

## ***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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**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

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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 and 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.

**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.

**Introduction:** line 40 and elsewhere: I would avoid abbreviating North Atlantic as “N.

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Atlantic" 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.

Regional setting: l. 64: capitalize "intertropical convergence zone" l. 71: replace "tropical pool waters" with "tropical warm pool" l. 73: "thermocline layer" is too unspecific. Does this refer to the permanent thermocline?

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 rationale for XRF scanning, why  $\delta^{18}\text{O}$  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).

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. 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). 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. l. 181: please call out Fig. 6 after "species are observed" l. 185: please call out Fig. 5 after "abruptly increase". Also note that the variations of *G. trunca* (sin) in Fig. 5e are within

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the 1-sigma error of their present-day abundances. Is it necessary to plot these *G. trunca* (sin) abundances?

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 from Lantzsch et al. (2007), the discussion of Sr/Ca can be omitted without losing information. l. 222: Please add a reference for the subsidence rate of the LBB 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. l. 256: "warm/cold conditions" – please specify what is meant here. 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  $\delta^{18}\text{O}$  is already low. Hence, vertical water column stratification is not the sole factor influencing the *G. trunc.* abundances. l. 283-287: Fe appears to lag  $\delta^{18}\text{O}$ , hence, question is if dust is really the dominant factor that governs the Fe abundances if  $\delta^{18}\text{O}$  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. 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). l. 332: please add a reference after "only by ~124 ka" l. 355: correct for "Hofman et al." (not Hofmann) l. 364: Notably, the *ruber* (w) abundances are strikingly similar to the  $\delta^{18}\text{O}$ -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. 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.

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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. 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. l. 398: replace “depressed” by “shifted”

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

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.

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