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Climate of the Past
Discussions

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

Anastasia Zhuravleva and Henning A. Bauch

azhuravleva@geomar.de

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A. Bahr (Referee)

andre.bahr@geow.uni-heidelberg.de

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Reviewer′s comment: GENERAL REMARKS: The authors present a comprehensive collection of faunal, stable isotope and sediment-geochemical data from Little Bahama Bank (LBB) core MD99-2202 encompassing MIS 5e in high temporal resolution. Such high-resolution low-latitude (27°N) records of the penultimate Interglacial are rare, but

important to constrain the climatic variability of previous interglacials when compared to the Holocene.

The authors argue that the surface ocean variability at LBB reflects changes in the position of the subtropical gyre and tropical warm pool, responding to latitudinal shifts of the ITCZ that are driven by insolation and AMOC changes. In addition, the sea level history at LBB is discussed, mainly based on the sedimentary composition (aragonite content) of the sediment. In general, the author's interpretations are well-founded by proxy evidence and supported by previous studies. Some problematic aspects of the interpretation are discussed below, but do not interfere with the general messages of the paper.

The manuscript is generally well-written and the study undoubtedly has its merits as a valuable contribution for the understanding of low-latitude climate variability during MIS 5e as well as the low-high-latitude feedbacks. However, the manuscript lacks a clear focus. This regards in particular the introduction - it should include more concise statements regarding the aims of the study, e.g. hypotheses to be tested and specific questions that should be solved. At the moment the introductory paragraphs (as well as the abstract ad conclusions) are very general, partly with a focus that hinges strongly on local aspects of sedimentary dynamics at LBB. Hence, I would strongly advocate to sharpen the focus of the manuscript, as the reader is left with the impression that the study confirms previous conceptual models (e.g. regarding the displacement of the ITCZ during MIS 5e) but wonders about the specific take-home-messages and new insights retrieved from this study. I therefore recommend the authors to re-write the respective parts of their manuscript (in particular the introduction; see also specific comments below) to avoid underselling of their data.

Author′s response: Please note that in accordance with the journal requirements, all changes in the manuscript are provided in a marked-up version (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.

The introduction has been rewritten, in attempt to provide a clearer focus of the study. Aims, methods and problematics are now also discussed.

Reviewer′s comment: SPECIFIC COMMENTS: Abstract: As discussed above, the abstract should be more specific about what exactly the authors want to study. At present, the first three sentences concentrate on the local/regional aspects concerning LBB, but in fact the data can be used to infer much more general insights into low-high-latitude feedbacks and subtropical gyre dynamics and Gulf Stream variability. Hence, I suggest to reduce the reference to LBB but focus on the broader context.

Author′s response: The rewritten abstract focuses on millennial-scale teleconnections between high and low latitudes during the last interglacial. In addition, the strong freshening in the high latitudes during early MIS 5e is described as the main reason for the warm-cold switches, observed across various oceanic basins.

Reviewer′s comment: Introduction: line 40 and elsewhere: I would avoid abbreviating North Atlantic as "N. Atlantic"

Author′s response: Done.

Reviewer′s comment: l. 50: ". . .. we attempt to close this gap. . ." reflects the problem of the introduction - this is far too general. Data generation per se is important, but should be done with some hypothesis/question to be tackled in mind. At the moment I also miss a more specific lay out of the controversies that are mentioned. This would help to formulate specific questions and hypotheses at the end of the introduction.

Author′s response: The introduction has been rewritten (see the comment above).

Reviewer′s comment: Regional setting: l. 64: capitalize "intertropical convergence zone"

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

Reviewer′s comment: l. 71: replace "tropical pool waters" with "tropical warm pool"

Author′s response: Replaced with "the Atlantic Pool Water" (ls. 117, 234).

Reviewer′s comment: l. 73: "thermocline layer" is too unspecific. Does this refer to the permanent thermocline?

Author′s response: Changed to the permanent thermocline (l. 237).

Reviewer′s comment: Methods: There should be a short statement in the introduction about the type of proxies used. In the present state, the purpose of the different proxies is unclear until the discussion. However, I would expect to read one or two sentences about the rational for XRF scanning, why δ18O of deep and shallow dwellers were used, and about the purpose of the faunal studies. Again, the mentioning of the proxies can be done in conjunction with the layout of the specific goals in the introduction (see comments above). Author′s response: The used proxies are mentioned in the rewritten introduction together with their specific goals (ls. 119-128).

Reviewer′s comment: Results: l. 163: "physical sediment properties" should be replaced by "sedimentological properties", this seems more appropriate as it refers to the grain size curve shown.

Author′s response: The text has been rephrased. Now "sedimentological records" are used (l. 389).

Reviewer′s comment: l. 174: I agree that the "significant sedimentological shift" mentioned here is no artifact as it displays in different, independent proxies. However, given its minute amplitude relative to the general fluctuations in the core it is an overstatement to call it "significant". Considering the rather diffuse discussion in Section 6.4 I would skip the reference and discussion of this feature (see also respective comment below).

