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Interactive comment on "The last interglacial (MIS 5e) cycle at Little Bahama Bank: A history of climate and sea-level changes" *by* Anastasia Zhuravleva and Henning A. Bauch

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Anonymous Referee #2

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Reviewer's comment: SUMMARY: Zhuravleva and Bauch present a detailed consideration of the climate evolution of the Last Interglacial (LIg) for a core site on the Little Bahama Bank (LBB) using faunal assemblage and scanning XRF techniques. The high resolution faunal assemblages nicely resolve hydrographic oscillations at the site for the LIg reflecting both the insolation driven and AMOC modulated migration of the

ITCZ for this region. I would recommend the following amendments/clarifications: (a) change of title to better reflect the content of the paper; (b) removal or at the very least restructuring of the discussion of sea level. This section could be significantly trimmed and simplified (no new insights offered but a nice corroboration). Alternatively, if the authors wish to retain the sea-level discussion, then discussion of other sea level evidence from the region, glacio-isostatic adjustment (GIA) processes etc. are needed. (c) clearer discussion of the teleconnections between N Atlantic oceanic changes (i.e., variation in AMOC), the migration of the ITCZ and surface hydrographic change at MD99-2202.

Author's response: Please note that in accordance with the journal requirements, all changes in the manuscript are provided in a marked-up version below (track changes in Word, converted into a *.pdf file). The line numbers used in the current Author's response refer to the marked-up version, which could be find in the supplement.

a) The title has been changed to better reflect the main finding of the paper, i.e., subpolar forcing on the subtropical climate during the last interglacial;

b) The discussion about sea level is significantly reduced and focuses now exclusively on relative sea level changes in the Bahama region and its implications for regional sedimentary processes (ls. 440-544);

c) Links between AMOC strength and ITCZ shifts are discussed now in the introduction (ls. 68-78) as well as briefly mentioned in the discussion (ls. 986-1078).

Reviewer's comment: GENERAL COMMENTS: The manuscript, in general, reads well. However, the structure and focus of the paper requires further thought. A clear statement of the research questions was missing and is reflected in the general tone of the introduction (and the manuscript generally).

Author's response: The introduction has been rewritten, in attempt to outline the aims of the manuscript (ls. 95-110), used proxies (ls. 119-128) and testing hypothesis.

Reviewer′s comment: 1. Title

I found this to be somewhat misleading. The data in Zhuravleva and Bauch is not a sea-level record per se, rather a record of increased aragonite supply to the core site during interglacials, with these intervals of increased aragonite production/supply likely corresponding to < -6 m relative sea level (RSL) due to the generally shallow nature of Little Bahama Bank (i.e., you can infer periods of <-6 m relative sea level). This work nicely corroborates the Lantzsch et al., 2007 and Chabaud et al., 2016 studies but isn't a sea-level story. What is new and interesting the palaeoceanographic evolution of the Last Interglacial (LIg) at the site, and the interplay of interglacial climate (movement of the ITCZ etc.). I would suggest changing the title to better reflect this.

Author′s response: The title has been changed.

Reviewer′s comment: 2. Sea level

This section requires some restructuring to help the reader. The definition of the "flooding interval" (and corresponding relative sea level, <-6 m) is key to this section of the manuscript but I struggled to clearly follow the logic of how you defined the flooding interval using your records and why a -6 m RSL for this interval was appropriate. The connection between the flooding interval and inferred RSL of < -6 m was found almost at the end of the section (line 222 to 226) when it should be at the start. All the information is there but the reader has to work hard to follow the argument.

Perhaps something along the lines of;

modern LBB lagoon is shallow, with an average water depth < 6 m (Williams, 1985);

tectonic stability of the region (refs needed);

during the LIg, increasing RSL at the site floods the generally shallow bank and increases the area for aragonite production (i.e., the carbonate shedding model, Droxler and Schlager, 1985; Schlager et al., 1994);

Conversely, during glacial intervals, the top is exposed which limits the production and export of aragonite;

As such, we define the flooding interval (and inferred <-6 m RSL) is defined by an increase in the sedimentation rate, increase in wt % aragonite, increased Sr/Ca ratio, increase % Globigerinoides/decrease in numbers of G. menardii.

