This paper reports the relationship of pCO_2 and DOC in Brazilian tropical lakes, indicating no or weak relationship was observed. These findings are intriguing and they are different from those derived from lower temperature temperate/boreal lakes. The authors' work adds new data to our collective body of knowledge and is important.

General comments:

1. As a whole, "introduction" is clearly addressed. The authors summarize the previous studies of the relationship of pCO_2 and DOC, indicating a lack of data in tropical lakes. Then the authors introduce the importance of temperature. However, logic transition is not smooth in some paragraphs and somewhat confusing. The specific comments are listed below:
    a. Line 52, what do "low latitudes" mean? I think that the authors want to compare the differences between high latitudes and low latitudes. It might not be appropriate to use ONLY "temperate systems" and "WINTER" to represent high latitudes conditions.
    b. Line 59, "heterotrophic activity…support high fluxes of CO_2, leading to CO_2 enrichment". Here "fluxes" should be "production". In contrast, CO_2 outgassing (high flux) from lakes to the atmosphere leads to a decrease in CO_2 content in lake waters.
    c. Line 62, the authors indicated that there was a paucity of low latitude data in Sobek et al. (2005). It is not correct. Sobek et al. (2005) included 148 tropical lakes. If the authors want to divide tropical lakes into high-temp and low-temp tropical regimes and indicate Sobek et al. (2005) doesn't have high temp data, they should address it clearly here.
    d. The last paragraph in Introduction: The authors should address that pCO_2 and DOC are related but pCO_2 is independent of temperature at high latitudes FIRST. Otherwise, it's confusing why temperature is related to pCO_2 and DOC. But here the relationship of pCO_2 and DOC might be influenced "OR NOT" by temperature in tropical lakes.

2. There is no detailed description of methods. Detailed methods need to be addressed for a comparison of different datasets, including method precision/accuracy, progress of calculation, and unbiased data processing between different datasets. ONLY Based on these work, the authors can yield confident results. Some specific comments listed below:
    a. Data source of published data? In line 122.
    b. Line 126, pH is measured on which pH scale and is calibrated against what standards? If the standard has a precision of 0.1, how could we get pH results with a precision of 0.01 pH unit?
    c. Line 136-137, pCO_2 calculated from pH and TA should use dissociation constants of carbonic acid. Weiss (1974) provides the equation of CO_2 solubility. The authors should address which set of constants is used, and if these constants are valid for low salinity lake waters?
    d. Line 138, the authors might cite correction methods as G. W. Kling, G. W. Kipphut, M. C. Miller, Hydrobiologia, 240, 23 (1992).
    e. Line 139-141. "aware of the difficulties in determining the pCO_2…". It should be noted that there has no difficulty in determining (such as direct measurement) of pCO_2. The problem in pCO_2 calculation is raised from the existence of Organic-TA. As well, there is no problem in TA and pH methods. In addition, sentence construction in the following sentences in the paragraph should be revised to make it clear. Avoid using "TA" and "Alkalinity" interchangeably.

f. The authors should address clearly how they remove the influence of TA in method section, or at least describe it concisely here and put details in the supplementary.

3. In the results part, the authors never address why they compare specific regions for temperature and DOC beyond a general description. It gives me a sense that the authors will compare their differences and the magnitude of temperature and DOC should be related to somewhat.
4. There is still no solid and strong evidence to support the primary conclusion of non-significant or weak negative relationship of pCO_2 and DOC in Fig. 3a and 4a. What I see is a strong relationship for pCO_2 and DOC at DOC<10 region with high pCO_2. In contrast, there is no relationship for low pCO_2 region. The authors should separate these regions to see if there is spatial difference which can be explained by different biomes.
5. There is no evidence of temperature related issue for the relationship of pCO_2 and DOC in tropical lakes to support authors' point. The authors do not need to remove fig. 3b in the previous manuscript. Using Ln(pCO_2) vs. temperature can explain the physical control of pCO_2 over temperature (see Takahashi et al. (1993) published in GBC). The authors also can check the relationship between DOC and temperature in tropical/temperature lakes. If there is no relationship between DOC and temperature, latitudinal difference is more convincible than temperature difference.
6. Suitable interpretation of the mechanisms controlling the weak relationship of pCO_2 and DOC and its broad impact are recommended in discussion.

Minor comments: Gramma/sentence construction/spelling need attention.

1. Line 48, "because of" should be "because".
2. Line 79, "conducted a survey of pCO_2…" should be "of pH, TA, and DOC". Here pCO_2 is calculated from pH and TA. Same as in line 87.
3. Line 80, "0 to 33 °" should add "south", same as in Line 83.
4. Line 86, link ftp://geoftp.ibge.gov.br/mapas_tematicos/mapas_murais/biomas is invalid, should be "biomas.pdf".
5. Line 133, GF/F filter is 0.7 um, not mm.
6. Line 175, "(pCO_2 = 45,70 ± 1,84 x DOC + 623,7 ± 18,83, R^2= 0,12, p <0,0001, n = 4433)," I have no idea if it's "pCO_2 = (45,70 ± 1,84) x DOC + (623,7 ± 18,83)???
7. The authors wrongly replace decimal point with comma in a lot of places. Such as line 175…
8. Fig. 1, font of coordinates is too small. Link is invalid.
9. pCO_2 cannot be written as pCO2.
10. Figure captions in support material are unclear.