

## ***Interactive comment on*** **“Temperature-dependence of the relationship** **between $p\text{CO}_2$ and dissolved organic carbon in** **lakes” by L. Pinho et al.**

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Interactive comment on “Temperature-dependence of the relationship between  $p\text{CO}_2$  and dissolved organic carbon in lakes” by L. Pinho et al.

Response to Anonymous Referee #1

Referee #1

General comments

The manuscript “Temperature-dependence of the relationship between  $p\text{CO}_2$  and dissolved organic carbon in lakes” by Pinho et al. analyzes  $p\text{CO}_2$ , DOC, and temperature

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data from 166 tropical and subtropical Brazilian lakes, concluding that in these systems, ambient  $p\text{CO}_2$  concentrations are frequently temperature dependent, not DOC dependent as concluded by Sobek et al. (2005). The paper does a good job highlighting the temperate lake bias in freshwater carbon cycling literature, and clearly demonstrates that low latitude lakes with warm annual temperatures may be functioning and processing carbon very differently than what is commonly reported. I think that this is a very important point, and that it should, as the authors propose, receive more attention in the literature. That said, I am not sure that the findings in this paper make a strong enough case to negate the findings of Sobek et al. (2005), as Pinho et al. lack the DOC gradient in the much larger Sobek dataset. The findings do, however, compliment this previous study by highlighting the variability of surface water  $p\text{CO}_2$  concentrations within tropical and subtropical biomes, and their deviation from trends seen across larger latitudinal gradients. This does not lessen the importance of this manuscript, but may require some generalizations to be tempered. Specific comments are below.

Response: First we would like to thank the referee #1 for the goods comments and by highlighting the importance to note that low latitude lakes with warm annual temperatures may be functioning and processing carbon very differently than what is commonly reported.

We recognize the importance of the positive relationship between  $p\text{CO}_2$  and DOC (Sobek et al 2005). The purpose of this manuscript is highlight that some patterns that well describe the processes in temperate lakes are not necessarily equal to tropical ecosystems. It is understandable the extrapolation of global models with dominance of a particular area, when there is a lack of data for some regions, as can be seen in the support material of Raymond et al. (2013). Lakes from Tropical areas has heterogeneous light and temperature regimes and this climatically driven processes have a strongly influence in OC cycling (Hanson et al. 2015). However, since this data will begin to be available in the literature, and in turn exhibit different responses,

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we should adding and using them to revisit previously global patterns and taken into account in a better overall context, even if the number of data is not yet of the same magnitude.

#### Referee #1 Specific comments

Overall, the scientific methods and assumptions are clearly outlined. Field, analytical, and statistical methods are clear, appropriate, and easily reproducible. Among the strengths of this paper are its clarity and brevity. The authors demonstrate that they are familiar with related work on DOC-pCO<sub>2</sub> relationships, and explain well how their results compliment previous findings. Some references may need updating as the most recent reference is from 2013, but this is a minor point.

Response: We will add into the text new references as Raymond et al. 2013, Hanson et al. 2015, Abril et al 2015. Where is cited the importance of latitudinal variation and temperature change to the carbon cycle, the paucity of data on tropical global estimates and the need for direct sampling of the pCO<sub>2</sub> by headspace.

“A second priority for comparative study is latitudinal and altitudinal where lake temperature, ice cover and mixing regime will differ and these climatically driven processes, in turn, should strongly influence OC cycling.” (Hanson et al. 2015).

“Tropical and saline lakes were higher and lower in pCO<sub>2</sub>, respectively (Supplementary Information), although these lakes were not well represented in the data set (1.5% and 0.8%, respectively)” (Raymond et al. 2013).

“We propose that while TA and pH measurements remain useful to describe the aquatic chemistry, data on pCO<sub>2</sub> should in the future rely on direct measurements of p CO<sub>2</sub> . Even if some studies report relatively robust calculation of pCO<sub>2</sub> from pH and DIC measurements (Raymond et al., 1997; Kratz et al., 1997; Aberg and Wallin, 2014), direct pCO<sub>2</sub> values in the field are stable, precise and straightforward and do not depend on the quality of pH measurements, which are often uncertain.” (Abril et al. 2015).

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Referee #1 Primary criticisms of this work are first, that the results may be slightly overstated. While the authors' findings do diverge from the generally accepted positive relationship between pCO<sub>2</sub> and DOC across latitudes and biomes, overlaying these data onto the larger Sobek dataset does not negate the entire relationship, particularly when lower DOC and higher latitude ecosystems are considered.

Response: In his paper, Sobek et al. analyzed 4902 lakes globally distributed. However in the relationship between pCO<sub>2</sub> and DOC they have 4555 lakes, and in this amount, only one is from Tropical area.