Author′s response: This section has been deleted from the results as well as from the discussion.

Reviewer′s comment: l. 178: "during the major deglacial transition . . . low isotopic

gradients. . ." this statement does not fully reflect the data, as there is a steady trend to more stratification from 135-129 ka, reaching the MIS 5e level of well-stratified waters. As written in the text it sounds like the entire transition is characterized by a persistent low isotopic gradients.

Author′s response: We fully agree with the Reviewer′s comment. The statement has been changed to "During the penultimate glacial maximum <. . .> gradients <. . .> are very low, succeeded by a gradually increasing difference across the T2, ∼135-129 ka" (ls. 425-427). Reviewer′s comment: l. 181: please call out Fig. 6 after "species are observed" Author′s response: Done (l. 431).

Reviewer′s comment: l. 185: please call out Fig. 5 after "abruptly increase". Also note that the variations of G. trunca (sin) in Fig. 5e are within the 1-sigma error of their present-day abundances. Is it necessary to plot these G. trunca (sin) abundances?

Author′s response: Fig. 5 (now Fig. 4) is called out after "together with a reappearance of G. inflata" (ls. 433). For simplification and clarity, relative abundances of G. truncatulinoides (sin) as well as G. falconensis are not shown in figures any more.

Reviewer′s comment: Discussion: l. 192-211: I wonder about the necessity to discuss the Sr/Ca record. In principle this is a good proxy for aragonite, however, the authors make the convincing case that this record is biased by changes in porosity and water content. Considering that the authors present the XRD-based record of aragonite form Lantzsch et al. (2007), the discussion of Sr/Ca can be omitted without losing information.

Author′s response: The discussion of our Sr/Ca record, in particular, of the "problematic intervals" is retained, however, has been substantially shortened (ls. 440-544).

Reviewer′s comment: l. 222: Please add a reference for the subsidence rate of the LBB

Author′s response: Done (study by Carew and Mylroie (1995) is cited, l. 441)

Reviewer′s comment: l. 228: if I am correct, the sea level rise should be between 12-15 m (15 = 9 + 6 m) not 12-16 m. Please check.

Author′s response: Correct. This paragraph has been, however, deleted.

Reviewer′s comment: l. 256: "warm/cold conditions" - please specify what is meant here.

Author′s response: Has been changed to "temperature estimations during late MIS 6 so far reveal cold subsurface conditions" (l. 670).

Reviewer′s comment: l. 270-272: In principle I agree with the interpretation that G. truncatulinoides abundances strongly depend on the upper ocean stratification. However, in this respect, it is interesting that G. trunca. (dex) is still high during late MIS 5e, when δ18O is already low. Hence, vertical water column stratification is not the sole factor influencing the G. trunc. abundances.

Author′s response: We agree with the Reviewer′s comment. A paragraph, dealing with additional forcing factors controlling occurrences of G. truncatulinoides (dex) and G. inflata has been included (ls. 688-803).

Reviewer′s comment: l. 283-287: Fe appears to lag δ18O, hence, question is if dust is really the dominant factor that governs the Fe abundances if δ18O is supposed to be the prime proxy recording for wind-driven water column homogenization. Fe might also be influenced by diagenetic processes, hence, it would be worthwhile looking at Ti/Al as Ti is not influenced by diagenesis.

Author′s response: We have reconsidered our Fe data and has significantly restricted the interpretation, given the "variety of additional effects that may have influenced our Fe-record" (ls. 816-820). Ti content in the investigated sediment core appears to be very low, in addition, possibly strongly affected by seawater content (Fig. 1).

Reviewer′s comment: l. 287-291: to check if winnowing plays a role during the deglaciation elemental ratios such as Zr/Rb or Zr/Al might be used to check for high

bottom current velocities (Bahr et al., 2014).

Author′s response: Increased winnowing at the northern slope of Little Bahama Bank during glacial times (a result of an intensified wind-driven Antilles Current) was previously suggested by Chabaud et al. (2016). As we could not prove the statement by using Zr/Rb or Zr/Al data (Fig. 1), as suggested above, this part has been removed.

Reviewer′s comment: l. 332: please add a reference after "only by ∼124 ka"

Author′s response: Done (l. 1081-1082).

Reviewer′s comment: l. 355: correct for "Hofman et al." (not Hofmann)

Author′s response: Done. Changed for Hoffman et al.

Reviewer′s comment: l. 364: Notably, the ruber (w) abundances are strikingly similar to the δ18Oivf-sw record of G. ruber (w) from ODP Site 1058 (Bahr et al, 2013). This supports the view that salinity is the main driver of G. ruber (w) abundances.

Author′s response: We agree with the comment above, however, a common temporal framework is needed for better core-to-core correlation and further climatic implications (Fig. 2).

