Response to Reviewers of: *Contrasting late-glacial paleoceanographic evolution between the upper and lower continental slope of the western South Atlantic*.

We are grateful to the reviewers for their interest, attention to detail, and constructive comments that significantly improved the manuscript. Below, we respond to each of the reviewers' comments. We have copied the reviewers' comments in BLACK text and added our responses in BLUE text.

The revised manuscript is provided in the Word file labeled "Luz et al 2020_CPD_tracked changes" selected to examine our changes.

Leticia G. Luz (on behalf of the co-authors)

Reviewer #1

General comments

This paper presents new records of organic and inorganic proxies, which are used to reconstruct changes in sea surface temperature and salinity off the Brazilian Margin over the last 50 kyr. The paper is well written and presents good interpretations. However, I think the authors need to clearly state their scientific questions and revise their approach regarding the salinity reconstruction (see my comments below). I am also not totally convinced that the authors can completely discard the influence of coastal upwelling in their records. Therefore, my recommendation is for a minor revision before publication in Climate of the Past.

We are very grateful to Reviewer #1 for her/his detailed and constructive evaluation of our work. We think her/his suggestions helped to increase the manuscript quality. Below, we show how we addressed each of the points raised by Reviewer #1. At this point in the Response letter we wish to emphasize that we added in a figure with the *Globigerinoides ruber* $\delta^{13}C$ results measured in cores RJ-1501 and RJ-1502. We think that this data is an additional support to discard an eventual strong upwelling of South Atlantic Central Water (SACW) at the RJ-1501 site during the Last Glacial Maximum (LGM) and last deglaciation. A discussion around these data was included in the correspondent part of the text and the potential limitation of planktic foraminifera $\delta^{13}C$ was accounted for.

Specific comments

Abstract - Not clear by the first sentences what is the goal of the study. What is the scientific question? - All the proxies look traditional to me...what is the new proxy?

We removed the proxy classifications (traditional or new) as they have all been used for over 10 years even though some of them (cf. δD-Alkenones) are being considered for the first time in the study area. Additionally, we rephrased the beginning of the Abstract in order to better present the background and the main goal of our study, which is to improve the knowledge about the paleoceanographic evolution of shelf waters in the subtropical western South Atlantic.

Introduction

Line 36: You start talking about millennial-scale changes, but finish the sentence with productivity changes citing papers that are not discussing millennial scale mechanisms. This is a little bit confusing. Please consider revising the sentence.

The purpose of this first paragraph is to provide to the reader a general chronological overview of the studies along the Brazilian margin and their different approaches and conclusions depending on the sediment core region. That is the reason why we cited papers discussing millennial-scale mechanisms (common along NE Brazil) and finish with papers that do not discuss millennial-scale variability at all (those usually south of 20 ºS). We have rephrased the paragraph in order to make this clearer.

Line 41: "application of cores"? Please consider revising this part.

This sentence has been rephrased.

Line 65: "Hence, BCC dynamics are a determining climate factor along the SE Brazilian coast". This is very vague; please explain the BCC dynamics and the influence of BCC on local climate.

Part of the influence on local climate was mentioned in the previous statement where we present that "The gradient imposed by the front may disturb atmospheric properties such as surface wind stress, stability, and air-sea flux exchange because sea surface in this region can act as a heat source to the atmosphere". In order to present more arguments, we have considered the findings of the regional simulation by Reboita et al. (2010) showing that the SST gradient and the consequent air-sea exchange may influence the annual cycle of precipitation along the southern Brazilian continental shelf. In addition, other temperature gradients from other regions where the sea/atmosphere heat flux behave similarly to our study site were included in the beginning of the last paragraph from the Discussion section.

Reading the entire introduction it remains unclear what is the main scientific question of the manuscript. The authors should make their goals very clear in the introduction.

We agree with Reviewer #1 and added a statement at the end of the Introduction to emphasize the main goal of the study.

Methods

Line 87: "Due to the chronological limitation of 14C dating, only the first 250 cm of the RJ-1502 core were considered in this study", but the core only has 250 cm as described in previous sentence. Did you analyze the entire core or not?

The paragraph was rephrased to make it clear that the first 250 cm of the core RJ-1502 was used.

