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

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Provenzale and co-authors describe a study in which they use high-frequency CO2 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 CO2 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 CO2 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.

Hanson et al. (2003, Limnology & Oceanography 48: 1112-1119) also estimated GPP, R, and NEP using CO2 measurements for 2-4 days in each of their study lakes, so this isn't the first time CO2 measurements have been used for metabolism studies other than Hari et al. (2008).

Do the authors have evidence that lateral transport of CO2 negligible in their study lake? Are there stream or groundwater inflows to the lake that may transport CO2? Even in lakes that are fairly isolated from the surrounding landscape, lateral transport of CO2 can be significant. (see Vachon et al. 2016 doi: 10.1002/lno.10454), and some autotrophic lakes can have significant CO2 outgassing, indicating a decoupling from NEP and CO2 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.. 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?

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 CO2. So I think it is incorrect to state that negative NEE is the same as NEP on page 4 line30. 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 CO2 (and other inorganic sinks / sources) are negligible, so equation 3 seems correct, but only under this assumption.

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 CO2 in addition to or in place of O2 to estimate metabolism in aquatic systems (e.g. respiratory quotient different than 1, etc. . .).

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?

Page 5 line7: It is unclear if hmix was set at 1.5m for the entire study period (due to stable stratification and setting Fu 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 CO2 into epi when thermocline deepens and epi CO2 into hypo when thermocline shallows?

Page 5 line 19: is "e.g." supposed to be NEP?

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 CO2 measurements reduces this undefined gap. Was using different methods of ecosystem productivity really creating a separation between terrestrial and aquatic studies?

Page 6 lines 1-4: Fa 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?

Page 6 line 28: I'm assuming Q10 is set to 2, but the authors should be explicit to make this clear.

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.

Page 7 line 6: do not use colloquial language such as "Anyhow, . . ."

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

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.

Page 7 line 20-21: why is respiration more negative with hotter years? Be explicit. Because Rh is more temperature dependent than GPP?

Page 7 line 26: change Figg. to Figure

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.

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

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

Page 9 line11: but see Lovett et al. 2006 where NEP does not equal –NEE.

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?

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.

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.

Page 10 lines 32-33: this is not a clear sentence.

I don't think figure 1 is necessary.

Figure 2-5: each dot represents a day or a 30 min interval? Please specify in legend

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

Signed: Jacob Zwart

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