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Climate of the Past Discussions

Interactive comment on "Understanding the mechanisms behind high glacial productivity in the southern Brazilian margin" *by* Rodrigo da C. Portilho-Ramos et al.

Anonymous Referee #1

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The manuscript represents an important contribution for the paleoceanographic and paleoclimatic reconstruction of the Southwest South Atlantic, addressing relevant scientific questions within the scope of CP. It analyzes the relative abundance of some species of planktonic foraminifera and 100m-temperature reconstructions from the uppermost 350 cm of a sediment core extracted from the southern Brazilian margin. Despite it does not contribute with new concepts or substantial data, it intends to elucidate the mechanisms behind the high glacial productivity recorded in the region, and consequently, interesting conclusions are reached. In general terms the manuscript is well written and it is easy to follow the reasoning proposed by the authors. The title clearly reflects the contents, the abstract is concise and complete,

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Discussion paper

the overall presentation is clear and correctly structured, and the references are correct. The language is fluent and precise; I did only some minor comments in the PDF about it. The scientific approach is correct. However, there are methodology aspects that could be improved. Some of the applied methods lack of robustness: the authors state that the "basic assumption is that temperature of ambient seawater is the primary control of foraminiferal assemblages" (line 139). In a particular region like this, why not considering that the planktonic foraminiferal assemblages could mainly respond to productivity instead of temperature? Did the authors test this option? I suggest them to consider this possibility and evaluate it. Instead, the authors use 100m-temperature reconstructions derived from MAT following Portillo-Ramos et al. (2015). In that contribution, the authors follow the criteria of Telford et al. (2013). Telford et al. emphasize that the highest performance of a transfer function is the one that should be used and, in 2015, Portillo-Ramos et al. obtained the best performance at 10 m, not 100 m depth. As in this contribution the aim of the authors is to reconstruct the subsurface temperature, I strongly recommend (following Telford et al., 2013) to constrain the training set to a regional scale and try to obtain a better performance at 100 m depth. In fact, they could apply the same criteria of Lessa et al. (2017), who added 161 core tops from upwelling areas such as the Iberian Peninsula and NW Africa to the training set. I see this item important to be addressed. Finally, I suggest the authors to use the WOA data previous to 2005 in order to avoid the "global warming" signal. The age model is another point of argue. The first meter (which corresponds to the first 23 ka) was already published by Tessin and Lund (2013) and it contains 5 reversals (if we also consider the one obtained by Portillo-Ramos et al. -2014-). The last 2.5 meters have one AMS point from Portillo-Ramos et al. (2014) and two δ18O points of control performed in this study. The δ18O curve fits correctly with the Stack LS16 and the one obtained for sediment core GL-1090. However, as there are so many reversal points, why not trying a Bayesian model like Santos et al. (2017)? In fact, it would be better for the comparison with core GL-1090. I consider this a major point in the MS and I encourage the authors to check the age model. These issues

can be easily improved. The results are clear, concise, and well-structured, and the number and quality of the figures/tables are also correct. However, I insist on checking the age model because the mismatch between G.bulloides' relative abundance and the dinocyst's relative abundance could be the result of an incorrect age model. There are ∼10 ka of difference between trends. If it is not an age model problem, then the results are correct and support the interpretation correctly. Discussion and Conclusions are properly addressed. I carefully read the referenced publications from the SBM and revise the results obtained for G. bulloides' relative abundances. When compared the different core's results, glacial abundances of G. bulloides in JPC-17 seem to be a bit higher than in the other cores (<10%). Nowadays, this species is considerably abundant along the Malvinas Current (Boltovskoy et al., 1996) and apparently the modern configuration of the Brazil-Malvinas Confluence would have been established ∼9 ka ago in response to changes in the strength of the SW-winds (Voigt et al., 2015). If the authors suggest that during the last glacial period there were "prolonged winter-like conditions of prevalent alongshore SW-winds and frequent cold front passages", I think they should consider the Malvinas Current also as a G. bulloides input.

Please also note the supplement to this comment:
https://www.clim-past-discuss.net/cp-2018-98/cp-2018-98-RC1-supplement.pdf

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