

Reviewer #1:

1. Robustness of representability of compiled records. The number of compiled proxy records are smaller than the previous studies. The representability of records to discuss global/regional trends is seriously questioned because of such limited data sets with heterogeneous spatial distribution.

Previous studies by Shakun et al. (2015) and Lisiecki (2014) used a different approach to compiling their records than ours – specifically stacking versus Empirical Orthogonal Function (EOF) analysis. While stacking allows for a greater number of available data sets, it always favors the younger part of the record where more data sets are available. For example, 34 of the 49 records used by Shakun et al. (2015) cover less than half of the 800-kyr stacked record, with only 7 covering the full 800 kyr. Similarly, the vast majority of Lisiecki's (2014) 49 $\delta^{13}\text{C}$ records used in her 3-Myr stack only span <1.5 Ma, with only 11 covering the full 3 Myr (see Fig. 2 of her paper).

We used Empirical Orthogonal Function analysis to provide an objective characterization of modes of regional and global variability present within the proxy data. This analysis requires that the data used all span to full period of time (800 kyr in our case), and that they do not have any large time gaps. We further limit our data sets to use only those with a reasonable resolution (Δt) – in our case better than 5 kyr. Nevertheless, the number of records we use that completely span the last 758-kyr for SSTs ($n=15$) and $\delta^{13}\text{C}$ ($n=18$) is greater than the number of 800-kyr long SST records used by Shakun (2015) ($n=7$) and the same number of $\delta^{13}\text{C}$ records used by Lisiecki (2014) ($n=18$). We thus consider our representation of the climate signal to be as robust as, if not more so, than that of Shakun (2015) or Lisiecki (2014), while at the same time allowing us to extract robust dominant modes of variability (our PC1 and PC2) which provides greater insights into the climate changes of the last 800 kyr, as we demonstrate in a number of places throughout our paper. We note that the dominant model (PC1) is typically the global signal, and should thus compare well to the stacked records. To illustrate this, we compare below our PC1's for SSTs and $\delta^{13}\text{C}$ to Shakun's SST stack and Lisiecki's $\delta^{13}\text{C}$ stack (now included as Fig. S1 and Fig. S2, respectively), where the excellent agreement is clear.

We have added the following text to make these points.

Under section 2.2:

We note that Shakun et al. (2015) reconstructed a global SST stack for the last 800 kyr using 49 records, but only seven of these spanned the entire 800 kyr. Comparison of our SST PC1 based on 15 records to the Shakun SST stack shows excellent agreement (Fig. S1).

Under section 2.3:

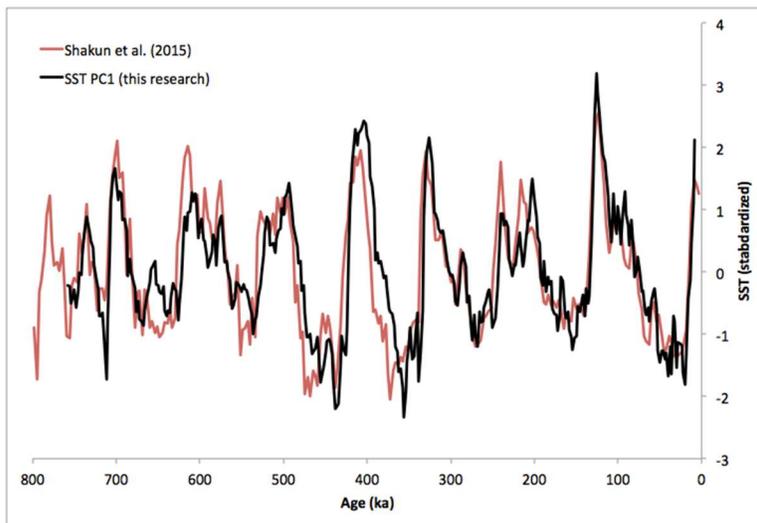
Similar to SSTs, Lisiecki (2014) reconstructed a global $\delta^{13}\text{C}$ stack for the last 3 Myr using 46 records, but only 18 of these spanned the last 800 kyr. Comparison of our $\delta^{13}\text{C}$ PC1 to the Lisiecki $\delta^{13}\text{C}$ stack shows excellent agreement (Fig. S2).

We also note that previous work by our group found that the first two modes extracted by an EOF analysis of 18 records covering the last deglaciation (Clark et al., 2002, Nature) were exactly the same as those extracted from a similar analysis of 74 records (Clark et al., 2012, PNAS), further suggesting that the results from our study are robust.

