study focuses on the summer periods when the lake was stably stratified and there were no high winds or rains, the lateral transport is not expected to play a significant role here. This assumption is supported by Dinsmore et al. (2013), who showed that for lake Kuivajärvi most of the CO_2 discharge happens at snowmelt or during heavy rains in the autumn. It is also supported by the mixed layer CO_2 concentration time series, which show no sign of a long-term trend on top of the diurnal cycles (see Figg. S5-S14 in the supplemental information).".

I also think it would be useful to indicate when or for what type of lakes that this method might be useful. This is the second lake that has tested this method, but do the authors think that this method can be applied to every lake? If not, then why not?
In principle, we think that the method could be applied to any lake under any conditions, with an expansion of the instrumental set-up. We added this information to the manuscript, which now reads: "At the current stage, the method we present here is still very system specific, and assumptions about lateral and vertical CO_2 exchange and photo-oxidation had to be made (negligible lateral exchange and photo-oxidation, no in-lake vertical exchange). However, the method can in principle be applied to any lake and under any condition, with an expansion of the instrumental set-up. Measurements or estimates of F_u, the CO_2 flux from the deeper layer to the surface layer of the lake, would be needed in order not to limit the analysis to isothermal (as in Hari et al., (2008)) or stable stratification (as here) conditions. This could be achieved for example adding water column turbulence measurements to the CO_2 concentration and temperature measurements. Chemical measurements would be needed to apply the method in clear-water lakes, where photo-oxidation could play an important role. Finally, information about CO_2 discharge would be needed for lakes or periods when lateral transport is not negligible.".

Page 4 and 5: NEP is the net biological conversion of organic carbon to inorganic carbon while NEE is equal to NEP + inorganic sinks/sources of CO_2. So I think it is incorrect to state that negative NEE is the same as NEP on page 4 line 30. See Lovett et al. (2006 doi: 10.1007/S10021-005-0036-3) for an in-depth discussion of terminology and Stets et al. (2009, doi: 10.1029/2008JG000783) for an application to lakes. I think it is correct to say that NEP = -NEE if lateral transport of CO_2 (and other inorganic sinks/sources) are negligible, so equation 3 seems correct, but only under this assumption.
We agree. The manuscript now reads: "Provided that there are no inorganic sinks or sources of CO_2, the NEP is the opposite of the net ecosystem exchange (NEE).".

I don't see how this manuscript harmonizes terrestrial vs. aquatic studies other than using a similar term (M-M dynamics as harmonizing isn't very convincing). And the authors don't give any good reasons for why harmonizing terrestrial and aquatic C cycling research is needed. Sprinkling in forest ecology references here and there (e.g. page 7 line 8, Figure 2 legend) seems like a cheap connection to make to terrestrial systems. I think this paper would be stronger if the authors did not try to compare to terrestrial systems and instead focused on the merits of using CO_2 in addition to or in place of O_2 to estimate metabolism in aquatic systems (e.g. respiratory quotient different than 1, etc. . .).
We agree that we did not clearly state what we meant. Our effort was to harmonize the procedures that are used to calculate productivity from measurements in different ecosystems (and not specifically M-M dynamics, which we used here as a validation for our calculated NEP, together with other models (Smith (1936) and Jassby and Platt (1976), see the supplemental information)). We stated our intentions more clearly. Also see the answer to the Page 5 lines 19-27 comment.

Page 2 line 29: What do you mean “the NEP was not mathematically parameterized” in the Hari et al. (2008) analysis? Does this mean that NEP was not explained by PI curves?
A PI curve is reported in Hari et al. (2008) Fig. 4, with the data points and a modelled NEP curve. However, the mathematical expression of the modelled NEP curve is not provided, and neither is information on its agreement with the data.

Page 5 line 7: It is unclear if h_{mix} was set at 1.5m for the entire study period (due to stable stratification and setting F_u to zero) or if this is calculated at the frequency of the temperature measurements. If it is calculated at a high-frequency interval, do the authors account for vertical entrainment of hypo CO_2 into epi when thermocline deepens and epi CO_2 into hypo when thermocline shallows?
h_{mix} was set to 1.5 m for the entire study period. The manuscript now reads: “the average value for the entire study period was h_{mix} = 1.5 m”.

