cp-2018-30 _ RESPONSE TO REFEREES

Role of the North Atlantic circulation in the mid-Pleistocene transition

by Gloria M. Martin-Garcia, Francisco J. Sierro, José A. Flores and Fátima Abrantes

Referee #1:

The current version of the manuscript text is not written in a way that makes it easy to evaluate whether or not the data support the major findings. The Results and Discussion sections need reorganization to better highlight how the data lead to the stated conclusions. I suggest describing all time series to guide the reader through the study.

The manuscript has been changed in the way Referee #1 suggests.

The Results and Discussion sections have been modified to better explain our findings and our conclusions.

Time series are better described. The present manuscript describes events occurring during interglacials, and not only during glacials, as the first version did.

In the context of Figure 3, why not show the N. pachy counts from Site 607? Interpretations regarding heat transport are based on spatial thermal gradients, yet none of the figures show such gradients. The reader is asked to figure this out from the SST records shown in Figure 4. It is also really difficult to follow the argumentation in the discussion because statements are not followed-up with appropriate call-outs to figures.

The new Figure 3 includes the N. pachyderma sin record from Site 607 (see Fig. 3c, yellow graphic). In this way, comparison with sites 980 and U1385 is clearer.

Both latitudinal and longitudinal thermal gradients have been calculated for the North Atlantic, using data from the studied sites. The estimation of such gradients is described in the Methods section, and the gradients themselves are included in Figure 3g. To better highlight the thermal variation along the time series, the statistical mean has been calculated for each MIS, in both latitudinal and longitudinal gradients, and represented in the same figure.

Call-outs to figures have been corrected in the text.

There are a few statements in the text that seem to go against what is generally known about deep water circulation on glacial/interglacial time scales. For example, in the abstract the authors imply that NADW is strong during MIS 16 (lines 20-27)? To my knowledge, and shown in Figure 3b, the relative flux of NADW increased during the deglaciation. So perhaps this is just a matter of carefully rewording the pertaining sentences. There are numerous other instances in the text where the wording of the sentences does not clearly communicate the message (see details below).

Following suggestion, the text has been changed as follows:

"…and the increase in the North Atlantic Deep Water (NADW) formation respect to previous glacials"

Interpreting changes in percentages is complicated by the fact that an increase in one species results in an apparent decrease in another, when, in fact, there may

not be a change at all in the accumulation of the latter species. The authors should address this so-called 'closed sum' problem.
It is clear that the closed sum effect exists, but there is no better way to show the results about the planktonic foraminifer assemblages. Several authors (e.g., Bé, 1977; Ottens, 1991;) have studied present-day North Atlantic water masses and identified the dominant planktonic foraminiferal species (in percentages) for each of them. In the same way, fossil assemblages have been associated to specific water masses (e.g., Cayre et al., 1999; Vautravers et al., 2004; Salgueiro et al., 2008)

SST reconstructions are also based on assemblage's composition (measured in percentages)

Regarding the description of deep-water mass changes, I suggest rewording the sentences to make it clear that it is the relative fluxes of NADW and AABW that are changing.
The text has been changed as follows:
"…mid-latitude North Atlantic sites registered a relative decrease of the AABW during glacials, and subtropical sites recorded the presence of NADW at depths previously occupied by the AABW"

How do these results compare with Alonso-Garcia et al. (2011) specifically? The time intervals of study overlap, so there is potential to make more of this comparison. Or, are the interpretations of the shifting fronts based on their findings? In this case the study should be cited in the discussion.

Both Alonso-Garcia et al (2011), and Hernandez-Almeida et al (2013) studied site U1314, situated too north-westward for being useful in the study of variations of the NAC through glacials. This site, as well as others located northward 980 - like 984, studied by Wright and Flower (2002) together with the 980, register advances of the AF very early in glacials, both before and after the MPT. Particularly, site U1314 was compared to U1385 in Martin-Garcia et al. (2015), and SST differences between both sites, studied for the interval 780-490 ka. This study demonstrated that the NAC did not reach site U1314 since glacial inceptions, both before and after MIS16.
Site 980, on the other hand, lies in the path of the NAC and thus, at a key location to register both advances of the AF and presence of the NAC during glacials, as can be observed in Fig. 3.

