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Discussions

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Interactive comment on "Contrasting late-glacial paleoceanographic evolution between the upper and lower continental slope of the western South Atlantic" *by* Leticia G. Luz et al.

Anonymous Referee #1

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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.

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Discussion paper

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?

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

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? - 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. - Fig 2: replace accumulation rates by sedimentation rates. - 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? - 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? - Line 194-196: The sentence is confusing. Please rewrite this sentence and better explain how you corrected the ice volume effect. - 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.

Results - Line 199: Please write "Marine Isotope Stage" before using just the abbreviation "MIS" - Line 203: "G. ruber (white and pink)". But in the methods you said you just measured in G. ruber (white). Did you measure in both types?

Discussion - Line 231-233: The sentence is a little confusing. Please consider rewriting. When you say that the organisms present different ecologies, it gives support to my previous comment regarding the approach used for the d18Osw estimations. - Line 247: Temperatures below 20°C in the surface layer in this region can already indicate an influence of the SACW. - Line 248: The core used in Lessa et al. 2017 (GL-1090) is offshore. Core RJ-1501 is much more close to the coast and can be influenced by coastal upwelling. I think that the authors need to provide more arguments and show more data in order to complete discard the influence of coastal upwelling. - Line 274: Campos et al. 2019 have recently questioned this mechanism of intensification of the SAMS during Heinrich stadials. XRF data for cores south of 19°S show no increase in Ti/Ca or Fe/Ca (proxies for terrigenous input) during HS. The authors should include these recent findings in their discussion. - Line 293: It is not sufficient to just say "corresponding to almost 1.0 salinity unit (considering...". The authors need to show the SSS equation and the precise estimation of the salinity gradient.

Minor comments (technical corrections):

Line 17: remove "the" before salinity Line 22: signal from mid/high latitudes? The BCC do not come from so far south. Please consider rewriting the sentence. Line 25: "...which may have generated perturbations in the air-sea heat flux, with consequences for the regional SE South America climate". Could you be more specific? Line 32: remove "for" before several Line 36: replace "recovered" by "collected in the..." Line 66: Include "records from" before "sediment cores retrieved..." Line 76: I think it is better "without a minima in SST..." Line 125: Please use the more recent taxonomic nomenclature (Trilobatus sacculifer) Line 128: Write the full name of ETH. Line 141:

Please give the sedimentation rates (cm/yr) instead of the accumulation rates. Line 163: It seems that some words are missing in the end of the name of topic 2.5 Line 179: Please consider changing the name of the topic 2.6. Replace "tracers" by "proxies" and use the symbols of the proxies instead of the full name. Line 261: evidences instead of "evidence"

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