This could then usefully be followed with your discussion of very high values of Sr/Ca due to increased saltwater (lines 192 to 211). Perhaps shade these 'problematic' Sr/Ca intervals in subsequent figures? You should also note the truncation of the Sr/Ca record in caption of Figure 4.

I would suggest confining discussion of sea level in this section to that suggested above. If you wish to make more of the sea level story, then greater consideration of other Bahamas sea-level records, as well as those from the wider area is needed. For example, the +6m notch on Little Sale Cay (LLB) (Neumann and Hearty, 1996), other geomorphological records (e.g., Hearty and Kindler, 1995; Neumann and Hearty, 1996), the elevated Last Interglacial (LIg) coral records of Chen et al 1991, Hearty et al., 2007, Thompson et al., 2011 and the regionally extensive erosional surface that is suggestive of a sea-level oscillation within the LIg (Bahamas, Florida and Yucatan; Chen et al 1991, Hearty et al., 2007, Blanchon et al., 2009; Thompson et al., 2011).

How does the timing of the highstand from the coral/other records from the Bahamas compare to the timing of the interval of enhanced aragonite production (and inferred sea levels < -6 m)? How do changes in hydrography (variations in faunal assemblages) at the site compare to the timing of the Bahamas LIg highstand? The broad correspondence between climate (δ18OG.ruber) and relative sea level (RSL) (weight % aragonite) is hinted at in lines 138 to 141 but could be developed further if you wish to keep the sea-level discussion.

Any discussion of the LIg highstand in a general sense (i.e., the eustatic record) (lines 227 to 231) and Bahamas RSL will need to consider glacio-isostatic (GIA) processes, given the intermediate location of the site on the peripheral bulge of the former Laurentide Ice Sheets. There will be a regional expression of the LIg highstand; the Bahamas would "see" a "late" LIg highstand compared to eustatic sea level (e.g., Figure 6 in Stirling et al., 1998). There seems to be good correspondence between the age of your "flooding interval" at the site (i.e., sea level < -6 m) and the predictions of RSL (Stirling et al., 1998, their Figure 6). Given that the records presented are not strictly a sea level record, rather incidence of increased aragonite production/export, and seems to corroborate previous studies rather than adding anything new, I would confine this section to just a brief consideration of the timing of your "flooding interval".

Author′s response: We agree on the importance of consideration of glacio-isostatic adjustment processes for interpretation of our aragonite-related records in terms of eustatic sea level change and also for comparison with other sea level reconstructions and curves (ls. 746-749). Therefore, we have restructured the sea-level discussion, in accordance with the Reviewer′s comment and significantly reduced this part, restricting the discussion to the relative sea level change, defining the "flooding interval" and associated changes in geochemical and sedimentary data around Bahama Banks (ls. 440-544).

The study by Carew and Mylroie (1995) was cited with regard to the tectonic stability of the Bahama region (l. 441). Truncation of the Sr/Ca record is now mentioned in the figure caption (Fig. 3, l. 1666).

Reviewer′s comment: 3. Palaeoceanographic reconstruction

This section is much more coherent and well written. I would recommend this as the focus of the manuscript.

The discussion, while nicely documenting the site/regional changes during the LIG, was lacking in consideration of the mechanisms. This section would be strengthened by a clearer exposition of the mechanisms linking ITCZ position, insolation (precession and the migration of the ITCZ to the warming hemisphere) and AMOC (i.e., modifica-

tion of the thermal condition at the surface, due to interactions with the ocean, that in turn act to drive atmospheric circulation). This is well documented for the last deglaciation and glacial period (and in modelling studies), where N. hemisphere extra-tropical cooling (brought about by variations in the AMOC, forced by freshwater inputs) lead to an interhemispheric thermal gradient and a southward shift in the ITCZ (e.g., review of Chiang and Friedman, 2012 or Schneider et al., 2014). This would help the reader to place the different records (Cariaco, MD99-2202 and Site 1063) within a broader climatological context. Again, all the 'threads' of the story are there, it just needs a stranger framework.