Specific Comments

Lines 59-62: include Alonso-Garcia et al 2011 in the list of citations?
These lines refer to the mid-latitude NAtlantic, not to the subpolar one, which is why this citation has not been included

Line 64: Alonso-Garcia's record begins with MIS 19. Therefore, it is no entirely appropriate to cite their study in the context of something that "began" during MIS 21?
This citation has been removed

Line 66: Why abbreviate the reference to Wright and Flower (2002) withW&F02? None
of the other citations are abbreviated.
The text has been changed as suggested

Line 92: “to obtain an conclusion” seems awkward. Perhaps replace with: to reach basin-wide conclusions? Or to obtain a basin-wide picture/view/reconstruction?
The text has been changed as suggested: “reach basin-wide conclusions”

Line 96-97: Awkward sentence. Do you mean that the records extend far back into the past, or that they have been studied for a long time?
The text has been changed: “for paleoclimate and oceanographic research on the Quaternary”

Line 134: “generally present” is vague. Figure 2b shows that N. pachy are present throughout the entire study interval, but their relative abundance increases during these glacial intervals. I suggest specifying what percentages are considered significant and why. For example, there is also a peak during MIS 15.
The new Results section explains the variation of this species through the time series, comparing its relative abundances during glacials/interglacials, and also the occurrence of peak percentages

Line 149-151: This reads as if you are implying that MIS 20 is an interglacial interval.
The text has been changed: “…even more abundant than during interglacials, like in MIS20, when it reaches the highest percentages of the whole study interval”.

Line 156: I would suggest changing the section heading to specify that the focus is on MIS 20 and MIS 18
The new heading is: “5.1 North Atlantic circulation during glacials MIS20 and MIS18”

Line 169-170: The sentence needs a specific figure call-out. I found the info in Figure 4c and d.
The figure call-out has been added. The information is now in the new Fig. 3f

Line 181: Vague: What is the difference between very low and relatively low? And, it is confusing to read about low ice volume in the context of glacial intervals (MIS 20 and 18).
This sentence has been removed.

Line 192: Define what the thermal gradient is. What does it mean when it is negative in terms of the temperature difference between the sites? Once this is established, it is easier to follow the interpretation with respect to heat transport.
The method to calculate the thermal gradient is now fully explained in the “Materials and Methods” section (“2.3. Estimation of thermal gradients”). This section also explains the meaning of a positive and a negative gradient between sites.

Line 214: I am not sure that I see that the thermal gradient was significantly different during MIS 18 from MIS 16. This is true only for some intervals of time, but not consistently. For example, the same SSTs are recorded by the sites during MIS 16 at ~640-650 Ka. In any case, significance, which is a statistical term, is not demonstrated in this data set.
The ambiguous term has been changed.
The new Fig. 3g, includes thermal gradients. As the average value has been calculated separately for each stage, it is easier to see that the latitudinal thermal gradient in the NAtlantic was higher during MIS16, and MIS14, than during the whole interval MIS20-MIS18.

Line 220: It is really difficult to follow how these records show a negative thermal gradient. Would it be possible to just calculate the SST difference between the records to support this point?
Thermal gradients have been calculated between the records, and represented in figure 3g

Line 226: I am not sure I detect a repeating pattern in the data set. MIS 14 has quite a bit of variability, as you point out, so which pattern are you referring to?
The text has been modified: “While in the older glacials SST decreased towards glacial maxima, this trend is not observed during MIS16 and MIS14, and warm SST was recorded also during glacial maxima”.

Line 251: Is this correct? Do these studies really show that AABW is reduced during glacial intervals? There is a study by Lang et al., 2016 (Nature Geosciences) that shows % NADW for the past 3 million years. They show that NADW goes to zero, or almost zero during glacial intervals beginning around 0.9 Ma.
We are comparing conditions during glacials. It is proved that there is and increasing trend in the NADW formation rate since MIS22, but it is during glacials that, the difference in the AMOC rate influences the mass of water present in the deep mid-latitude North Atlantic.
The text has been changed to explain this better:
“…data from the sub-polar North Atlantic (Wright and Flower, 2002; Hodell et al., 2008) document a long-term increase in the NADW formation rate, that initiated in MIS22 and culminated in MIS14. This enhanced the southward flux of the NADW and, since MIS17, mid-latitude North Atlantic sites registered a relative decrease of the AABW during glacials, and subtropical sites recorded the presence of NADW at depths previously occupied by the AABW (e.g., Poirier and Billups, 2014; Hodell et al., 2015)”.

Technical Comments
The following is an incomplete list of editorial-type fixes.
Line 23: “At” the surface
Line 30: Blocking
Line 68: during interglacial periods
Line 69: related “to”
Line 86: “...which makes it an ideal location: : :.”
Line 99 meters: : :. At the surface: : :; at depth: : :.
Line 122: on average

Line 123: commas before and after 1 cm thick?
Line 131: associated with
Line 141: replace ‘since then’ with ‘after’
Line 215: higher

All the type fixes indicated by reviewer 1 have been taken into account.

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 appropriated 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 ciclicity over the Mid-Pleistocene Transition, but a detail discuss on time-series is missing.
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 NAtlantic 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 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 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.