This is a detailed point-by-point response to all comments from the Referees and the Editor. The response include the comments from Referees and Editor in black, our responses in blue, and the changes performed in the manuscript in red (page and line numbers refer to the revised version of the marked-up manuscript).

**Referee #1 - Dr. L. Barbara**

We thank Dr. L. Barbara (Referee #1) for his constructive review of our manuscript.

The manuscript by Chiessi and colleagues seeks to reconstruct oceanic and atmospheric thermal conditions in the southeastern South America region, as evidence for changing climate during Termination 1. In general the manuscript is well presented, well-written, and with comprehensive literature support. The finding that the marine temperature appear to be associated with AMOC strength, and that changes in the continental temperature in this region are synchronous with atmospheric CO2, are interesting. In general, I am very much in favor of this paper which is within the scope of Climate of the Past. Before publication, however, I would like the authors to consider these 2 major points listed below.

1. My overall concern is that the results and discussion sections are brief and could be more informative. Chiessi et al. present a really interesting high-resolution data set of the Termination 1. However the authors discuss only the (multi-)millennial scale variability when the data show interesting multicentennial scale variability. I would like the authors to complete the results and discussion sections in term of centennial scale variability, particularly the opposite centennial trend between SST and MAT during the HS1: For example, high SST occur when minima in MAT occur at 16.5 and 15 cal ka BP. SST drop Vs MAP sharp increase at 15.5 cal ka BP. How the authors can explain this opposite thermal evolution during HS1 between the continent and the BC? In this way, I would like the author to give more details about regional ocean-atmosphere interactions at millennial and centennial time scale.

Uncertainties intrinsic to (i) radiocarbon based age models, and (ii) our sea surface temperature and mean air temperature proxies call for caution while interpreting and correlating multi-centennial-scale variability to other records. Thus, we prefer to limit our interpretation to the main features present in our records that are robustly supported. This is the case, for instance, of the marked negative anomaly in our sea surface temperature record around 15.5 cal ka BP. It has been described in the results but not appropriately addressed in the discussion. We agree that giving more attention to it in the discussion will improve our manuscript, particularly considering new high temporal resolution records like Martrat et al. (2014. Quaternary Science Reviews). Accordingly, we implemented the following sentences to the revised version of our manuscript (section 5.1, page 11, lines 25-30):

Moreover, the marked drop in our SST record reaching minimum values at ca. 15.5 cal ka BP could be related to an intervening warm spell registered within HS1 in the North Atlantic mid-latitudes (Martrat et al., 2014). We hypothesize that not only millennial-scale changes associated to Termination 1 (e.g., HS1) affected the BC, but also centennial-scale fluctuations (e.g., internal structure of HS1) were registered. However, we primarily discuss the millennial-scale events because of age model uncertainties.

We examined a putative anti-phased behavior between our sea surface temperature and mean air temperature records. Since a scatter plot between both records shows no inverse correlation (Fig. 1) we do not have support to discuss this topic in more detail.

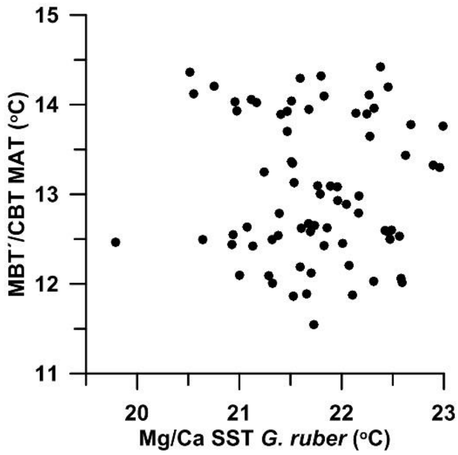


Fig. 1. GeoB6211-2 Methylation Branched Tetraether (MBT') and Cyclisation Branched Tetraether (CBT) based mean air temperature (MAT) vs. GeoB6211-2 *Globigerinoides ruber* (white) Mg/Ca based sea surface temperatures (SST) from 18.8 until 14.1 cal ka BP (the time window for which both datasets show similar temporal resolution). Before plotting, both datasets were linearly interpolated with a step of 65 years starting at 14.1 cal ka BP.

2. Following the previous studies, the slowdown of the AMOC during HS1 has been presented as responsible of the heat retention in the Southern Hemisphere. This is not strictly the case as explained by Mayewski el al., 2009. Those authors describe that changes in the Antarctic ice sheet, sea ice extent, and Antarctic Circumpolar Current (ACC) position can also affect Southern Hemisphere heat retention and ocean circulation. The Brazil-Malvinas Confluence is connected with the ACC. Several studies in Antarctic Peninsula have shown that the winter sea ice edge, cold fresh water discharged, Iceberg runoff would have driven the latitudinal position of the ACC in the South Atlantic Ocean during the Termination 1. In this way, a change in ACC position would have affected the latitudinal position of the Brazil-Malvinas Confluence and affected the SST of the BC. I suggest that the teleconnection between the high latitude and the mid-latitude should have to be taken on board during this period. I would like to ask the authors to consider their interpretations of the SST changes in light of the studies about the ACC evolution during the Termination 1. Maybe the authors could compare their results with the SST reconstruction of Bianchi et al., 2004 (EPSL) or the Icebergs discharge reconstruction of Weber et al., 2014 (Nature).