Page 5 line 19: is “e.g.” supposed to be NEP?
“e.g.” stood for “for example”, we agree that it was unclear.
The manuscript now reads: “considering for example forest EC calculations”.

Page 5 lines 19-27: I don’t think equation 4 and the paragraph surrounding it adds very much to the MS and is distracting to the methods section. It is also unclear what the ‘gap with terrestrial ecology’ is and how using CO_2 measurements reduces this undefined gap. Was using different methods of ecosystem productivity really creating a separation between terrestrial and aquatic studies?
We believe that the idea of finding a common language between different fields (aquatic and terrestrial productivity studies) is important. For this reason, we decided to keep the equation and the paragraph. However, we now explain in a hopefully clearer way what the gap is and why in our opinion it is important to harmonize the methods. The manuscript now reads: “High-frequency measurements for productivity are common in forest ecology. They are, however, less common in aquatic ecology, where traditional approaches are still widespread despite their limitations (low temporal resolution, unnatural conditions). Having different methodologies and different time resolutions creates a gap between the two fields, and makes comparing the estimates more difficult. Given that the terrestrial and aquatic ecosystems are a continuum through which carbon is cycled, using shared procedures is a step in the direction of connecting and integrating these ecosystems, in order to have more precise carbon budgets and a deeper knowledge of the carbon cycle.”
Ongoing European projects and infrastructures such as ICOS and RINGO for example have tasks related to this harmonization need.

Page 6 lines 1-4: F_a was not possible at 30 min resolution; did you ever compare to a model of gas flux and fill in data gaps that way? i.e. why not use Heiskanen et al. (2014) gas flux model?
We compared the available EC data with the model from Heiskanen et al. (2014), but for the analysed periods we did not find a good agreement between them, with the model underestimating the fluxes. This might be due to our analysis being focussed on the periods with low wind speeds (even though we did take that into account, and also tried using the median k value reported in Heiskanen et al. for low wind conditions). Given the poor agreement between model and data, we decided not to use the model to fill the gaps, but resort to average F_a values.

Page 6 line 28: I’m assuming Q_{10} is set to 2, but the authors should be explicit to make this clear.
We agree. The manuscript now reads: “Q_{10} is a non-dimensional temperature coefficient whose generally accepted value (and the value we used) for freshwater communities is 2; in the literature, values between 1.88 and 2.19 are reported”.

Page 7: The results section is not very well organized and is a combination of results that do seem to fit in the same paragraph. For example, the first paragraph covers results from Figure 2-5 and table 1 and does not flow well together. Break these up into individual paragraphs with topic sentences so that the reader knows what point the authors are trying to make with the paragraph.
We agree. The assessment section is now further divided into four subsections, and some of the subsections are further divided into paragraphs. The Discussion section is also now divided into two subsections.

Page 7 line 6: do not use colloquial language such as “Anyhow, . . .”
Changed to “However, …”.

Page 7 line 12-13: remove “Figure 3 displays the NEP versus PAR.”
Removed.

Page 7 lines 18-19: get rid of “In case of . . .. it was not necessary.” This doesn’t add anything to the fact that there was no photoinhibition.
Removed.

Page 7 line 20-21: why is respiration more negative with hotter years? Be explicit. Because Rh is more temperature dependent than GPP?
Yes. The manuscript now reads: “Year 2014 was particularly hot, so the strongly negative NEP can be due to increased respiration rates, given the strong dependency of R_h on temperature”.

Page 7 line 26: change Figg. to Figure
Removed.

Page 7 line 31-32: Get rid of “An in-depth analysis. . . some comments are possible.” Since you do spend three paragraphs of the results discussing these parameters. Replace with a more constructive topic sentence.
We agree. The manuscript now reads: “We then focused on the inter-annual variability of the values of the model parameters (reported in Table 1).”

Page 8 line 7-10. Move to methods rather than results
Moved.

Page 8 line 13-14: get rid of “In general, we can say that there are statistically significant differences between the years.”
Removed.

Page 9 line11: but see Lovett et al. 2006 where NEP does not equal –NEE.
We specified it, and added the reference.

