Author comment (response to reviewers' comments) on: 'Deglacial intermediate water reorganization: new evidence from the Indian Ocean' by S. Romahn, A. Mackensen, J. Groeneveld and J. Pätzold

**We would like to thank both reviewers for their positive and constructive comments. It will greatly help to improve the manuscript. Below, we address each of the comments including explanations.
Technical corrections, such as typos, grammar and figure corrections will be included directly in the revised version of the manuscript. The original comments are given in italics; our responses are highlighted in bold.**

Anonymous Referee #1

1) There is a remarkable shift in sedimentation rate between 18 and 12 ka, from 3.5 to 40 cm/ka (Fig 2). In only 600 m of water I doubt it, but are the authors sure this is not a core-stretching artifact? It is not mentioned. The slow glacial/fast Holocene sedimentation rate, differing by a factor of 10, is noteworthy. (It makes no difference to the conclusions).

**We don't think that the shift in sedimentation rate is caused by core-stretching, because the core description gives no evidence of shortening of the sediment column (mud line) or vertical disturbances and destructions (flow-in) resulting from the coring process (Skinner and McCave, 2003).
Furthermore, geochemical measurements (XRF) as well as the benthic foraminiferal fauna indicate a corresponding change in sediment regime and environmental conditions from LGM to Holocene. Especially the peak at 8.5 kyr BP (see Figure 1, below) is associated with fundamental variability in elemental composition of the sediment (Romahn et al., in prep.).
The reorganisation of the environmental setting can be partly explained by the flooding of the East African shelf since ~ 12 kyr BP, caused by rising sea level (Siddall et al., 2003). Additionally, the core position is located close to the Rufiji River delta. Comparison with dated sediment records from the same cruise (Meteor cruise M75/2) indicate that Rufiji River discharged further south during the LGM, and the river delta has been relocated to its current position during the early Holocene (N. Rippert, personal communication).**

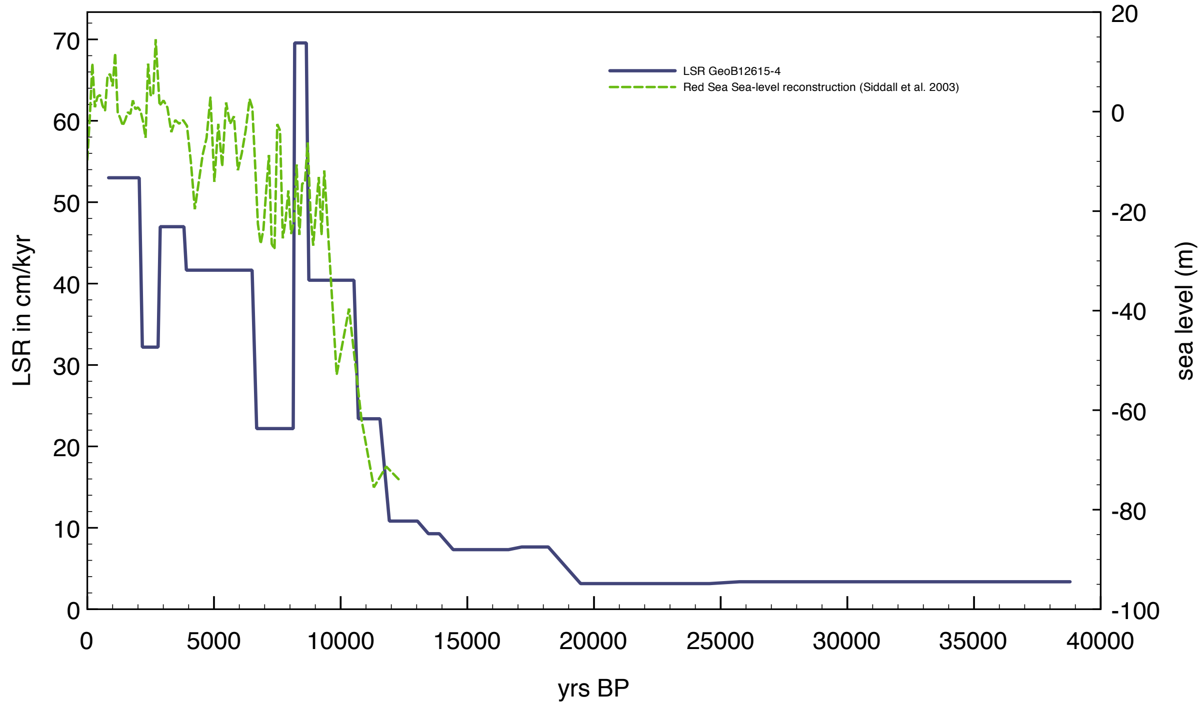

Figure 1: linear sedimentation rate of GeoB12615-4 in cm/kyr (blue) and reconstructed Red Sea Sea-level (Siddall et al., 2003)

