

## ***Interactive comment on “Thermal evolution of the western South Atlantic and the adjacent continent during Termination 1” by C. M. Chiessi et al.***

**Anonymous Referee #2**

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First of all, sorry I'm late for the review... but here it is:

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 fea-

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ture 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 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

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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.

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.

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.

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Other minor comments:

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

-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

-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.

-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.

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!

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Interactive comment on Clim. Past Discuss., 10, 4553, 2014.

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