The paleoclimatic section from Mayewski et al. (2009. Reviews of Geophysics) rather deals with the Holocene. However, in the introductory section "1. Prelude to recent climate", the authors briefly discuss millennial-scale changes of the last glacial/deglaciation. More specifically, the authors suggest, "The cause(s) of these millennial-scale climate events are not fully understood, but slowing of the MOC has been attributed to North Atlantic meltwater flood events and/or to massive iceberg discharges (Heinrich events) that slow the formation of North Atlantic Deep Water. Changes in the Antarctic ice sheet and sea ice extent can also affect Southern Ocean heat retention and ocean circulation [Stocker and Wright, 1991; Knorr and Lohman, 2003]." Although we see no fundamental contradiction to our view of HS1, we agree that encompassing the potential influence that changes in the Southern Ocean may have over our records (see below) will strengthens the manuscript.

The Brazil-Malvinas Confluence is a major barrier for planktonic foraminifera (Boltovskoy et al., 1996. Marine Micropaleontology; 2000. Journal of the Marine Biological Association of the United Kingdom). To the north of the Brazil-Malvinas Confluence, subtropical species like *Globigerinoides ruber* and *Globigerinoides sacculifer* dominate the mixed layer of the warm and salty Brazil Current. To the south of the Brazil-Malvinas Confluence, the uppermost water column is dominated by transitional species like *Globigerina bulloides* and *Turborotalita quinqueloba*. The presence of *G. ruber* throughout Termination 1 is an indicator that the Brazil Current always bathed our core site. Moreover, *Globorotalia inflata* $\delta^{18}O$ shows a ca. 2 ‰ change across the Brazil-Malvinas Confluence (Chiessi et al., 2007. Marine Micropaleontology; Voigt et al., 2015. Paleoceanography). In our core, *G inflata* $\delta^{18}O$ never reach the heavy values typical of the Malvinas Current during Termination 1 (Chiessi et al., 2008. Geology), if changes in global sea level are taken into account. Thus, we exclude a direct influence of the Brazil-Malvinas Confluence over our core site during Termination 1. Still, the Southern Ocean could indirectly affect our core site through the Benguela and South Equatorial Currents, with the signal eventually reaching the Brazil Current. In addition, as discussed below, the Southern Ocean may have influenced mean air temperatures over the La Plata River drainage basin through an atmospheric teleconnection.

Because of the higher temporal resolution, we prefer to compare our results to Weber et al. (2014. Nature) instead of to Bianchi and Gersonde (2004. Earth and Planetary Science Letters).

Regarding mean air temperatures, the multi-model experiment from Weber et al. (2014. Nature) suggests a cooling over southern South America (i.e., to the south of 30$^{o}$S) as a response to an Antarctic meltwater pulse. The cooling amounts to ca. 1.0$^{o}$C over the southernmost portion of the La Plata River drainage basin, whereas the temperature anomaly over the rest of the basin is not shown. As mentioned in section 3.5 of our manuscript, we expect our mean annual temperature record to represent an integrated signal over the La Plata River drainage basin with a predominant contribution from its northwestern domain. The absence of most of the La Plata River drainage basin, including its northwestern domain, on the modelling results from Weber et al. (2014. Nature) hampers a direct evaluation of the impact that an Antarctic meltwater pulse may have had over our mean air temperature record. However, the two most prominent events of increased flux of iceberg-rafted debris at the Scotia Sea (a proxy for Antarctica meltwater pulse) recorded during Termination 1 (Weber et al., 2014. Nature) (i.e., Antarctic Ice Sheet discharge (AID) event 7 between 16.91 and 15.75 cal ka BP, and AID6 between 14.86 and 13.94 cal ka BP) partially correlate with negative anomalies in our mean air temperature record, given age model uncertainties (Fig. 2).