2) The percentages of You (1998) (p 4038/9-10) may be far off relative to Fine's better constrained data, but they do draw attention to the fact that Red Sea (RSW) and Persian Gullf waters make their way down this margin from the north. The high salinity /low oxygen core of RSW with high nutrients is very obvious in the WOCE section at about 5°S, very close to this site. A low $\delta^{13}C$ would be expected in these waters. In this regard It would be useful to have a figure showing hydrographic profiles (S, O2, a nutrient, maybe DIC) that would cast light on the setting of this core site. This information will assume significance when discussing posssible shifts of water mass boundaries as the authors appear to have an open ocean hydrographic structure in mind with UCDW below AAIW and have ignored the possibility of RSW hugging the margin where their core is located. A cursory examination of the diagrams shown above suggest that at the depth of the sediment core there is a patch of lower salinity (<34.8)/high oxygen (>180 μ mol/kg) water consistent with SAMW as they claim. However examination of the N-S salinity profile of WOCE line IO7 suggests that AAIW does not get much further than 10° S near this margin. It may be a mistake to refer to AAIW/SAMW as a single entity at this location; open ocean maybe, but not here. The authors need to consider the hydrographic setting, taking into account Red Sea isolation at the lowest sea-level, and expand this section.

We refer to a recent publication that presents hydrographic profiles of the study area (Birch et al., 2013). The CTD data (Figure 2) give a good overview of

the hydrography and location of Red Sea Water, AAIW and SAMW (named Indian Ocean Central Water/IOCW there) along the continental margin. Additionally, we show a salinity profile based on a CTD cast (Fig. 3) which was taken during Meteor cruise M75/2 and is located closest to GeoB12615-4 (unfortunately, we cannot present the corresponding oxygen profile). Comparing both salinity profiles it is obvious that the determinations made by Birch et al. (2013) also hold quite well for our position at 7°S.

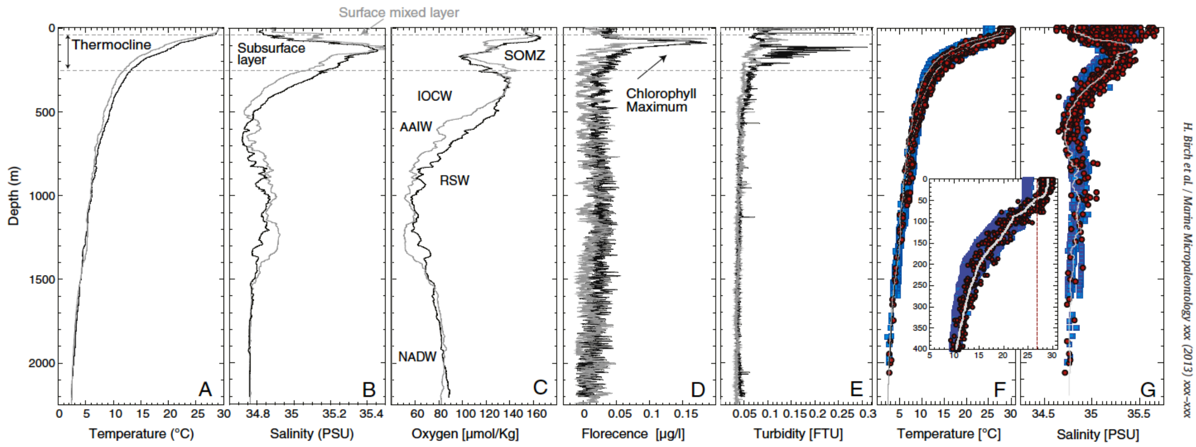

Fig. 3. Water column characteristics offshore Tanzania. A–E, CTD casts (see supplementary data) from GLOW Station 2 (black) and Station 5 (grey) sampled on 15 and 20 February 2009 respectively. F–G, Seasonal water column temperature and salinity measurements (with inset close-up of upper water column temperature) sampled in multiple years between 1930 and 1990 obtained from the World Ocean Database 2009 (http://www.nodc.noaa.gov/OC5/WOD09/pr_wod09.html). White line compares GLOW Station 5 temperature and salinity profiles. SOMZ = shallow oxygen minimum zone; IOCW = Indian Ocean Common Water; AAIW = Antarctic Intermediate Water; RSW = Read Sea Water; and NADW = North Atlantic Deep Water. See Fig. 7 for detailed view of temperature and salinity in the upper 400 m at GLOW stations. The vertical red line represents the mean annual SST (26.9 °C) estimated from the multi-site data sets. (For interpretation of the references to colour in this figure legend, the reader is referred to the web version of this article.)