Reviewer′s comment: l. 372-381: as mentioned above, the discussed changes in Sr and aragonite content are really minute compared to the other variations observed in the proxy records. Given that the authors make only very general inferences about the paleoclimatic implications I suggest to remove this paragraph.

Author′s response: Done. The paragraph has been removed.

Reviewer′s comment: Conclusions l. 387: "in the investigated core section": this is much too local, especially for the conclusions (see also my general comments). The broader implications of this study should become clear here.

Author′s response: Conclusions have been rewritten to emphasize the broader impli-

cations of the study (particularly, the third paragraph).

Reviewer′s comment: l. 389-392: these statements regarding the local sedimentological processes on LBB are quite general and not novel considering the amount of publications dealing with this topic.

Author′s response: The statements have been removed from the conclusions.

Reviewer′s comment: l. 398: replace "depressed" by "shifted"

Author′s response: Done (l. 1191).

Reviewer′s comment: Figures: Fig. 4A-C is repetitive of Fig. 3 Fig. 4E: if Sr/Ca remains in the figure (see comments above): this record has been truncated at 0.3, please state this in the captions.

Author′s response: Fig. 4 has been deleted (XRF data and also #G. menardii are shown now in Fig. 3, plotted against age). We also note now that the Sr/Ca record has been truncated (l. 1666).

Reviewer′s comment: Fig. 7E-F: Is it necessary to show G. ruber and G. sacculifer abundances from ODP Site 1063 here?

Author′s response: Abundances of G. ruber and G. sacculifer from ODP Site 1063 have been removed.

Reviewer′s comment: Reference: Bahr, A., Jiménez-Espejo, F.J., Kolasinac, N., Grunert, P., Hernández- Molina, F.J., Röhl, U., Voelker, A.H., Escutia, C., Stow, D.A. and Hodell, D. (2014) Deciphering bottom current velocity and paleoclimate signals from contourite deposits in the Gulf of Cádiz during the last 140 kyr: An inorganic geochemical approach. Geochemistry, Geophysics, Geosystems, 15, 3145-3160.

Author′s response: References:

Bahr, A., Nürnberg, D., Karas, C. and Grützner, J.: Millennial-scale versus long-

term dynamics in the surface and subsurface of the western North Atlantic Subtropical Gyre during Marine Isotope Stage 5, Glob. Planet. Change, 111, 77-87, doi:10.1016/j.gloplacha.2013.08.013, 2013.

Carew, J. L. and Mylroie, J. E.: Quaternary tectonic stability of the Bahamian archipelago: evidence from fossil coral reefs and flank margin caves, Quat. Sci. Rev., 14, 145-153, doi:10.1016/0277-3791(94)00108-N, 1995.

Chabaud, L., Ducassou, E., Tournadour, E., Mulder, T., Reijmer, J. J. G., Conesa, G., Giraudeau, J., Hanquiez, V., Borgomano, J. and Ross, L.: Sedimentary processes determining the modern carbonate periplatform drift of Little Bahama Bank, Mar. Geol., 378, 213-229, doi:10.1016/j.margeo.2015.11.006, 2016.

Please also note the supplement to this comment:
https://www.clim-past-discuss.net/cp-2018-38/cp-2018-38-AC1-supplement.pdf

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Supplementary Figure 1 for the Author´s response

to the comments of A. Bahr

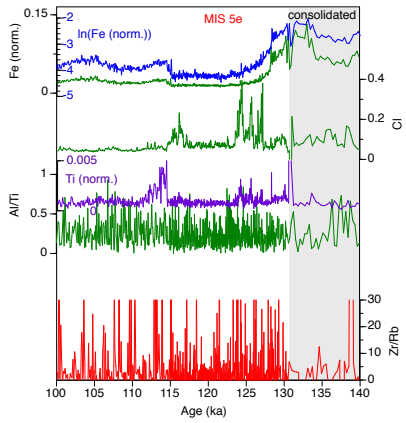

Fig. 1. XRF-scan data from core MD99-2202.

Fig. 1.

Supplementary Figure 2 for the Author´s response

to the comments of A. Bahr

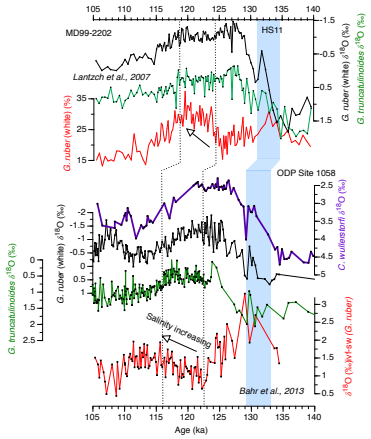

Fig. 2. Comparison of climatic records from core MD99-2202 and ODP Site 1058 (Bahr et al., 2013).

Fig. 2.