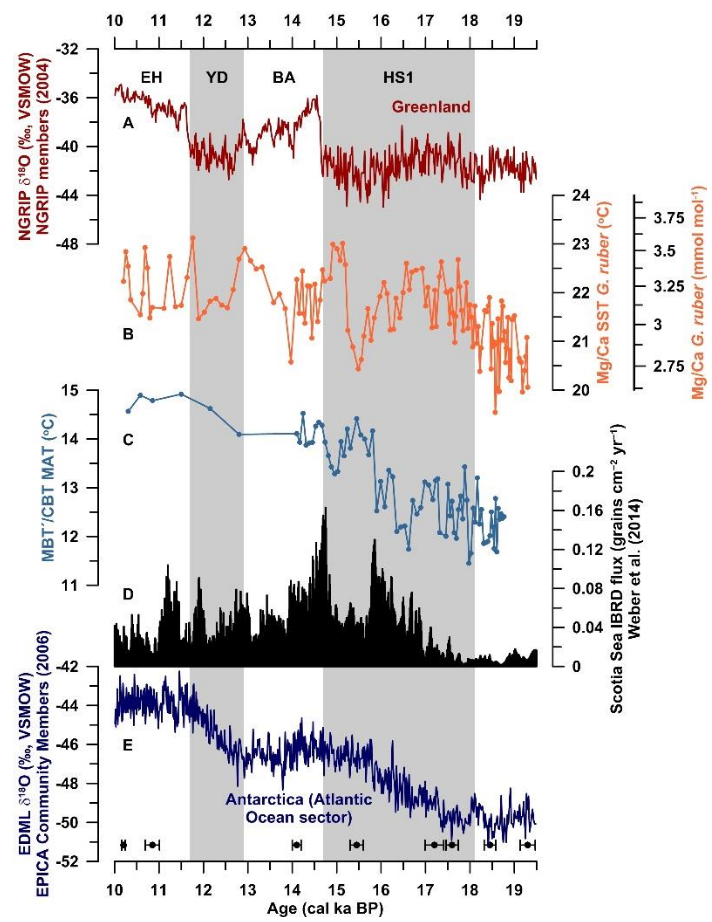


Fig. 2. Millennial-scale variability of the sea surface temperatures of the Brazil Current and mean air temperatures of southeastern South America spanning Termination 1 compared to selected circum-Atlantic records. (a) North Greenland Ice Core Project (NGRIP members, 2004. Nature) $\delta^{18}O$ plotted vs. the Greenland Ice Core Chronology 2005 (GICC05) (Rasmussen et al., 2006. Journal of Geophysical Research). (b) GeoB6211-2 *Globigerinoides ruber* (white) Mg/Ca and Mg/Ca based sea surface temperatures (SST). (c) GeoB6211-2 Methylation Branched Tetraether (MBT') and Cyclisation Branched Tetraether (CBT) based mean air temperature (MAT). (d) Stacked iceberg-rafted debris (IBRD) flux record from the Scotia Sea (Weber et al., 2014. Nature). (e) EPICA Dronning Maud Land (EPICA Community Members, 2006. Nature) $\delta^{18}O$ plotted vs. its original chronology. Black symbols at the bottom of the panel depict calibrated radiocarbon ages used to produce the age model for GeoB6211-2.Grey vertical bars depict Heinrich Stadial 1 (HS1) (Sarnthein et al., 2001) and the Younger Dryas (YD) (Rasmussen et al., 2006). BA: Bølling–Allerød; EH: early Holocene.

Accordingly, we implemented the following sentences to the revised version of our manuscript (section 5.2, page 15, lines 6-12):

Additionally, Antarctic meltwater pulses may have decreased MAT over southeastern South America (Weber et al., 2014). The modeled cooling amounts to ca. 1.0°C over the southernmost portion of the LPRDB. Given age model uncertainties, the two most prominent events of increased flux of iceberg-rafted debris at the Scotia Sea (i.e., AID7 and AID6; Weber et al., 2014) partially correlate with negative anomalies in our MAT record (Chiessi et al., 2015a). Thus, Antarctic meltwater pulses may have contributed to the variability of MAT over the LPRDB in addition to changes in atmospheric $CO_2$ concentration.

Regarding sea surface temperatures, the multi-model experiment from Weber et al. (2014. Nature) also suggests a cooling for the uppermost ca. 1000 m of the water column of the subtropical South Atlantic as a response to an Antarctic meltwater pulse. The cooling amounts to ca. 0.5°C at the uppermost water column, where we expect our sea surface temperature signal to come from. Indeed, our sea surface temperature record shows minor (i.e., ca. 0.5°C) decreases around peak iceberg-rafted debris fluxes within AID7 and AID6. Thus, Antarctic meltwater pulses may have contributed to the variability of sea surface temperatures of the subtropical domain of the Brazil Current on top of the mechanisms described in our manuscript. Accordingly, we implemented the following sentences to the revised version of our manuscript (section 5.1, page 14, lines 17-24):

Recently, Weber et al. (2014) suggested that Antarctic meltwater pulses may have cooled the upper water column of the South Atlantic. Indeed, our sea surface temperature record shows minor (i.e., ca. 0.5°C) decreases around two most prominent events of increased flux of iceberg-rafted debris at the Scotia Sea (a proxy for Antarctic meltwater pulses) recorded during Termination 1 (i.e., Antarctic Ice Sheet discharge (AID) event 7 between 16.91 and 15.75 cal ka BP, and event AID6 between 14.86 and 13.94 cal ka BP) (Chiessi et al., 2015a). Thus, Antarctic meltwater pulses may have contributed to the variability of SST from the subtropical domain of the Brazil Current on top of the mechanisms already described.

However, the ocean temperature modelling results shown by Weber et al. (2014) relate to a zonally averaged meridional transect, and has to be compared with caution with our sea surface temperature record that comes from the westernmost portion of the subtropical South Atlantic.