Figure 2: hydrographic profiles off Tanzania, taken from (Birch et al., 2013)

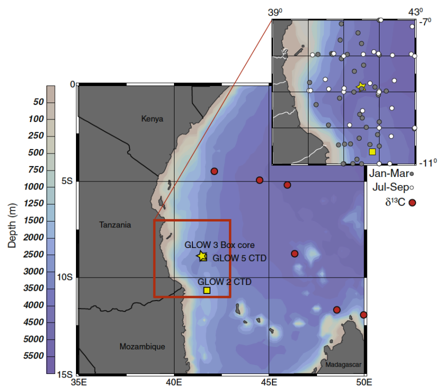

Fig. 2. Study area with box core, CTD and DIC $\delta^{13}C$ locations. Larger scale inset includes World Ocean Database 2009 stations (http://www.nodc.noaa.gov/OC5/WOD09/pr_wod09.html). Sites indicated by white and grey circles were sampled in multiple years between 1930 and 1990, and red circles in January 1996.

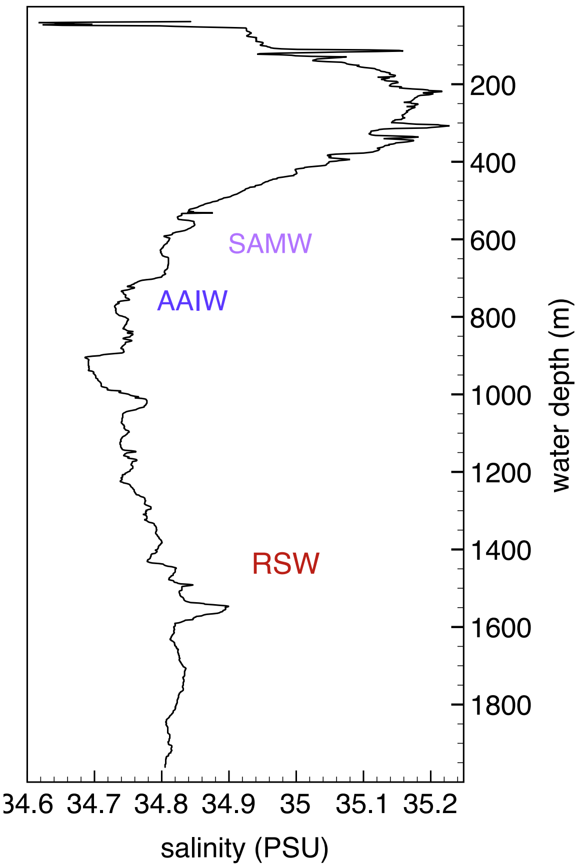

Figure 3: salinity profile from CTD station GeoB12616-6, 06°57.62S 40°23.66E

In the following we will answer the questions regarding the position of RSW first, and then we clarify/answer the question whether AAIW is present at our core location.

<u>Red Sea Water</u>:

The CTD profiles (Birch et al., 2013) as well as CTD profile GeoB12616-5 show a slight salinity maximum at 600- 1400m water depth, corresponding to low oxygen, which can be identified as the peripheral layers of RSW. We do not see the most saline core (35.2 PSU) of RSW itself along the East African continental margin, as one might expect from the WOCE section IO2. At roughly 500m water depth we find well-oxygenized water with a *lower* salinity than below, therefore we are quite confident that RSW does not affect our location today.

As there is evidence that RSW outflow was greatly reduced during the LGM (Rohling and Zachariasse, 1996), due to lower sea-level, and that RSW settled deeper in the water column during the late deglaciation and the early Holocene (Jung et al., 2001), we conclude that RSW did also not affect our study site during the last 40 kyr.
We will expand the section '2. Oceanographic framework' in the revised version of the manuskript by including information on RSW position and evolution.

AAIW/SAMW:

We are aware of the problematic identification and determination of AAIW and SAMW in the western Indian Ocean and Arabian Sea. As there seemed to be some agreement (based on earlier oceanographic studies) that AAIW does not extent farther north and maybe even does not cross the equator in the western part of the ocean, recent studies such as Fine et al. (2008) and Ullgren et al. (2012) asserted the presence of AAIW along the western margin. And in fact, the hydrographic profiles of our site clearly show the presence of a well-oxygenated, low-salinity (<34.8, as denoted) layer between 400 and 1000m, which we (and others) therefore identified as SAMW/AAIW.
We can´t participate in the ongoing oceanographic discussion on the differentiation between the two water masses and their specific position here, as we don´t have the data to do so.
The focus of our data and story is rather different, and therefore we defined AAIW/SAMW in the manuscript as following:

„In the past years, it has become common practice in paleoceanographic studies to refer to AAIW/SAMW when speaking of, in general, a glacial Southern component intermediate water, which originates from circumantarctic surface waters, subducts and subsequently spreads northwards (Bryan et al., 2010; Chen et al., 2011; Ninnemann and Charles, 1997; Spero and Lea, 2002). Although this is a simplification, it seems helpful when hypothesizing (schematically) about changes in Southern Ocean overturning (Marshall and Speer, 2012; Skinner et al., 2010). For the sake of clarity, we continue with this practice and refer to AAIW/SAMW subsequently in the text, always keeping in mind that this term represents southern component intermediate water as part of the Southern Ocean overturning circulation."