We do not intend to deny the relation found by Sobek et al. for global lakes, but emphasize the necessary to include Tropical data to be considered as a global relationship. We present that, when analyzed separately, tropical data didn't show the same relationship that was previously demonstrated.

“In contrast to previous reports (Sobek et al., 2005), we found no relationship, or a weak negative one, between pCO<sub>2</sub> and DOC in Brazil lakes. Contrasting results between low and high latitude lakes show that consistent positive pCO<sub>2</sub>-DOC relationships from data sets strongly dominated by temperate lakes (Houle, 1995; Prairie et al., 2002; Jonsson et al., 2003; Sobek et al., 2005; Roehm et al., 2009; Lapiere and del Giorgio, 2012; Larsen et al., 2012) cannot be extrapolated to tropical lakes.”

Referee #1 Second, I would suggest more attention be given to effects of productivity in these lakes. The authors deemphasize temperature effects on increased productivity, but it is well known that many phytoplankton prefer warmer temperatures (particularly bloom-forming Cyanobacteria, e.g., Paerl and Huisman, 2008).

Response: We agree with the importance of temperature effects increasing the productivity in tropical lakes, especially in case of cyanobacteria (Paerl and Huisman 2008 and Kehoe et al. 2015). However, as we have no data for oxygen or even chlorophyll, we prefer not to address this issue to avoid speculation.

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Referee #1 Finally, the discussion section would benefit from some speculation on how these findings might be important in the context of warming and climate change in higher latitude lake ecosystems. This would give better context for the broad impact of the main findings of the paper.

Response: We avoid further speculations on global impacts because the main point of the article is related to different responses of tropical lakes to a pattern of correlation and not about increasing temperature can influence this correlation.

Referee #1 Further detailed comments are below.

Referee #1 p. 2790, line 15 & p. 2796, lines 20-23. Emphasis is placed on high temperatures enhancing heterotrophy, not productivity. Nutrient availability is mentioned, but a brief mention or discussion of productivity would also be useful. Related to this, the manuscript would benefit from a table of general limnological/ water quality characteristics of the study lakes (e.g., Chl a, TN, TP). These could be summarized by biome for brevity if these data are available.

Response: We dont have data of nutrients and chlorophyll for these lakes.

Referee #1 p. 2790, line 19-20. Further clarification is needed as to how tropical and subtropical lakes reported in Sobek et al. (2005, n=310) are qualitatively different than those presented here (n=166). It may not be appropriate to describe this paper as having a “paucity” of low latitude data, as it contained more low latitude lakes than this study.

Response: In his paper, Sobek et al. analyzed 4902 lakes globally distributed. However in the relationship between pCO<sub>2</sub> and DOC they have 4555 lakes, and in this amount, only one is from Tropical area.

Referee #1 Figure 3: It is unclear how the strong relationship reported was generated from the data and line shown. These data would be better fit by a curvilinear relationship than a linear one, which is an interesting result in itself.

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Response: The original text: “We tested the temperature-dependence of the relationship between pCO<sub>2</sub> and DOC concentration by pooling both data sets and examining the strength of pCO<sub>2</sub> vs. DOC relationships for lakes grouped within temperature and latitude bins.”

The text now reads: “We tested the temperature-dependence of the relationship between pCO<sub>2</sub> and DOC concentration by pooling both data sets and examining the strength of pCO<sub>2</sub> vs. DOC relationship for lakes grouped within temperature bins. For each bin was performed one linear regression of pCO<sub>2</sub> and DOC and each result was classified as significant (full square) or non-significant (open square). After was developed a linear regression for all bins together and tested the significance of this relationship.”

Referee #1 Figure 4: This plot is slightly misleading. At a glance, it appears that the authors have reanalyzed the full Sobek dataset including data from this study, resulting in an overall non-significant trend. Upon closer inspection, it seems that the non-significant trend line is only fit to data from this study. If the authors can acquire access to the Sobek dataset and reanalyze it with their own contributions, it would make a much stronger case (but this understandably may not be realistic).

Response: The dashed line represented only the equation relative to tropical data.

Referee #1 Figures 5 and 6: Not necessary and can be removed.

Response: We removed the figure 6

Referee #1 Technical comments

Referee #1 Overall the manuscript is very clear, concise, and well written. A few grammatical errors described below need attention.

Referee #1 p. 2793, line 15: Multiple grammatical errors (tense, sentence construction, word spacing); generally unclear. p.2794, line 16: Clarify what is meant by “Most pCO<sub>2</sub> lakes. . .”;

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Response: We will check all sentences erros mencionated in the original manuscript.

Referee #1 line 20: incorrect tense. p. 2796, line 13: Grammar/ sentence construction needs attention.

Response: We will check all sentences erros mencionated in the original manuscript.

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