**Referee #2 - Anonymous**

We thank anonymous Referee #2 for his constructive review of our manuscript.

The paper by Chiessi and colleagues present a high-resolution, high-quality record of the termination 1 SST and SAT from the adjacent landmass from a core collected North of the La Plata river mouth. I am very much in favor of publishing this study. I however suggest some minor to moderate revisions prior to publication, as I feel the results and discussion can be improved.

First, I just had a look at Loic Barbara’s comment. Before starting my own review I want to strongly emphasize that I couldn’t agree more with him on the two points, especially on point 1. The authors present a very high quality paleo-record at unprecedented resolution for this area, but instead of commenting the extremely interesting music found in their records they comment on the H1 anomaly, as if it was the only interesting feature in their high-resolution record. Why? Such an analysis MUST be more descriptive, if the authors want their record being a reference record for the region. Instead, they try to make their own record fitting to other ones - sometimes of worse quality – and I sometimes have the sad feeling that the authors try to avoid commenting on their high-quality record (because it is complex?).

This observation is very similar to comment #1 from Dr. L. Barbara (Referee #1). Since we already answered that comment we do not see the need to repeat it here, and we kindly remit Referee #2 to our answer to comment #1 from Dr. L. Barbara (see above).

This being said, I have a few major and minor comments that I list below, hopefully from the most to the least important to consider.

1. Chapter 5.2 should be reconsidered / rewritten. Again, the MAT record acrobatically tries to fit to westerlies, to $CO_2$, Antarctica, etc. The only regional record to which the MAT has been compared here is the Lake Consuelo pollen record. But you can't write the MAT bears "close resemblance" with it! The only thing one can say about the pollen record is that there might have been an overall temperature increase of about 3°C between the LGM and the early Holocene. The MAT record during the H1 is indeed impressive, and the authors just forgot to discuss some interesting connections between the MAT and the seawater d18O! What can been told about the internal complexity of H1? What can be told about land-sea interactions? What can be told about some apparent anti phasing between the SST and MAT records?

Continuing on the MAT, how the authors can be sure that there is no contribution from marine temperature? I am not familiar with GDGT but suspect that some membrane lipids from marine algae are used in the MBT/CBT proxy? As for TEX, it is usual to show the BIT to invoke that there are no marine vs. continental source, but the authors just don't show it. Why? Did I miss an important technical point here? This would be much more convincing than invoking Nd isotopes or the origin of particulate organic matter if the authors could show that none of the molecules of the MBT/CBT proxy are of marine origin by using the same armada of GDGTs or whatever other membrane molecules. If I'm technically wrong about the GDGTs (meaning any of the molecules used in the MBT/CBT are not used either for the TEX), then some easy-to-understand explanation of why it is pointless to show the BIT might be useful to non-specialists of the GDGTs proxies like me.

Finishing on the MAT, the first sentence "Most of the warming in our step-like structured MAT record takes place during the second half of HS1 and during the YD, whereas little or no warming characterizes the LGM, the BA and the early Holocene" should be deeply rethought. The truth is that the resolution is not sufficient to write such a sentence (no data for the LGM, only few points at the very beginning of the B/A, two points in the YD, 4 points during the early Holocene. Again, you really should deal with internal variability during the H1 there. In any case, no data = no variability to comment on.

We agree that the wording used to characterize the similarity of our mean air temperature record and the temperature record from Bush et al. (2004. Science) was not appropriate. Thus, we rewrote part of the second paragraph of section 5.2 (the main paragraph dealing with the comparison of our MAT record to the temperature record from Bush et al., 2004. Science) to (section 5.2, page 15, lines 20-27):

Nevertheless, the comparison of our MAT record to other continental temperature records with relatively lower temporal resolution allows evaluating the spatial variability of MAT. Our MAT record, for instance, bears some resemblance with a pollen-derived temperature record from eastern Peru (Lake Consuelo) collected at 13.95°S (Fig. 6B, C) (Bush et al., 2004). The linkage of MAT in the LPRDB to low latitude temperature evolution is supported by modern observations of a relatively flat MAT profile in tropical to subtropical South America between 10°N and 20°S (Fig. 1B) (Legates and Willmott, 1990).

Also, the first sentence of the third paragraph of section 5.2 that also referred to the comparison of our MAT record to the temperature record from Bush et al. (2004. Science) was changed to (page 15, lines 28-31):

The general agreement between our MAT and eastern Peru temperatures (Bush et al., 2004) taken together with the rise in atmospheric $CO_2$ content (Monnin et al., 2004) (Fig. 6B, C, D) suggests that greenhouse gas concentrations exerted a strong control on South American MAT during Termination 1.

Still, we are not aware of other quantitative mean air temperature records from (sub)tropical South America to the east of the Andes that is continuous for most of Termination 1 and shows the necessary high temporal resolution (Shakun et al., 2012. Nature). We added this information to the revised version of our manuscript as mentioned below, in order to justify the comparison to the record from Bush et al. (2004. Science) (section 5.2, page 15, lines 18-20):

High temporal resolution and continuous MAT records from tropical South America to the east of the Andes spanning most of Termination 1 are, to our knowledge, still absent (Shakun et al., 2012).