Page 9 line 21-22: “This is in agreement with. . .” who? The citations in parenthesis? And what is in agreement with them? That lakes are heterotrophic when NEP is negative? Or that many lakes are heterotrophic?
We rephrased and also moved the sentence to make it clearer. The references were supporting the fact that many lakes at higher latitudes are supersaturated with respect to CO_2, as is our lake. The manuscript now reads, where the genral results are first commented: “the net productivity values are almost always negative, meaning that the ecosystem, overall, is heterotrophic and a source of CO_2. In fact, the daytime and nighttime average values of the CO_2 flux were also always positive, albeit

having lower values during the day than during the night. This is not surprising: many lakes, especially at high latitudes, are supersaturated with respect to CO_2 (Cole et al., 1994; Sobek et al., 2003); as a result, the CO_2 flux is from the lake to the atmosphere also during the day, when the aquatic primary producers are photosynthesising and absorbing CO_2.".

Page 10 line 10-16: This is a confusing paragraph. The authors state that 1) more info on algal communities was needed to explain differences in fitted parameters, but 2) this isn't needed because the whole point of this method is to be simple and parsimonious, but 3) this method should be applied in many lakes to make links between parameters and environmental conditions / algal communities. I don't know what point the authors are trying to make with this paragraph.
We rephrased the paragraph, we hope our point is now clearer. The manuscript now reads: "In this study, we could not clearly link the environmental variables to the changes in the Michaelis-Menten model parameters, and more information on the algal communities living in the lake would have been required in order to expand the analysis. However, it is important to stress that the simplicity of this method lies in the fact that to estimate the parameters, which can then be used to calculate the productivity, information on the algal communities is not needed. It is needed only when widening the scope of the productivity studies: when, for example, the parameters themselves and their relationship with the environmental conditions or the specific phytoplankton communities are investigated. Knowledge on the algal communities would also help when extending the productivity calculation to the whole year.".

Page 10 line 28-29: How is there a comparison between the calculated NEP and modeled NEP when you fit the model to the calculated NEP? To make a statement like this, it seems like you should be training the model on a set of the calculated NEP data and then verifying with a separate set of the calculated NEP data. Also, what do you mean calculated NEP vs. modeled NEP was compared for first time? Compared for the first time using the MM method? I know there are many other examples where predictor variables are used to model lake NEP, so the authors will have to be more specific here.
We agree. We added the out-of-sample comparison. There is now a new subsection in the Results section, which reads: "The analysis we performed was based on an in-sample comparison, since our goal was to check whether our method to calculate the NEP was in agreement with the PI models typically used (Michaelis-Menten, Smith (1936) and Jassby and Platt (1976) equations). However, for the Michaelis-Menten model, we also ran an out-of-sample validation for each year, in order to further verify the correspondence between the calculated NEP and the model. For each year, we randomly selected half of the data points and used them for the fit, to calculate the model parameters. Then, for the other half of the sample, we estimated the NEP using the equation and the parameters we had obtained, and compared it to the originally calculated NEP. We both evaluated the correlation coefficient r between the two NEPs (the one calculated from the data, and the one calculated from the model trained on half of the data points, then discarded), and the RMSE of the validations. The results are reported in Table 2, and show that the two NEP values compared well. The correlation coefficient r varies between 0.84 and 0.92 and the RMSE varies between 0.15 and 0.31 $\mu mol(CO_2)\ m^{-2}\ s^{-1}$.".
The "first time" in the comparison referred to the NEP calculated with this method. It has been calculated with this method only in Hari et al. (2008), and in that paper there is no quantitative evaluation of the modelled NEP vs the calculated NEP. We rephrased it to make it clearer.

Page 10 lines 32-33: this is not a clear sentence.
We rephrased and expanded the sentence, we hope it is now clearer. The manuscript now reads: "Overall, we believe that the method proposed in Hari et al. (2008) and further tested and developed here represents an improvement over the traditional approaches (bottle method and ^{14}C technique), given its time resolution and the fact that it is a free-water approach. We also think it is promising

compared to the other more common free-water approach, the O_2 method, since it is direct and the respiratory quotient is not needed".
I don't think figure 1 is necessary.
Removed.

Figure 2-5: each dot represents a day or a 30 min interval? Please specify in legend
Each dot represents a 30-min interval. The legend now states that.

Can tables 1 & 2 be combined? It would just add 5 more columns.
The tables are now combined.