For example, Pena et al. (2013) used the term „Southern Ocean intermediate waters (SOIW)" instead of AAIW/SAMW to address a schematical concept rather than a well-defined modern water mass. Maybe this is a better option in

order to avoid confusion. We will replace the term AAIW/SAMW with SOIW with reference to Pena et al. (2013) in our manuscript.

3) In section 5.2.4 of the discussion the authors rather over-extend themselves. This discussion goes well beyond the capacity of the author's data to resolve the problem of water mass production rates. Their data have nothing to say directly about AAIW production rates and indirectly the carbon isotope ratios have several possible controls as enumerated in section 5.2.2 : Gas exchange fractionation, productivity, water mass mixing and so on. No causal connection between benthic δ13C and water mass production rate is set out here, just reference to others, often with larger databases, who have made a tenuous set of assumptions leading to a conlusion regarding dynamics. The authors appeal to shoaling of 'CDW' (that would be Upper CDW, (UCDW)), a water mass marked by oxygen depletion, high nutrients and low δ13C. But on this margin, if the Red Sea Outflow was active by 17 ka then the water mass here at some depth would likely have been RSW with similar properties but high salinity. This is a can of worms and the authors should stay well clear of it by removing this speculative section

The reviewer is right, our data cannot resolve the problem of AAIW production rates, nor should we try to link the benthic ^{13}C data we present to water mass production rate at all. We see that this section might appear to be written a little too confident, especially since others made assumptions on much larger databases.
But besides this, we think it is important to point to an interesting fact: There are currently two hypotheses on deglacial SOIW variability, and they conflict with each other, if both are proved with ^{13}C records from intermediate depth.
If we state that SOIW transferred both Southern Ocean temperature variability and an aged deep water signal via SOIW to the Indian Ocean, then this story conflicts with Jung et al. (2009), who interpreted the NIOP905 record completely different. As core NIOP905 is located so close to our study site and its benthic ^{13}C record not only does *not* match (which should be the case since GeoB12615-4 and NIOP905 both are supposed to show SOIW) but anti-correlates, we strongly feel that we have to mention and discuss this aspect.

We will rewrite the section 5.2.4 completely. Instead of speculating on a global scale, we will highlight the conflicting datasets of NIOP905 and GeoB12615-4 solely. We also add that further data is needed and will help to combine and constrain hypotheses on SOIW variability in the past.

Anonymous Referee #2

1) Page 4, Section 2 Oceanographic framework, Please add details of productivity in the study area. It will help in assessing the possible contribution of downward flux of organic matter in driving benthic stable isotopic ratio, as the core is located at only 446 m depth.

The western Indian Ocean experiences strong seasonal variability in surface ocean circulation, driven by the Inter-Tropical Convergence Zone (ITCZ). North of the equator, seasonal reversing monsoonal winds and currents lead to phases of upwelling and downwelling, which results in large variability in thermocline depth and nutrient availability in coastal regions. Seasonal supply of cool and nutrient-rich waters causes high planktic productivity here. In contrast, south of ~4°S surface waters are stratified year-round and are characterized by low nutrient conditions (Birch et al., 2013;McClanahan, 1988), which also holds for our study site at 7°S.

We will include this information in the revised version of the manuscript.

2) Page 5, Section 3.2, Add a table with AMS date details including which dates are monospecific while which ones are based on mixed species.

We are very sorry, but we made a mistake in the manuscript: The age model for GeoB12615-4 is not based on both monospecific and mixed samples of planktic species; we used mixed samples for all AMS measurements instead. We thank the reviewer for asking for details here, so we can correct this mistake. Below we listed all AMS radiocarbon analyses with core depth, Lab label, information on the planktic foraminifera species we picked, the measured age and calendar age range minimum and maximum (1 σ).