We also agree on the apparent connection between our mean air temperature and $\delta^{18}$Oivc-ssw records (i.e., higher mean air temperatures associated to lower $\delta^{18}$Oivc-ssw). However, since this relationship does not hold for the whole investigated period (e.g., from 19 until 18 cal ka BP the mean air temperature record remains stable while the $\delta^{18}$Oivc-ssw record increases) we do not feel confident enough to include this into the revised version of the manuscript.

Regarding the internal complexity and apparent anti-phasing between our sea surface temperature and mean air temperature records, please see our answer to comment #1 from Dr. L. Barbara (Referee #1) above.

The referee is right in pointing out that there might be some complicating processes when applying the mean air temperature proxy. The most widely discussed issue with this proxy in marine sediments is the in-situ production of the branched glycerol dialkyl glycerol tetraethers (brGDGTs) by some uncharacterized microbial community in sediments. There are a few studies describing this effect (e.g., Peterse et al., 2009. Organic Geochemistry; Zhu et al., 2011. Organic Geochemistry). In these studies, the authors consistently find an increase in the relative abundance of those brGDGTs containing cyclopentane moieties (e.g., brGDGT Ic and brGDGT IIc) as well as a decrease in the relative abundance of the compounds brGDGT I and brGDGT II. We examined our data set for indications of marine in-situ production, which was not present (Fig. 3). Accordingly, we added the following sentences to the revised version of our manuscript (section 3.5, page 8, lines 15-20):

The production of GDGTs by some uncharacterized microbial community in marine sediments has gained recent attention (e.g., Zhu et al., 2011). For in situ production in the marine realm, the authors consistently describe an increase in the relative abundance of those GDGTs containing cyclopentane moieties (e.g., GDGT Ic and GDGT IIc) as well as a decrease in the relative abundance of the compounds GDGT Ia and GDGT IIa. We carefully screened our results for a similar behavior.

And (section 4.4, page 10, lines 10-13):

We first examined our data set for indications of marine in-situ production of GDGTs as described in section 3.5, which was not the case (Chiessi et al., 2015b). Then, we calculated continental MAT values that range from 11.5°C at 18.0 cal ka BP to 14.9°C at 11.5 cal ka BP (Fig. 4E).

Furthermore, Referee #2 suggests presenting a branched and isoprenoid tetraether (BIT) index record along with the mean air temperature estimates to illustrate unchanged continental sources. Our BIT record is indeed rather constant over the time interval discussed here. However, as this quantifies the relative contributions of brGDGTs and isoGDGTs derived from aquatic archaea, and the latter are not considered at all in the MBT'/CBT indices, we do not think that much can be gained from the BIT record.

We agree that rewording the sentence "Most of the warming…" taking into consideration the comment from Referee #2 will improve the manuscript. More specifically, we are not able to make a statement about the Last Glacial Maximum, and have to be more careful on the second half of the Bølling-Allerød, Younger Dryas and early Holocene due to the low temporal resolution of our record for that specific period. Accordingly, in the revised version of our manuscript we added one sentence to section 4.4, and changed the first sentence of the first paragraph of section 5.2 as follows (section 4.4, page 10, lines 18-21):

The marked decrease in the mean temporal resolution of our MAT record after 14.1 cal ka BP that shifts from ca. 70 years to ca. 555 years is worthy of note. This has to be considered while interpreting the MAT trends described for the period after 14.1 cal ka BP.

And (section 5.2, page 14, lines 27-31):

Most of the warming in our step-like structured MAT record takes place during the second half of HS1 and during the YD, but due to the marked decrease in temporal resolution of our MAT record after 14.1 cal ka BP we raise a note of caution while interpreting changes in continental temperature during the YD (Fig. 4E) (Sarnthein et al., 2001; Rasmussen et al., 2006).