Changes in Fig 1: (i) add the main surface currents; (ii) include a figure with the water mass structure; (iii) consider expanding the map to include the La Plata River mouth and the other cores from this region that are mentioned in the discussion.

As required by Reviewer #1, we modified Figure 1 by adding the main surface currents, including additional panels to show the water mass structure and expanding the map to include La Plata River mouth. The locations of other cores mentioned in the manuscript were also added, except core TNO57-21, to preserve a map scale showing details of important hydrographic features.

Fig 2: replace accumulation rates by sedimentation rates.

The term "accumulation rate" was replaced by "sedimentation rates" in Figure 2 and in the text, accordingly.

Line 161: The authors decided to use the equation of Müller et al. (1998). Why? What are the main arguments to use this particular equation and not the other available equations?

Several calibration studies using core-top sediments proposed models to improve SST calculation from U37K´ (Conte et al., 2006; Müller et al., 1998; Tierney and Tingley, 2018 among other studies). Although these studies offer a global scope, some have a spotlight on a specific region. In 1998, Müller and co-authors analyzed alkenones in 149 surface sediments from the tropical to subpolar eastern South Atlantic to establish a sediment-based calibration of the U37K´ paleotemperature index. And so, the use of the equation of Müller et al. (1998) must take priority over all other equations in South Atlantic samples. Additionally, Ceccopieri et al., (2018) tested the equation of Müller et al (1998) to calculate the U37K´-derived SST values in the Campos Basin, in an area nearby to our cores, using both the annual mean core top calibration and based on the seasonal (austral) calibration curves. In this paper, the authors calculated the U37K´-derived SST data based on the calibrations of Müller et al (1998) and compared the results obtained with the World Ocean Atlas 2013 of SST (WOA13, Locarnini et al., 2013), concluding that the U37K´-derived SST agrees with the annual mean SST. Therefore, our decision to use the equation of Müller et al. (1998) is based on this previous published data. At the end of the last paragraph of item 2.4, we inserted a sentence to add contextual information.

Please remove the first sentence of the topic 2.5. In which lab did you perform the d18O analysis? In which lab did you perform the delta-D analysis?

We removed the first sentence of the topic 2.5 to improve the text of the paragraph. The delta-D analysis was performed at the Geological Institute (ETH-Zurich) and we added this information to the text.

Line 194-196: The sentence is confusing. Please rewrite this sentence and better explain how you corrected the ice volume effect.

This sentence has been rephrased.

The authors use the SST derived from the alkenones in the paleotemperature equation of Mulitza et al. (2003). This is clearly not ideal and can generate errors. The authors should show arguments to support this approach. In my opinion, the ecology of these organisms from which the proxies are derived is very different (seasonality, habitat depth. . .), and this is the reason why it is probably not right to use this approach.

In this new version of our manuscript, we have cited works in the Material and Methods section 2.6 that proceeded with the same assumption, that is, reconstruct the $\delta^{18}O_{IVF\text{-}SW}$ by combining planktic

foraminifera $\delta^{18}O$ and U37K′-derived SST (e.g., Rostek et al., 1993; Emeis et al., 2000; Carter et al., 2008; Sepulcre et al., 2011). Usually, some kind of seasonal correction was performed only on those studies that investigate areas with an extreme seasonal cycle in hydrographic parameters, as the Mediterranean Sea (Essallami et al., 2007). Ecological works suggest that *Globigerinoides ruber* and *Emiliania huxleyi* could be mostly found dwelling the same depth in the subtropical western South Atlantic (Venancio et al., 2017; Ceccopieri et al., 2018). We do not consider that the seasonal cycle in our area is drastic enough to shift our reconstruction to unlikely values. Furthermore, the $\delta^{18}O_{IVF\text{-}SW}$ reconstructed by our approach in core RJ-1502 and that of Santos et al. (2017) in core GL-1090 based only on planktic foraminifera show a reasonably good agreement in terms of absolute values and general trend throughout the last 50 ka (Figure 4C). This evidence suggests that our approach, although it has limitations associated with proxies uncertainty, is robust to reconstruct past variability of $\delta^{18}O_{IVF\text{-}SW}$.

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