Core depth (cm)	Label	Planktic foraminifera species	Age dated (yrs BP)	Age calibrated min (yrs BP)	Age calibrated max (yrs BP)
20	KIA 46245	G.ruber, G. sacculifer, G. aequilateralis	1375 ± 25	727	805
88	KIA 43717	G. sacculifer, G. aequilateralis, G. conglobatus	2555 + 35 / -30	1996	2102
112	KIA 43716	G. sacculifer, G. aequilateralis, G. conglobatus	3175 ± 35	2753	2836
160	KIA 43715	G. sacculifer, G. aequilateralis, G. conglobatus	3995 ± 30	3761	3871
272	KIA 44976	G.ruber, G. sacculifer, G. aequilateralis	6225 ± 35	6450	6560
308	KIA 43714	G. sacculifer, G. aequilateralis, G. conglobatus	7820 ± 45	8062	8193
344	KIA 46244	G.ruber, G. sacculifer, G. aequilateralis	8305 ± 40	8577	8713
420	KIA 44977	G.ruber, G. sacculifer, G. aequilateralis	9795 ± 50	10481	10570
444	KIA 44978	G.ruber, G. sacculifer, G. aequilateralis	10520 ± 55	11501	11602
461	KIA 46243	G.ruber, G. sacculifer, G. aequilateralis	11685 ± 55	12943	13116
468	KIA 44979	G.ruber, G. sacculifer, G. aequilateralis	12590 ± 60	13815	13969
488	KIA 46242	G.ruber, G. sacculifer, G. aequilateralis	13980 ± 70	16475	16776
500	KIA 43713	G. sacculifer, G. aequilateralis, G. conglobatus	15700 + 130 / -120	18083	18307
504	KIA 44424	G.ruber, G. sacculifer, G. aequilateralis	15710 ± 80	18115	18287
524	KIA 46241	G.ruber, G. sacculifer, G. aequilateralis	21090 ± 140	24333	24782
572	KIA 44980	G.ruber, G. sacculifer, G. aequilateralis	34460 + 770 / -700	38628	38958

Table 1: AMS radiocarbon analyses details.

3) Page 5, Section 3.2, Does an age model based on both monospecific and mixedspecies dates lead to age discrepancy due to depth dependent 14C reservoir age?

No, we do not expect any age discrepancy, since we exclusively used mixedspecies samples (see above). The abundance of planktic foraminifera in GeoB12615-4 is low, so we combined different mixed-layer-dwelling species to get enough material for radiocarbon dating. We picked equal percentages of each species per sample. As the table shows, we used the species *Globigerinoides ruber white s.s.*, *Globigerinoides sacculifer*, *Globigerinella aequilateralis* and *Globigerinoides conglobatus,* as they all live between 50 and 100m water depth in the Indian Ocean (Hutson, 1977;Bé and Hutson, 1977;Birch et al., 2013).

4) Page 5, Section 3.2, How much is the error associated with age model? It will help in assessing the timing of events mentioned in the text.

We give an overview of the radiocarbon dating error in table 1. As we wrote in the manuscript, the untuned age model is based on radiocarbon ages with Δ R=140yrs, and we did not apply a change of the surface reservoir age through time. As the dating error is quite small, we think it makes a trivial contribution and that possible changes in surface reservoir age will contribute much stronger (see Referee 2, reply 10).

5) Page 5, Section 3.3, As the authors picked only 6-8 G. ruber and 3-4 Planulina ariminensis for stable isotopic analysis, is there a possibility of a seasonal bias in the record?

There is evidence that *G. ruber* shows some seasonality in the Mozambique channel. But although *G. ruber* reproduces in the warmest months, two recent studies focusing on this region found that the offset between flux-weighted SST of *G. ruber* and mean annual instrumental SST is very small and far less than the mean difference in SST between winter and summer (Fallet et al., 2010). Therefore, *G. ruber* reflects mean annual SST reliably due to natural averaging processes (Birch et al., 2013). We therefore expect no seasonal bias in the records based on *G. ruber.*

6) Page 7, Line 6, The d18O planktic enrichment at _13 kyr is based on only two data points. As authors have drawn far reaching conclusions based on this enrichment, is it possible to supplement it with additional close spaced samples. It will help to strengthen the arguments put forward by the authors.

We performed additional re-measurements for the core section representing the deglacial. We calculated an average $\delta^{18}O$ value for each sampling point. Figure 4 shows the individual measurements (crosses) as well as the calculated average data (open circles) used in the manuscript.

