

Interactive comment on “Role of the North Atlantic circulation in the mid-Pleistocene transition” by Gloria M. Martin-Garcia et al.

Gloria M. Martin-Garcia et al.

gm.martin@usal.es

Received and published: 25 July 2018

Referee #2:

The present version of the manuscript is confused and it is very hard to follow the text with these figures. Systematically, the reader has to jump from one figure to another, when it could be possible to plot the data in one single figure.

Figures have been changed as suggested: figures are appropriately called-out, and the information previously included in figures 3 and 4 has been plotted in the new Figure 3

The authors suggest a possible link of the observed changes with change in cyclicity over the Mid-Pleistocene Transition, but a detail discussion on time-series is missing.

[Printer-friendly version](#)

[Discussion paper](#)



The objective of the manuscript is not to study the variation of microfaunal assemblages through a specific time series, but only during glacials before/after the end of the MPT and the completion of the 100-ky cyclicity. Our study focuses on glacials, because the effects of the MPT are more evident during glacial stages, and the surface oceanography in the mid-latitude NAtlantlc was similar during interglacials before/after the MPT. Anyway, the text has been changed to include time-series description

Moreover, the study interval corresponds to the end of the Mid-Pleistocene Transition and without a detailed spectral and wevelt analysis on proxy records is very hard to propose in the manuscript a connection with this important, but not well understood, climate transition. In my opinion, the authors have to describe, using a statistical approach on proxy data, difference, similitude and trend between the three sites. This statistical approach could be used also to evaluate possible thermal gradients.

We have calculated the thermal gradients that were not included in the original version. Average thermal gradients for each glacial stage have also been calculated, to see if our statements are justified by the data.

The authors plotted as proxies the NAC and WARM SURFACE groups, but the connection with glacial/interglacial cycles is not clear. This is mainly evident for the NAC signal. This signal is characterised by noise and if we exclude, the increase in abundance at ca. 655ka upwards, the signal does not show a particular pattern. The pattern of WARM SURFACE shows a clear strong increase in abundance in correspondence to the onset of interglacial interval. This pattern is not strongly described in the manuscript.

Although our study focuses on glacials, the Results section has been extended to better explain the variations of species and assemblages along the study interval.

The importance of studying the NAC assemblage is the difference between its percentages at site 980 and at site U1385 in figure 3. It is clear that both percentages are similar in interglacials but very different in glacials. This clearly demonstrates the

[Printer-friendly version](#)[Discussion paper](#)

strong influence of the NAC in the high latitudes during interglacials.

There is an explanation for the strong peaks in abundance of *N. pachyderma* in coincidence of Termination VIII? This peak is in full deglaciation phase.

Yes. In this site, *Nps* is associated to deglaciations, both Terminations and other main deglacial episodes, as well as to Heinrich-type events (Martin-Garcia et al. 2015). TVIII was very prolonged, with continuous iceberg surges that deposited abundant IRD in the subpolar NATlantic (e.g., Wright and flower, 2002), and advected very cold water to site U1385, which increased *Nps*' percentage.

I would like to suggest to add in the methods a description concerning the construction of the planktonic foraminiferal groups used in the manuscript.

This has been added to the Methods section: "The microfaunal analysis focused on species and assemblages (Appendices A and B) that are associated with North Atlantic surface water masses". The components of each assemblage are included in Appendices, not in Methods because the assemblages are not original of this work, but taken from literature.

I am very surprise to see that *Globigerina falconensis* is considered as part of warm surface assemblage. This species is generally considered as cool water taxon.

We have used the assemblage defined by Vautravers et al., 2004 (see Appendix B). *G. falconensis* may be a transitional form, but it has also been identified in tropical waters, as a tropically-adapted symbiont-bearing form of *Gb* (Hemleben et al., 1989)

In my opinion the strong difference in time resolution of the sites render very difficult the comparison between the *T. quinqueloba* and *N. pachyderma*. In addition, where is the distribution of these taxa for site 607?

The distribution of *Nps* for site 607 has been added in Fig. 3. It is true that the time resolution between sites does not allow performing certain studies, like detailed statistical analysis, but the existing records allow the comparison with our data and obtain

[Printer-friendly version](#)[Discussion paper](#)

basin-wide conclusions for whole isotope stages.

In addition, the strong difference in NAC patterns from site U1385 and site 980 is not well described and in my opinion not discussed in detail.

The NAC is the dominant assemblage in site U1385 for the whole study interval. On the other hand, site 980 only registers this assemblage when the AF is northward the site. In both sites, the NAC flows from site 607, or its near region. That is the reason why sites 980 and U1385 are compared with site 607, and not between them.

Why Nps is abbreviate? Please write *N. pachyderms* left coiled – See line 192

As they are continuously mentioned in the text, *Neogloboquadrina pachyderma sinistral*, as well as *Turborotalita quinqueloba*, and the assemblages, are abbreviated for sake of making the reading easier.

Line 168 – the authors reported Fig.4c-e, But where is Figure 3?

The appropriate figure has been addressed

Line 213 – Are you sure that the correct figure is 2? I think that the figure to call up is the Fig. 3

The first version of the manuscript did not include the Nps record from site 607, which is why line 213 refers to literature respect to site 607, and to Fig. 2, respect to U1385. Nevertheless, Nps data from site 607 have been plotted in the new Fig. 3 of the reviewed manuscript and the text has been changed accordingly.

Please also note the supplement to this comment:

<https://www.clim-past-discuss.net/cp-2018-30/cp-2018-30-AC2-supplement.pdf>

Interactive comment on Clim. Past Discuss., <https://doi.org/10.5194/cp-2018-30>, 2018.

Printer-friendly version

Discussion paper

