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 #2

Here we try to subdivide the comment reviewer #2 in order to facilitate understanding of the response structure.

General comments

It has been a pleasure reading your manuscript. I think it is a very interesting comparison between two datasets form two closely located cores. I think we can learn a lot form these kinds of studies including the one presented here. That being said, I have difficulties following the text. I have the feeling there is a lot of duplication in the description of the currents, for one and I think it would be really good if you would check the writing thoroughly again.

We have tried to build the text to bring the arguments of our interpretation gradually to our reader. That is why in the discussion we (1) present the general findings of low-latitude climate during the last glacial for our studied region; (2) place the new records presented here in this context, highlighting the similarities and discrepancies among them; (3) exclude alternative explanations for the main divergence found in core RJ-1501 and; (4) present what we think is the most likely explanation for core RJ-1501 data in the light of Southern Hemisphere mid- to high-latitudes. We think that it is a very linear and logical sequence to follow. Regarding the regional settings (section 2.2), we could not find precisely (maybe citing line by line) where Reviewer #2 found the duplication in the description of the currents. The main water masses and currents influencing our region are always described by a unique designation, following some critical studies carried out in the region. Notwithstanding, in this new version, we present a broader map where the thermal and salinity gradients along the N-S transect are better visualized. A section map of the water masses also contributes to the oceanographic understanding. The position of cores discussed is now indicated.

Specific comments

On top of that there are some weird things I would like to mention, I doubt if anyone co-injected water with a known isotopic composition into a GC setup for alkenone analysis. I am guessing that the nC27 n-alkane was not for quantification, you already describe quantification in the Uk section, but actually was used as isotope standard to be co-injected with your samples. The nC27 from Arndt (not Arna) Schimmelmann has a pre-determined isotopic composition. Hydrogen isotopes are expressed in ‰ relative to VSMOW (0‰. This complete mash-up of this methods section makes me wonder about the knowledge of the authors and the quality of measurements and/or the involvement or interest of the person that did the actual measurements?

We re-check the procedure and replaced the first paragraph of section 2.6 to clarify and add more detail to the methodology of the alkenones hydrogen isotopic composition, including where the samples were performed and the correcting the spelling of the n-C_{27} standard's laboratory. We agree with Reviewer #2 and the δ^2H quantification step for smaller and larger analytes amounts is more fully described in the text. In addition, we have also clarified that the samples were performed by the Timothy Eglinton team (ETH-Zurich) using the methodology of the hydrogen isotopic ratios of individual organic compounds applied to the previous studies (e.g., Makou et al., 2007; Häggi et al., 2019).

My slightly negative feelings are further strengthened by the ice volume free oxygen isotope record. According to the manuscript this was obtained by correcting for the Uk temperatures. So it is a temperature corrected $\delta^{18}O$ record, not and ice volume free $\delta^{18}O$ record? $\delta^{18}O$ of forams and I will ignore diagenetic overprinting, is determined by (calcification) temperature and the $\delta^{18}O$ of seawater. The latter is correlated with salinity and affected by ice volume especially in these glacial/interglacial records. To get to salinity the forma record has to be corrected for temperature and ice volume by subtracting a benthic foram record, for instance. If you did what you said, the IVF record does not only reflect changes in salinity? Be careful there. Your actual measured $\delta^{18}O$ records are not so different from each other, except maybe for the bump in the coastal record during the deglaciation. The temperature records are different and that basically determines the difference between the temperature corrected $\delta^{18}O$ records. Again, be careful with what you are looking at. In this case the temperature comes from different organisms than the $\delta^{18}O$, which will result in additional uncertainties. The mismatch between the δ^2H of the alkenones and the $\delta^{18}O$ of the forams suggests that these organisms reflect different growth conditions, water masses and/or seasons which does not make it any easier. A Mg/Ca based temperature correction might be better. Of course, other people have also used Uk temperatures to correct $\delta^{18}O$ to get at water isotopic composition and with that salinity. So it is not necessarily wrong, just be careful and discuss this potential problem. Especially since your whole story is based on the temperature corrected $\delta^{18}O$ records and not the actual measured data.