For example, I found the correspondence between the % G. ruber and G. sacculifer and the XRF Mo count of the Caricao Basin striking (demonstrating the clear record of ITCZ shifts at the LBB) but the link to N. Atlantic surface density changes (AMOC slowdown with surface freshening e.g., Galaasen et al., 2014 etc.) and positive the δ18O G. ruber excursion and faunal changes at MD99-2202 and southward migration of the ITCZ (Cariaco Mo, MD99-2202 decrease in % Globigerinoides) weak. A short introductory paragraph should fix this.

Author′s response: We have included information on coupling between high-latitude forcing (AMOC strength) on the ITCZ position with its further influence on upper ocean properties in the introduction (ls. 68-78) and as well as briefly in the discussion (ls. 986-1078).

Reviewer′s comment: A southward shift of the ITCZ, strengthens of the trade winds increases the eolian input from the Sahara, resulting in reduced Al/Ti in Cariaco. These episodes of decreased Al/Ti ratios in Cariaco correspond to elevated salinities in the Caribbean (e.g., Yarincik et al 2000). I assume there is clear correspondence between the Cariaco Al/Ti and Mo records and hence to your % of tropical species?

Author′s response: Despite similar approach for age model construction (alignment of stable isotope data to SPECMAP/benthic stack), there is no correspondence between the Al/Ti record from Yarincik et al. (2000) and the Mo-data from Gibson and Peterson (2014) and, therefore, our relative abundance of the tropical species. This is likely due to low-resolution of the first record, providing general information about atmospheric circulation changes mainly on glacial-interglacial timescales (Fig. 1).

Reviewer′s comment: Do you see an increase in iron (with increased dust transport) in your record during the positive the δ18OG.ruber excursion ~127 ka? (plotting this on a log scale for the LIg might help).

Author′s response: We don't find any response in iron accumulation during the 127-ka event.

Reviewer′s comment: Dust inputs are probably better reflected in the XRF core scanning Ti record, given that your Fe inputs could change with a number of factors.

Author′s response: We agree with this statement, but our XRF Ti measures appear to be very low and could be strongly influenced by Cl content (Fig. 2), therefore they were not considered in the study. We agree on the comment and restrict the interpretation of our Fe content, due to the "variety of additional effects that may have influenced our Fe-record" (ls. 816-820).

Reviewer′s comment: It would be interesting to compare to your faunal assemblages and a calculated δ18Oseawater for MD99-2202.

Author′s response: Please, see further below.

Reviewer′s comment: Additionally, is there any correspondence to the dated palaeosols on the Bahamas (Muhs et al., 2007)?

Author′s response: Study of Muhs et al. (2007) reveals two major sources for the dated palaeosols (~125 ka) on the Bahamas: African dust and Mississippi River valley loess. Today particularly strong input of African aerosols occurs during summer time, when the ITCZ position is to the north. The study, thus, suggests variable parent materials for eolian inputs, possibly with a greater role of Mississippi River valley loess at times

of southward ITCZ shifts (glaciations). The text has been improved accordingly (ls. 816-820).

Reviewer′s comment: Given you have faunal assemblage data, could you calculate a transfer function/MAT sea surface temperature? From this you could then calculate δ18Oseawater at the site to think about density changes during the LIg.

Author′s response: While we agree with the suggestion to look at density changes and inferred calculated salinities, we assume that the use of Mg/Ca-based temperatures, derived from similar species used to obtain δ18O values, would be more plausible. As we don't have Mg/Ca-ratios for the investigated samples, we rather suggest considering proportions of selected species for relative temperature/salinity change reconstructions.

Reviewer′s comment: lines 372 to 381 - I found this paragraph to be highly speculative and not well supported by your data (I struggled to see the change in the sedimentological properties in your figures, even with the help of the white arrows). I would suggest removing this section.

Author′s response: The section has been removed.

Reviewer′s comment: TECHNICAL CORRECTIONS

Referencing: greater care needed with referencing. Please check manuscript. For example, the depth of submersion of the LBB (and origin of the -6 m quoted often in the paper) is Neumann and Land, 1975. Similarly the carbonate shedding model (upon which the inferred sea-level story is based) comes from Droxler and Schlager, 1985; Schlager et al., 1994

Author′s response: We tried to improve the referencing. Neumann and Land, 1975 and Droxler and Schlager, 1985 are cited (l. 444 and ls. 244, 250, 283, 444, respectively).

Reviewer′s comment: line 124 - what potential biases are you refereeing to? Please give appropriate references for these.