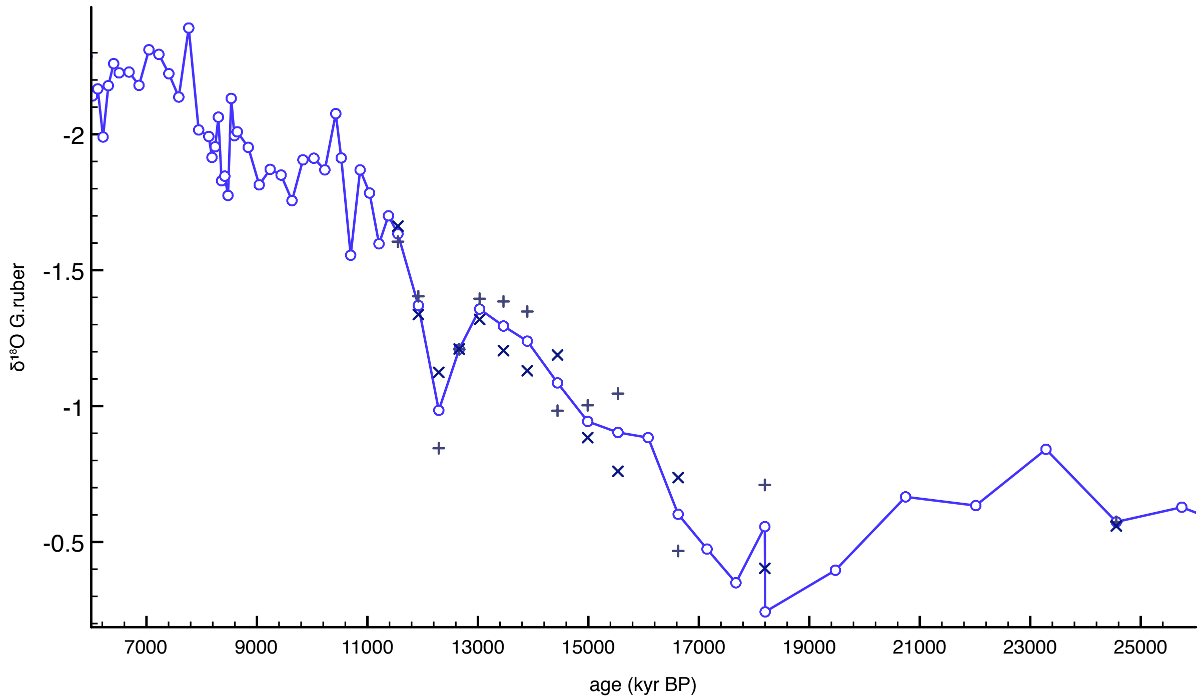

Figure 4: individual data points δ ^{18}O *G.ruber*

7) Page 7, Line 7-8, The overall structure of the d18O ruber matches well with Antarctic temp record rather than with NGRIP d18O, especially the beginning of deglacial depletion of d18O ruber, which is contrary to what the authors have mentioned.

This is partially true, the δ ^{18}O *G.ruber* record starts to increase simultaneously with the benthic δ ^{18}O. We did not discuss this aspect, since we applied no ice-volume correction and show measured δ ^{18}O calcite of both planktic and benthic forams. As a result, one has to consider that both signals still incorporate the same deglacial δ ^{18}O decrease associated with global ice volume changes. Furthermore, the δ ^{18}O *G.ruber* also contains a temperature component (resembling Antarctica, see the Mg/Ca data), which might be responsible for this early deglacial decrease, too.
Therefore, we decided to focus on the more conspicious timing of YD and, how we see it, the ACR-like pausing in the benthic record when discussing potential links to Northern and Southern Hemispheres.

8) Page 7, Results, Please include core-top estimated Mg/Ca SST as well as modern SST, in order to get an idea about the validity of Mg/Ca SST.

***G.ruber* is supposed to be the shallowest living species and most suitable for SST reconstructions in this region (Birch et al., 2013;Fallet et al., 2010). The long-term average SST is 27.3°C with an annual range of approximately 4°C (25°–29°C) around Mafia Island, close to our core site (Damassa et al., 2006). Table 2 shows the uppermost core samples that have been analysed for Mg/Ca, as well as the corresponding radiocarbon age. The SST data from 4 and 8 cm core depth are younger than 766 yr BP and therefore represent the core top. The calculated SST are higher than the mean annual SST, but at least for 8 cm within the range of a standard deviation of ±1.1- 1.4°C (Dekens et al., 2002;Anand et al., 2003). Taking into account the higher SST during late winter in this region we think our record reliably records past SST at the study site.**

Core depth(cm)	age (yrs BP)	SST in °C
4		29.95
8		28.40
12		
16		
20	766	28.60
24		
28		29.15
32		
36		28.48

table 2: core top estimated Mg/Ca SST

9) Page 7, Line 23, I'm not convinced about the argument about d18Oplanktic being similar to Northern Hemisphere climate variability.

See above (Referee 2, reply 7)

10) Page 8, Line 3-4, Several SST records from the Indian Ocean, especially the latest high resolution SST record by Saraswat et al, 2013, EPSL, does not match with the Antarctic temp record.