We agree with Reviewer #2 that the full details regarding the ice-volume free seawater $\delta^{18}O$ ($\delta^{18}O_{IVF-SW}$) was not properly described in the original submission. We accounted for that in this new version by adding more explanations of how the records were produced. It is important to emphasize that the $\delta^{18}O_{IVF-SW}$ is not a temperature-corrected record but indeed a $\delta^{18}O$ corrected record. The sea-level/ice-volume correction, in this case, is assumed from Grant et al. (2012), and the meters of sea-level change is translated to their equivalents in seawater $\delta^{18}O$ considering a glacial $\delta^{18}O$-enrichment of 0.008 ‰ per meter sea-level decay (Schrag et al., 2002). The fact that the *G. ruber* $\delta^{18}O$ is not so different from each other, except maybe for the bump in the coastal record during the deglaciation, is the central pillar of our argumentation. The offset (bump) noted by Reviewer #2 from the LGM to the last deglaciation was likely caused by the intrusion of fresh coastal waters flowing from the southern shelf. In the new section 2.5, we deal with the eventual bias that could be generated by applying two different organisms to reconstruct $\delta^{18}O_{IVF-SW}$. We cited other references that have done the same on the grounds that the depth habitat of *Emiliania huxleyi*, the dominant alkenone producer, and *G. ruber* is comparable (e.g., Rostek et al., 1993; Emeis et al., 2000; Carter et al., 2008; Sepulcre et al., 2011), which is also the case of the subtropical western South Atlantic (Venancio et al., 2017; Ceccopieri et al., 2018). Seasonal corrections over the U37K′-derived SST before application in $\delta^{18}O_{IVF-SW}$ has been used only in regions of extreme seasonal variations in temperature and salinity, as the Mediterranean Sea (e.g., Essallami et al., 2007), which is not the case of the subtropical western South Atlantic. $\delta^{18}O_{IVF-SW}$ and δD are not conflicting since both are showing that RJ-1501 suffered the influence of a fresher surface water. It is worthy to note the comparison of $\delta^{18}O_{IVF-SW}$ between RJ-1502 (the most offshore record of our study) and that of GL-1090 (Santos et al., 2017) presented in Figure 4C. The foraminifera-only $\delta^{18}O_{IVF-SW}$ of GL-1090 and the alkenone-foraminifera $\delta^{18}O_{IVF-SW}$ of RJ-1502 are rather similar in terms of general trend and values. If some kind of strong bias because of ecology preferences was taking place the signals would be separated by large offsets, which is not the case. Figure 4C shows that, at the end, the hydrographic features in which the organisms are exposed is likely more important than their biological singularities. Indeed, for standardization proposes a foraminifera-only $\delta^{18}O_{IVF-SW}$ would be the best scenario, but unfortunately, producing a *G. ruber* Mg/Ca at this point is a suggestion impossible to overcome. The samples presented here were analyzed at ETH (Switzerland) and there is no financial and logistical support for this to be repeated (as a result of the troubled moment that Brazilian science lives added to the impacts of Covid-19). Furthermore, the analytical routine for Mg/Ca is not yet implemented in Brazil.

The last thing that makes me wonder a little what is going on with this manuscript is the Δδ SST from figure 6, big delta as difference fine, little delta is for isotopes not Uk based SSTs. Very strange. All in all, I think that this is an interesting study, but I think the data needs a bit more work and I am not entirely sure the authors know exactly what they are doing or some of them have not seen the actual submitted version. As is it can not be published.

We have used the notation as "Δδ" because we are doing a double subtraction of the SST. The first "δ" would come from the subtraction of the mean around zero (anomaly) of each record by itself. The second "Δ" would come from the subtraction of the mean around zero between the records placed on a common timescale. We agree that this may cause confusion and, in this new version, we adopted only the single "Δ" notation. Once more, it would be useful if Reviewer #2 could indicate by line, paragraph or section where he/she thinks the data needs a bit more work (as was the case of Reviewer #1). We respectfully would like to emphasize that all coauthors have the opportunity to see the manuscript before the submission and any statement opposed to that is just speculation.

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