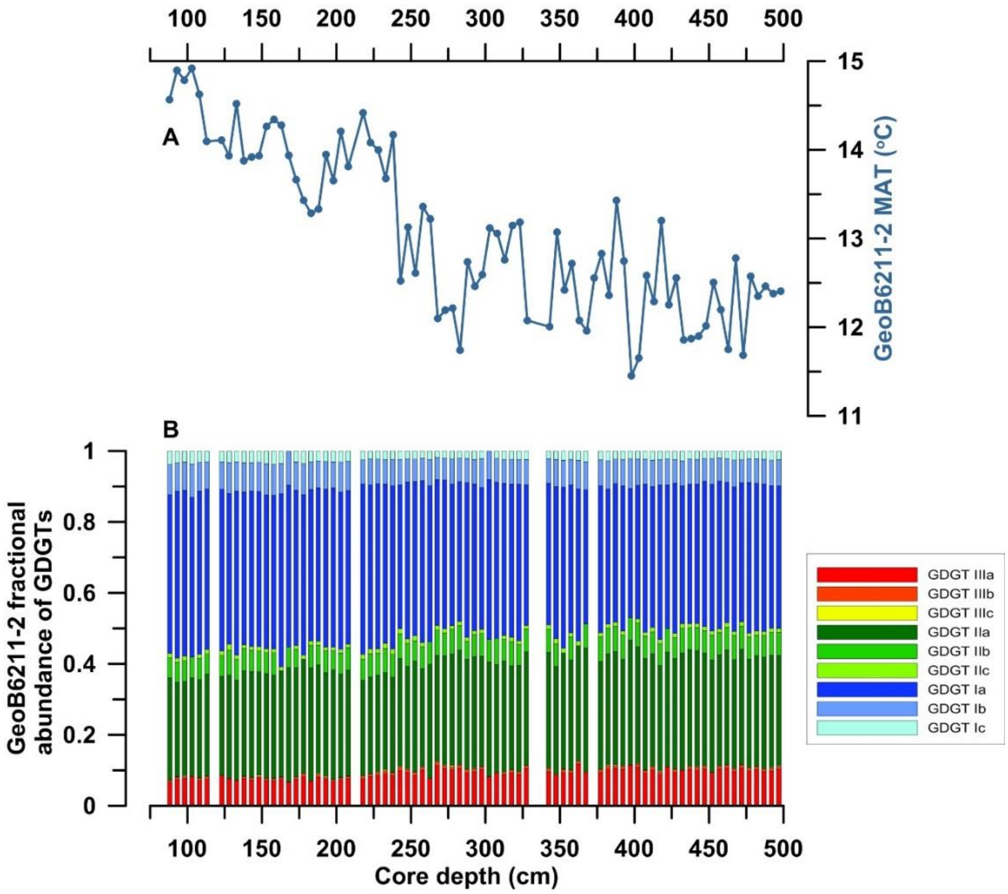


Fig. 3. (a) Mean air temperatures (MAT) based on branched glycerol dialkyl glycerol tetraethers (GDGTs), and (b) fractional abundance of the branched glycerol dialkyl glycerol tetraethers (GDGTs) from core GeoB6211-2. For the estimation of mean air temperatures molecules IIIb and IIIc are not used (Peterse et al., 2012. Geochimica et Cosmochimica Acta). Note that (a) and (b) are plotted against core depth.

2. The data "shows very similar patterns" with Weldeab. This is true if, again, you just deal with the LGM/H1/B-A broad shifts. But the resolution of each core contains much more than that, and interesting differences should be commented. When Weldeab starts warming, your data

already reached its SST maximum. At the end of th H1 you barely comment, in the result chapter, the very most prominent shift in SST at around 15.5 ka which is not seen in Weldeab, etc. Without going too far in the details you should spot those prominent features, so that people interested in the curve zigzags such as the famous "W" recorded in some tropical rainfall records can be more interested in your data. So the "in phase" behavior is, in the end, very sketchy given the golden piece of dataset you have in hands.

We agree that giving more attention to the marked negative anomaly in our sea surface temperature record around 15.5 cal ka BP will improve the discussion of our manuscript, particularly considering new high temporal resolution records like Martrat et al. (2014. Quaternary Science Reviews). This negative anomaly has been described in the results but not appropriately addressed in the discussion of our manuscript. ). Accordingly, we implemented the following sentences to the revised version of our manuscript (section 5.1, page 11, lines 25-30; already mentioned above):

Moreover, the marked drop in our SST record reaching minimum value at ca. 15.5 cal ka BP could be related to an intervening warm spell registered within HS1 in the North Atlantic mid-latitudes (Martrat et al., 2014). We hypothesize that not only millennial-scale changes associated to Termination 1 (e.g., HS1) affected the BC, but also centennial-scale fluctuations (e.g., internal structure of HS1) were registered. However, we primarily discuss the millennial-scale events because of age model uncertainties.

However, uncertainties intrinsic to (i) radiocarbon based age models, and (ii) our sea surface temperature proxy call for caution while interpreting and correlating multi-centennial-scale variability to other records. We prefer to limit our interpretation to the main features present in our records that are robustly supported. Still, we toned down the statements that our sea surface record and the one from Weldeab et al. (2006. Earth and Planetary Science Letters) are “in-phase” in the second paragraph of section 5.1 of the revised version of our manuscript in the following way (section 5.1, from page 11 line 31 until page 12 line 9):

Interestingly, the other high temporal resolution Mg/Ca based SST record from the western South Atlantic covering Termination 1 shows similar changes in SST during HS1 (Figs. 1A, 5C) (Weldeab et al., 2006). This core (i.e., GeoB3129-1/3911-3) was collected off NE Brazil at 4.61$^{o}$S, thus under the influence of the NBC. The similarity in SST between both western South Atlantic records goes beyond HS1, and is also valid for the SST drop with minimum values at ca. 14 cal ka BP, and peak SST at ca. 13 cal ka BP, during the Bølling-Allerød (BA). Thus, our SST record (from the BC) together with the SST record from Weldeab et al. (2006) (from the NBC) suggest a generally in-phase behavior of the BC and the NBC regions during HS1 and the BA. This is contradicts the BC-NBC anti-phase relationship suggested by Arz et al. (1999), at least concerning SST since we have proxy to assess current strength.