Saraswat et al. (2013) present a SST record from the Lakshadweep Sea, which does not exactly match Greenland, Antarctic or some tropical records, but it does *precede* initial Antarctic warming and the ACR for 1 kyr. The authors discuss greenhouse gas forcing as the dominant control on deglacial warming in the Lakshadweep Sea. However, the SST signal precedes atmospheric CO_2. On a closer look, our SST record also precedes Antarctic temperature variablility for some hundred years: The initial warming starts at the latest at 18.2 kyr, the ACR starts at 15 kyr. Furthermore, the Tex_{86} -temperature record of NIOP905 (off Somalia) resembles Antarctica but precedes for more than 1 kyr (Huguet et al., 2006).
We suggest that this lead of Indian Ocean SST over Antarctic temperature could be the result of increased surface reservoir ages during H1, providing that the hypothesis of old carbon transported by SOIW (see manuscript for references) is correct. In particular, the results of Bryan et al. (2010) indicate much older thermocline/intermediate reservoir ages in the Arabian Sea during H1. Considering the coastal upwelling in the Lakshadweep Sea, older surface reservoir ages might have likely affected the study site of Saraswat et al. (2013). In any case, increased reservoir ages will result in a biased age model with preceding events for all sediment records, which are influenced by SOIW.

11) Page 7, Line 24, Again the d18O benthic is continuously increasing during ACR unlike Antarctic temp record.

Indeed, the $\delta^{18}O$ benthic record does not show a pronounced ACR-like increase. But the steep decrease during H1, a phase of slackening simultaneous to ACR and no YD like the surface record, fits much better to Antarctic climate variability, with regard to timing and duration of the events. Furthermore, we have to consider that the $\delta^{18}O$ benthic record also reflect the deglacial $\delta^{18}O$ decrease associated with global ice volume changes.

12) Page 8, Line 10-11, The assumption that SST in the entire western Indian Ocean is controlled by Antarctic Temperature is too-much generalization, as surface water in the entire Arabian Sea is mainly sourced either from Red Sea or surface runoff from Bay of Bengal. Please modify

We think the assumption more or less also holds for the Arabian Sea, as there are some Antarctic-style deglacial surface temperature records (Govil and Naidu, 2010;Huguet et al., 2006;Saraswat et al., 2013) from this area. Bryan et al. (2010) give a good discussion why this is plausible: SAMW and AAIW ventilate much of the thermocline and intermediate waters of the Indian Ocean, including the Arabian Sea (Fine et al., 2008;You, 1998). Compared to the outflow water from marginal seas and Bay of Bengal, the volumetric contribution of intermediate waters from the Southern Ocean is much larger (Fine, 1993;You, 1998). Furthermore, outflow waters were strongly reduced or eliminated due to the lower sea level during the LGM (Rohling and Zachariasse, 1996).
Nevertheless, we will change the general term "western Indian Ocean" to "tropical western Indian Ocean", since both the hypothesis by Kiefer et al. (2006) as well as our data focus on this specific region only.

13) Page 8, Line 18, How relevant is the assumption that the EPICA temp record is the representative or average of entire Antarctic deglacial warming, especially in the context of recent WAIS temp record?

In the light of the recent WAIS temperature record (WAIS Project Members, 2013) the EPICA record (Jouzel et al., 2007) seems inappropriate to represent Antarctic temperature, as deglacial warming in West Antarctica precedes about 2 kyr and the data suggest a more active role for the Southern Ocean during the onset of the deglaciation. If the WAIS data were published (14 August 2013) when we submitted our manuscript (7 June 2013), then we might have used this record instead. However, we do not think that results and conclusions of our study are significantly affected.

14) Page 9, Line 9, Though the beginning of CIME in this core is same as that in previous reports, the timing of most depleted d13C during CIME in this core (towards the end of deglaciation) is clearly different than previously reported (beginning of deglaciation). Please explain.

We do not have an explanation for this. We find it remarkable that even in the light of the tuned age model of Bryan et al. (2010), our ^{13}C benthic record and their ^{14}C records fit together so well, especially the „oldest" signal in the Arabian Sea, and the lightest signal at our location. Additionally, we would like to refer to Murgese et al. (2008), who observe a CIME in their surface record (*G.ruber*) at 10 kyr off Western Australia as well.

We do not know exactly why this event at 10 kyr occurs in the Indian Ocean, as we noted in our conclusion chapter (page 16, line 6- 9): „Finally, the question remains unanswered why Indian Ocean AAIW/SAMW shows a pronounced CIME and corresponding radiocarbon minimum (Bryan et al., 2011) during the EH, while other regional (Marchitto et al., 2007) and global records (Schmitt et al., 2012) precede it.“

15) It is possible that the late deglacial termination timing of CIME is linked with the strengthening of the SW monsoon as the record comes from a region highly affected by upwelling induced productivity, which will lead to enhanced downward flux of light carbon organic matter to the bottom.

We disagree. Neither the TOC record shows increased rain of organic matter (Figure 5), nor does the benthic faunal composition (Romahn et al., in prep.) give any evidence of high productivity during that time period.