Author′s response: PhD thesis of Chabaud, 2016 has been cited (l. 336). This study extensively discusses the potential biases for XRF measurements in periplatform sediments, related e.g., to coarse-grained intervals, increased sediment porosity and/or seawater content.

Reviewer′s comment: line 242 - what is the derivation of "minimal ice volume interval" and it's reference?

Author′s response: This part has been removed.

Reviewer′s comment: line 332 - reference required for the "full resumption of the AMOC. . . only by ∼124 ka"

Author′s response: Studies of Hodell et al. (2009) and Barker et al. (2015) are cited (l. 1081-1082).

Reviewer′s comment: 2. Other: line 64 - capitalisation of "intertropical convergence zone"

Author′s response: Done (ls. 15-16, 69-70, 1651).

Reviewer′s comment: line 133 - unit of measurement missing, add "m".

Author′s response: Done (l. 344).

Reviewer′s comment: line 330 - please clarify or add examples of the "additional forcing" or add "as discussed below".

Author′s response: The term "additional forcing" (now changed to "other forcing factors at play", l. 986) is clarified (i.e., AMOC control on the ITCZ position, ls. 986-1078).

Reviewer′s comment: line 324-325 - Clarification of the age of the cooling/increase salinity event ∼127 ka is needed. The ∼127 ka age for this event is derived from your age model, whereas the U-series ages for the correlative event in core 152JPC (Bahamas, Slowey et al., 1996), dated (in duplicate) above and below the event to

∼121 ± 3 ka and 125.6 ka (mean of 127 ± 4.8 and 124.1 ± 5.1) respectively. (Note, the relatively large age uncertainties associated with these U-series ages)

Author′s response: We agree with the Reviewer′s comment both with regard to only subtle agreement between our age estimates and direct U-Th dating of the cooling/salinification event, as well as large age uncertainties associated with U-series ages. Therefore, this section has been removed.

Reviewer′s comment: 3. Figures: typo - Axis label in Figure 4D should read "# G. menardii" rather than "# G. menradii" I would remove the sea level records (H), or if you choose to retain these and your discussion of sea level, then you should remove the dashed blue line "RSL above -6 m" from the lowermost panel (H).

Author′s response: Fig. 4 has been removed, instead XRF data and # G. menardii are shown together in Fig. 3, plotted against age.

Reviewer′s comment: REFERENCES CITED: Blanchon, P., Eisenhauer, A., Fietzke, J. and Liebetrau, V., 2009. Rapid sea-level rise and reef back-stepping at the close of the last interglacial highstand. Nature, 458, 881.

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Please also note the supplement to this comment:
https://www.clim-past-discuss.net/cp-2018-38/cp-2018-38-AC2-supplement.pdf

Interactive comment on Clim. Past Discuss., https://doi.org/10.5194/cp-2018-38, 2018.

Supplementary Figure 2 for the Author´s response

to the comments of the Anonymous Referee #2

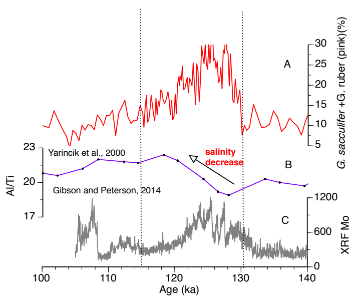

Fig. 1. Comparison of proxy records from the Bahama Bank (A) and Cariaco Basin (B-C). All records were interpreted in terms of ITCZ movements. (A) is from the current study, (B) is from Yarincik et al. (2000), (C) is from Gibson and Peterson (2014). B and C are plotted in accordance with their published age models.

Fig. 1.

Supplementary Figure 2 for the Author´s response

to the comments of the Anonymous Referee #2

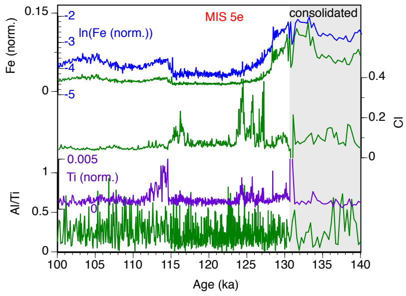

Fig. 2. XRF-scan data from core MD99-2202.

Fig. 2.