Additionally, we substituted the second sentence of the fifth paragraph of section 5.1 by (section 5.1, page 12, lines 3-6):

However, the thermal response of the surface layer of the western South Atlantic cannot be described as an anti-phase in SST between the BC and the NBC regions (Arz et al., 1999), but rather as a widespread and in-phase increase in SST.

3. What exactly your proxies mean, and what is the implication of that? You rapidly deal with seasonality of G. ruber at your core site, but does it apply also at theWeldeab site? What would happen if instead of Mg/Ca you used alkenones? What would be the final overall interpretation? Of course I don't want to push you measuring alkenones, but you might have opted also for the SST record of Jaeschke (2007, paleoceanography) while attempting to compare you record to a SST record form the NBC branch. The Jaeschke, at almost the same site than Weldeab, shows a more Greenland-like SST record (!), definitely different from that of Weldeab. I feel there is more to dig here in terms of rapid climate changes/seasonality during the deglaciation.

In section 3.3 of our manuscript, we state that our Mg/Ca based sea surface temperature record reflects southern hemisphere summer conditions. As suggested by Steinke et al. (2008. Quaternary Science Reviews) and Leduc et al. (2010. Quaternary Science Reviews) different sea surface temperature proxies may record different seasons. This is one of the reasons that compelled us to compare our Mg/Ca based sea surface temperature record to other Mg/Ca based records like Weldeab et al. (2006. Earth and Planetary Science Letters) and Barker et al. (2009. Nature). Moreover, many water hosing model experiments (e.g., Stouffer et al., 2006. Journal of Climate) place a change in sign of sea surface temperature anomaly in the tropical Atlantic off northeastern South America. To the north of this boundary, the sea surface temperature anomaly is negative under a weak Atlantic meridional overturning circulation, and to the south of it the anomaly is positive. This boundary may have shown seasonal meridional migrations producing different signals in different proxies from nearby cores, as it seems to be the case in Weldeab et al. (2006. Earth and Planetary Science Letters) and Jaeschke et al. (2007. Paleoceanography).

Other minor comments:

-Chapter 2.2, last paragraph, I just don't get what you want to say.

This paragraph was rephrased in the revised version of our manuscript as follows (section 2.2, page 4, lines 14-19):

Air temperatures at low atmospheric levels over South America are dominated by the equator-to-pole thermal gradient (Fig. 1B) (Garreaud et al., 2009). The meridional temperature profile is rather flat between the equator and 20$^{o}$S, amounting to ca. 20$^{o}$C. To the south of 20$^{o}$S, temperatures gradually decrease to 0°C over the southern tip of the continent. Zonal departures from this meridional gradient are relatively small to the east of the Andes, as is the case for the LPRDB.

-As for the H1, the B/A variability in both your and Weldeab's records is quite interesting, why not developing this a little more, as already suggested for the H1? There is the Bolling, the older dryas, the early allerod, the intra-allerod cold reversal, the late allered etc. already documented in the north atlantic and in greenland, I feel you also miss some interesting comments on that time window.

Again, we claim that uncertainties intrinsic to (i) radiocarbon based age models, and (ii) our sea surface temperature proxy call for caution while interpreting and correlating multi-centennial-scale variability to other records. The negative anomaly of our sea surface temperature record centered around 14 cal ka BP and the gradual increase in sea surface temperatures from 14 until 13 cal ka BP that seem to be reliable features have already been described in section 4.2 and discussed in section 5.1 of our manuscript. At this stage, we do not feel confident to interpret additional minor features that characterize our record during the Bølling-Allerød.

-The chapter 5.3 says all and nothing. Please try to hierarchize the information and interpretation you want to convey instead of having a shopping list of all the Science and Nature paper you might want to consider.

According to the comment from Reviewer #2, we agree that the first paragraph of section 5.3 needed substantial changes. Thus, we substituted the first paragraph of section 5.3 by (section 5.3, page 16, lines 16-27):

Our SST record suggests that the South Atlantic and the BC more specifically, was of paramount importance for the southward propagation of the thermal bipolar seesaw signal of HS1 (Fig. 5). Indeed, the western South Atlantic was more sensitive to AMOC forcing than lowland South America, which appears to be more susceptible to atmospheric $CO_2$ changes (Figs. 5 and 6). Thus, our SST and MAT records sum up to other lines of evidence supporting the notion that global continental air temperature closely tracked the increase in atmospheric $CO_2$ concentration during Termination 1, and that variations in the AMOC caused a seesawing of heat between the hemispheres mainly impacting the oceans in the Southern Hemisphere (Shakun et al., 2012).

