

Interactive comment on “Detecting the onset and effects of major northern hemisphere glaciation in the abyssal tropical Atlantic Ocean” by Brent Wilson and Lee-Ann C. Hayek

Anonymous Referee #1

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Wilson & Hayek present a statistical re-analysis of benthic foraminiferal assemblage data from Ceara Rise (see lines 77-78), Atlantic Ocean, already presented in a previous study (Yasuda, 1997) published in the Proceedings of the Ocean Drilling Program, Scientific Results. That study included three sites, whilst this one focuses only on Site 926. They use a range of different statistical analyses to define the data in terms of foraminiferal change, and interpret the records in terms of changing productivity, bottom water currents, Amazon river outflow, and changes in the North Equatorial Countercurrent.

As there are no new data presented here, the choice of analyses is not justified, and the interpretations are unclear, I strongly recommend the manuscript be rejected. I

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would encourage the authors to generate new valuable data of their own, from ocean drilling cores in areas of the ocean where we do not yet have data, to increase our knowledge of deep sea environmental changes during the time of NHG.

In terms of analysis of data, I would like to see some hypothesis that is being tested, or specific question answered, followed by a rigorous appraisal of the climate proxy tools needed, and a justification of the ones ultimately chosen. I am not a statistician, but if this manuscript is a paleoclimate/oceanography study, the authors must make the case for each analysis. This needs to include a clear description of how each analysis works, as well as what the analysis is supposed to elucidate. Is this paper simply an exercise in various possible statistical methods?

Too much self-citation indicates a lack of knowledge of the wider literature. I would encourage the authors to draw more on the diverse and excellent literature out there on foraminiferal and deep sea ecology where they may find ideas to incorporate and build upon.

Detailed comments:

Line 1: I find the title a little uninformative. It would be better to include some detail about the specific findings.

Line 18: It may or may not be true that the impacts of NHG on the deep ocean are elusive, but this dataset has been published before and so we already know the changes during NHG at this location.

Lines 28-30: The authors state here that the faunal change was due to repositioning of the ITCZ, but on lines 347-350 of the Conclusions they say that it was perhaps instead due to a repositioning of the Northern Equatorial Countercurrent. On lines 48-49 they seem fairly certain that the benthic faunal changes are due to changes in the countercurrent, but on line 298 they say it might have been due to changing outflow from the Amazon. This all seems unclear.

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Lines 35-38: These sentences seem too short.

Line 37: Outline what kind of “subsequent impact” is being referred to here.

Line 59: The interpretations in this paper are qualitative; the methods are quantitative, as are the methods in Yasuda (1997).

Lines 61-67: This looks more like text in a discussion section.

Lines 69-70: Explain ‘niche widths’

Lines 70-74: The authors cite themselves here to define the ecology of the species, rather than the wider literature that includes modern biological studies.

Lines 76-77: Is it even worth stating that different species within a genus can have different ecologies? And how does this tie in with this part of the paper?

Lines 103-109: Why do the authors feel it is necessary to define if the foraminiferal data fall into a normal distribution – what does that tell us about the paleoecology?

Lines 114-115: Outline what “sequencing” means in this context.

Lines 124-129: Why are these species “here used to assess variations in relative current strength” when the authors show they are not significantly correlated with each other (lines 220-227)? I therefore don’t see the point of the paragraph on lines 321-334.

Lines 139-140 and on: These numbers have no meaning to the general reader, so need context. What is ‘high’ and what is ‘low’, and what does that mean in terms of paleoecology?

Lines 154-164: The assemblages defined by SHE analysis are not shown in any figures. Considering this proposed assemblage change at 2.6 Ma is what the whole paper hinges on, I think it is necessary to show this in a figure.

Lines 160-161: Evenness never goes this low in Fig. 2C.

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Line 162: Species richness is not shown in the figures.

Line 166: Why is it important that the data are normally distributed?

Line 170: But there also seems to be a lot of variability in dominance in the lower part of the section, which is not acknowledged here? It is essential that the SHE assemblages are shown somewhere.

Line 174: States that SHE is used to define assemblages, but does not show SHE.

Lines 227-228: The authors cite themselves – are they the only ones to show that species of the same genus can have different ecologies? What about modern biological studies?

Lines 228-232: Kaiho (1994) has been shown to be partially wrong, in that benthic foraminifera do not seem to respond primarily to benthic oxygen levels above 0.5 ml/l (e.g. Gooday & Jorissen 2012, Benthic Foraminiferal Biogeography: Controls on Global Distribution Patterns in Deep-Water Settings, *Annu. Rev. Mar. Sci.* 4: 237–62). It seems that phytodetrital flux is a bigger factor determining deep sea benthic assemblages outside of OMZs.

Lines 252 and 264: I do not think that samples separated by >50 kyr can be considered a “community”. Therefore, in this context, environmental instability cannot be inferred simply because one sample shows a different fauna from the next.

Lines 271-283: A lot of self-citation. What is the relevance here of high and low diversity within times of assemblage ‘stability’? I would like to see a much wider reading and reference of the literature.

Lines 290-292: I don’t see any point in this part of the sentence.

Lines 306-307: This has been found by other studies (e.g. Yasuda, 1997)

Figure 2: The line at 2.6 Ma seems arbitrary, which is rather strange given the highly statistical nature of this manuscript.

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Figure 3: Give species names on plots.

Interactive comment on Clim. Past Discuss., doi:10.5194/cp-2015-185, 2016.

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