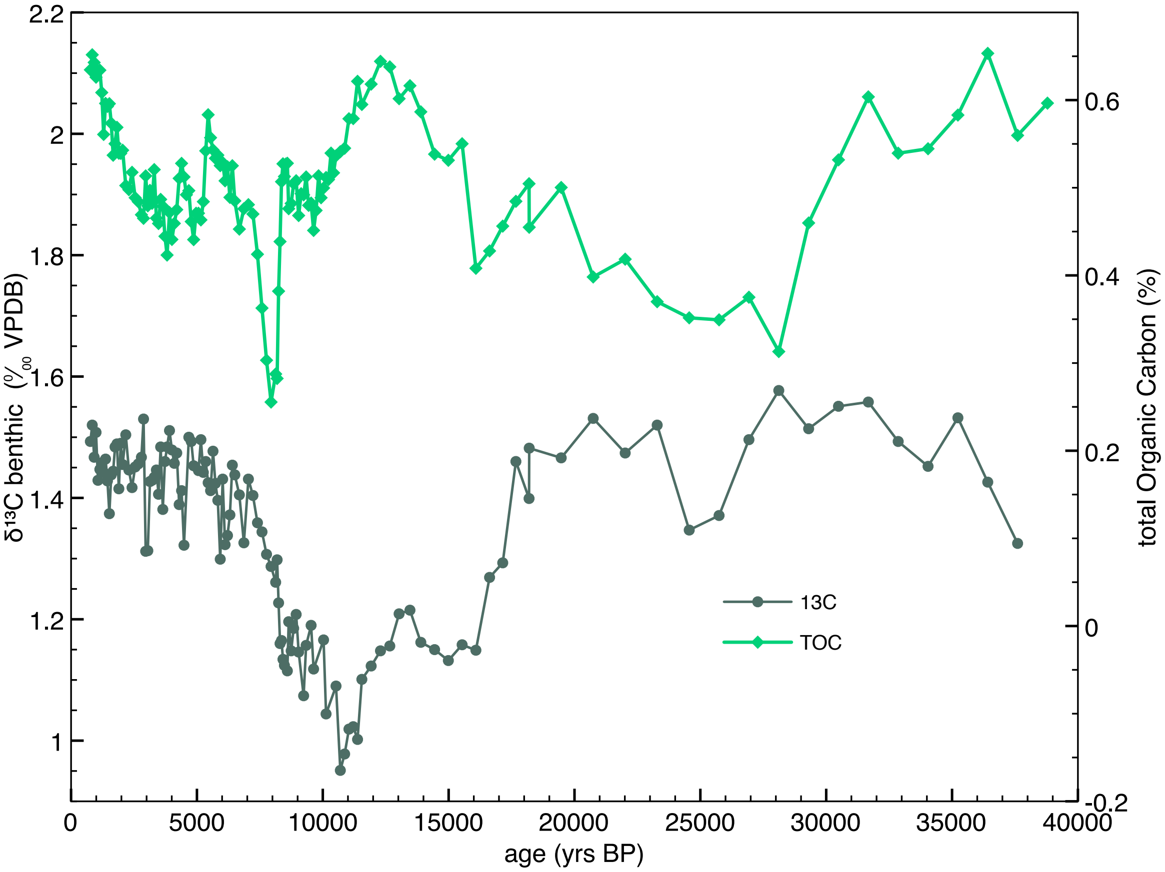

Figure 5: total organic carbon content (%) and benthic ^{13}C record of GeoB12615-4

16) Page 11, Line 24, can you please define the high southern latitudes (from what to what S)

We changed the term “high southern latitudes“ to “along the SAF“: “During the LGM, $\delta^{13}C_{DIC}$ of sea-ice free surface seawater along the SAF was high due to low temperatures and high wind speed.“

17) Page 12, Line 8, It is difficult to accept ’Southern Ocean surface water temperature variability’ as the cause of global occurrence of CIME, as a few records from tropical Indian Ocean not affected by AAIW/SAMW also have a distinct deglacial CIME and as authors also mention in the very next section. Please modify the text.

We mention in the text that there are some puzzling results, such as CIME without radiocarbon depletion (Cléroux et al., 2011). But we question if the argument holds that a site, which is not affected by AAIW/SAMW *today,* could not have been affected in the past, especially when fundamental reorganisation of the Southern Ocean Overturning took place. As some authors (Rickaby and Elderfield, 2005;Pahnke et al., 2008;Pahnke and Zahn, 2005) speculated, AAIW/SAMW might have been different both in spatial and vertical extent in the past.

18) Page 12, Line 8, The authors also mention (Page 4, Line 10) that several of the northern tropical Indian Ocean records are from regions affected by Red Sea water. Therefore they should be cautious while proposing Southern Ocean processes as the sole cause of CIME.

We refer to our response on Referee #1, reply 2.

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