-The "no reservoir age"… I am OK, but if the authors decide to re-focus a little on the centennial-scale features they may deal with that issue a little more lengthy. Further South of their core, there are some samples along the argentinian coast with reservoir ages of more than 1000 years (one sample has a 2800 years reservoir age!) As Loic Barbara points out, any change in the Antarctic circumpolar current is likely, and might also affect the latitude of the

Malvinas/Brazil confluence and input some old carbon into surface waters, obscuring the timing of the high-resolution climate records.

Because we exclude a direct influence of the Brazil-Malvinas Confluence over our core site (for a thorough rationale, please see the second paragraph of our answer to comment #2 from Dr. L. Barbara (Referee #1) we have no reason to use an additional $\Delta$R.

I sincerely wish very good luck to the authors for the review process and very warmly encourage them to re-submit an article that is not shy to present an awesome reference curve from the region!

**Editor – Dr. J.P. Bernal**

We thank Dr. J.P. Bernal (Editor) for his constructive comments to our manuscript.

Two referees have commented on your recently submitted paper entitled "Thermal evolution of the western South Atlantic and the adjacent continent during Termination 1" in which you are the lead author.

I have read the manuscript as well as the referees comments posted on-line and I agree with their comments and suggestions. Both referees agree that the record presented in the manuscript is of extremely high quality and reveals an interesting story on the multimillennia time-scale and the tele-connections that may be driving them. However, they also agree that the high-resolution sampling of the core permits further interpretations into the centennial-scale processes taking place, particularly during HS-1.

Another common issue found by the reviewers encourages you to include other records from the area, as well as to enhance the discussion regarding the significance of the selected proxies as well as potential lags and leads between such records.

Consequently, I encourage you to answer to the questions/issues raised by the referees and I look forward to read the corrected version of the manuscript.

The comments from Dr. J.P. Bernal (Editor) exclusively refer to topics raised by Dr. L. Barbara (Referee #1) and anonymous Referee #2. We kindly remit Dr. J.P. Bernal to our thorough answers to Dr. L. Barbara and Referee #2 above.

**Additional changes made to improve the readability or comprehensiveness of the text and figures**

Page 1, line 22: Added "so far".

Page 1, lines 22-23: Deleted "and a compelling record of the BC-NBC antiphase behavior remains elusive".

Page 1, line 27: Substituted "existing NBC record" by "existing SST record from the NBC".

Page 2, line 9: Added "Stocker, 1998".

Page 2, line 9 (and further occurrences): Added a space between adjacent citations.

Page 2, line 13: Substituted "collapse" by "reduction".

Page 2, line 19: Substituted "lack of a high temporal resolution record" by "lack of high temporal resolution records".

Page 4, line 11: Substituted "minima" by "minimum".

Page 6, line 16: Deleted "ratios".

Page 6, line 27: Substituted "record austral hemisphere summer conditions in our" by "records austral hemisphere summer conditions at our".

Page 7, line 13 (and further occurrences): Formatted all suffixes to $\delta^{18}O$ (i.e., "ivc-ssw", "sw", "ssw", "*G. ruber*") as subscripts.

Page 9, line 10: Substituted "cal kyr BP" by "cal ka BP".

Page 9, line 21: Deleted "ratios".

Page 10, lines 27-28: Substituted "During the LGM sea-level lowstand shifted" by "The sea-level drop preceding the LGM shifted".

Page 11, line 4: Substituted "in the" by "on the".

Page 11, lines 15-16: Added "(Fig. 5A) (Bard et al., 2000)".

Page 13, line 12: Added "EPICA Community Members, 2006".

Page 13, line 13: Substituted "oceans" by "South Atlantic".

Pages 15 and 16, lines 31 (page 15) – 5 (page 16): Deleted "Considering equilibrium climate sensitivity to changes in atmospheric $CO_2$ concentration to be within the range 1.5 to 4.5°C (Bindoff et al., 2013), the deglacial temperature increase exclusively due to $CO_2$ rise would range from 0.9 to 2.7°C. Thus, the deglacial temperature rise in our MAT record (i.e., 2.5°C) may largely be explained by the deglacial atmospheric $CO_2$ increase. Nevertheless, this attribution hypothesis has to be considered with caution since deglacial equilibrium climate sensitivity may have differed from the modern one (Crucifix, 2006).".

Page 17, line 3: Substituted "duration of the stadials of the last deglaciation was" by "duration of last deglaciation stadials was".

Page 18, lines 17-18: Added ", and L. Barbara (reviewer), one anonymous reviewer and J.P. Bernal (editor) for constructive comments".

Pages 19-27: References were updated.

Page 29, line 13: Substituted “and mean annual low-level (925 hPA) atmospheric” by “and annual mean low-level atmospheric”.

Page 29, line 21: Substituted “number 1 in the central panel indicate the” by “number “1” in the central panel indicates the”.

Page 30, line 10: Deleted “the”.

Figure 4: Formatted “ivc-ssw” (y-axis title of panel D) as a subscript.