

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

### **Anonymous Referee #1**

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#### 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 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 at-

C944

tention 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.

#### 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- $p\text{CO}_2$  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.

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  $p\text{CO}_2$  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. 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).

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 find-

C945

ings of the paper. Further detailed comments are below.

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.

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.

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.

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).

Figures 5 and 6: Not necessary and can be removed.

Technical comments

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

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. . .";

C946

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

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