

We thank anonymous Referee #2 for his constructive review of our manuscript. To facilitate the discussion, we copied his comments below in black and inserted our responses in blue.

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 and our answer is open access 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.

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<sub>2</sub>, 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 d<sub>18</sub>O! What can be 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 and will be changed accordingly in the revised version of our manuscript. 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 will add this information to the revised version of the manuscript in order to justify the comparison to the record from Bush et al. (2004. Science).

We also agree on the apparent connection between our mean air temperature and  $\delta^{18}\text{O}_{\text{ivc-ssw}}$  records (i.e., higher mean air temperatures associated to lower  $\delta^{18}\text{O}_{\text{ivc-ssw}}$ ). 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  $\delta^{18}\text{O}_{\text{ivc-ssw}}$  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).

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 closely for indications of marine in-situ production, which was not present (Fig. 1). We will include a comment about this on the revised version of our manuscript. 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 and it will be implemented in the

revised version of our 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.

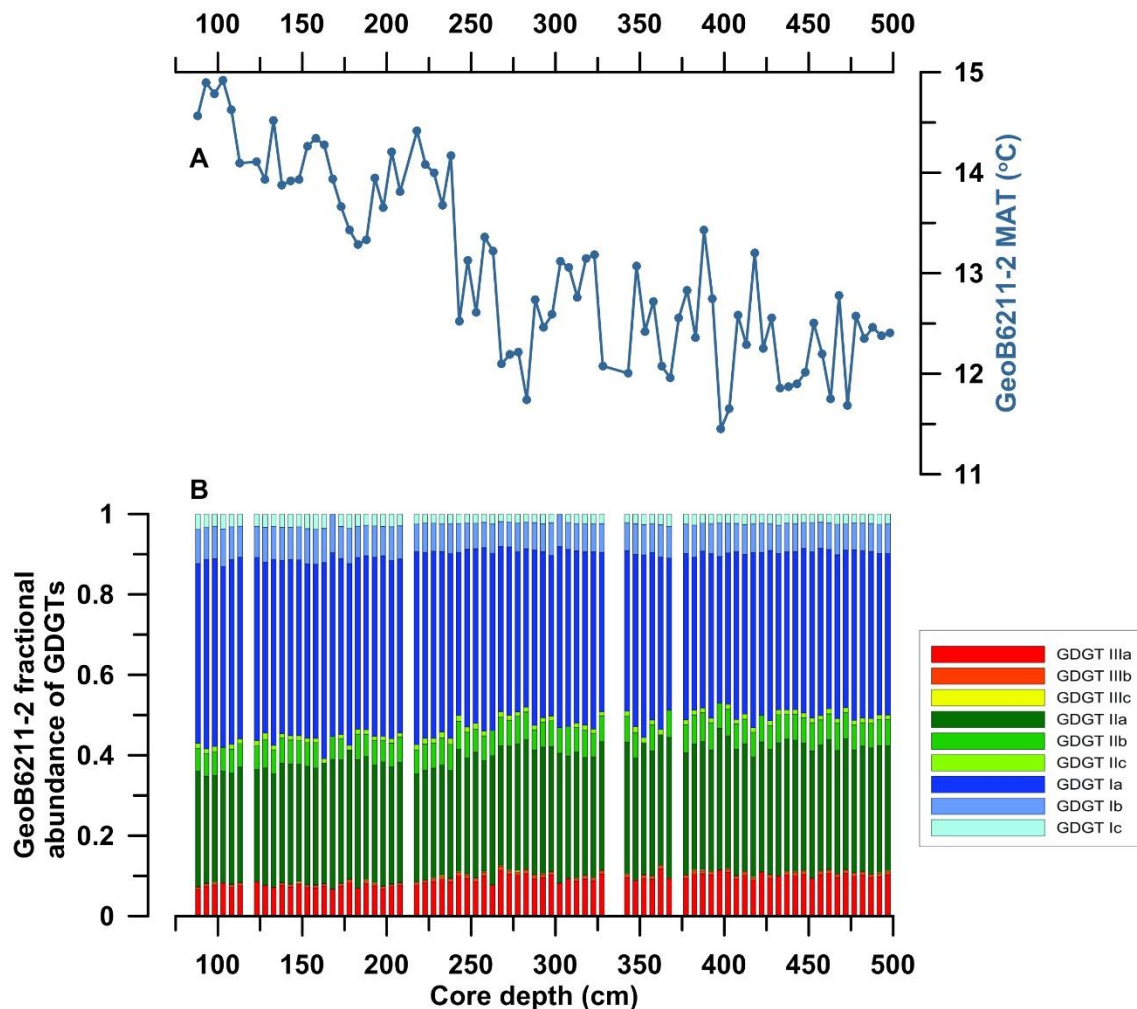


Fig. 1. (a) Mean air temperatures (MAT) based on branched glycerol dialkyl glycerol tetraethers (GDGTs), and (b) fractional abundance of the branched glycerol dialkyl glycerol tetraethers (GDGTs) from core GeoB6211-2. For the estimation of mean air temperatures molecules IIIb and IIIc are not used (Peterse 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. We will include further details about it in the revised version of our manuscript. 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 will tone down the statement that our sea surface record and the one from Weldeab et al. (2006. *Earth and Planetary Science Letters*) are “in-phase” in the revised version of our manuscript.

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 the Weldeab 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 from 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 will be rephrased for more clarity in the revised version of our manuscript.

-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 allerod 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.

We agree that some changes may improve section 5.3 and will do so in the revised version of our manuscript.

-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!