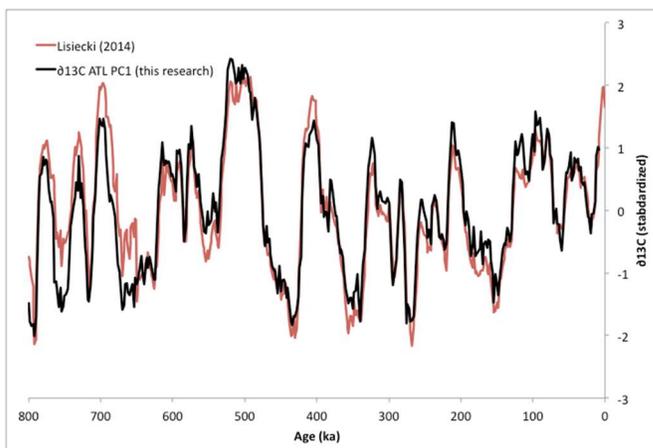
We thus conclude that our results are as robust as those of previously published compiled (stacked) records, while at the same time our use of EOF analysis has provided greater insight into the behavior of the climate system than is possible from such stacked records.

In addition, SST proxies are based on alkenone, Mg/Ca, transfer function/modern analog. These different proxies may have distinct bias because of seasonality and depth distribution in water column of proxy producers as well as proxy preservation state. Since each site is represented by one proxy, it is not clear whether the observed regional trend reflects real geographical trend or the bias related to proxy. In addition, there is no explanation about possible bias and its potential influence of extracted PC1 trend.

Although there is potential for some bias among different proxies, many compilations have found that these are typically minimal with regard to the signal that is reconstructed (e.g., Shakun et al., 2012, Nature; Shakun et al., 2015, EPSL; Marcott et al., 2013, Science; Hoffman et al., 2017, Science). Moreover, this is also where an EOF analysis provides an advantage in that it is extracting the common modes of variability.



New Fig. S1: Comparison of our PC1 of SSTs to the Shakun (2015) SST stack.



New Fig. S2: Comparison of our PCI of $\delta^{13}C$ to the Lisiecki (2015) $\delta^{13}C$ stack.

The similar difficulty exists for dust records since this variable is estimated from dust flux, the mass accumulation rate of detrital fraction or detrital element, grain size and the concentration of detrital element. Concentration of detrital element is not always representative of dust flux since the variability of sediment density and sedimentation rate are important in certain regions. Again, possible influence of mixed indicators on dust PC1 is not discussed.

We are interpreting the records as a proxy of dust variability exactly as they were interpreted in their original publications, where such issues as raised by the reviewer (variability in sediment density and sedimentation rate) have been accounted for in developing the published age models.

In addition, as discussed above, we are using EOF analysis which is only extracting the common mode of variability. In this regard, we state in the paper that “Dust records were standardized to a mean value of zero and unit variance so that each record provided equal weight to the EOF.”

The authors are careful with temporal resolution of selected records but there is no information on sedimentation rate of considered records. Bioturbation affects amplitude of variability as well as lead/lag of signals. It is not clear whether the authors applied certain criteria of sedimentation rate for their compilation.

We did not distinguish between records based on any differences in sedimentation rate. The temporal resolution we used is that determined in the original publications, where all information on development of their age models is provided.

At last, the use of $\delta^{18}O$ to obtain a common age model is not sufficiently explained. It is unclear whether only benthic foraminifera $\delta^{18}O$ values were used to tune to LR04 or planktonic $\delta^{18}O$ values were also considered. Since offset between benthic and planktonic $\delta^{18}O$ may exist, the use of planktonic $\delta^{18}O$ could add further uncertainty of the representability and timing of compiled records.

Thank you for pointing this out. All tuning to LR04 was done using associated benthic $\delta^{18}O$ records. We have clarified this in the Methods section.

Above mentioned points are examples that should be clarified to go further.

2. Original new finding of the present study in relation to climate mechanism. Since no new data are presented, the significance of this work essentially depends on new observation based on the compiled data that were not revealed by individual records and climate mechanism that can be inferred from the compilation. Unfortunately, it is difficult to identify such findings. For instance, the authors interpret $\delta^{13}C$ excursion during MIS13 is due to “a change in the carbon reservoir and not related to ocean circulation”. Then, the authors propose that stronger monsoon (thus more precipitation) during MIS13 that followed by smaller ice sheets of MIS 14 contributed to more light carbon storage on continents during MIS13.

We believe the reviewer is referring to the idea that Asian monsoons increased during MIS 15 (not MIS 13) and persisted until the onset of MIS 12.

It is curious that they do not refer the work by Hoogakker et al. (2006) that proposed an alternative mechanism. Hoogakker et al. (2006) treated the same theme by the compilation of

surface and deep-dwelling planktonic $\delta^{13}\text{C}$ and box modelling. They suggested detailed mechanism that consists of concomitant changes in the burial fluxes of organic and inorganic carbon because of ventilation changes and/or changes in the production and export ratio. Section 4.1 should be revised considering this work.

While the work by Hoogakker et al. (2006) addresses carbon isotope excursions associated with the Mid-Brunhes Transition (Late Pleistocene $\delta^{13}\text{C}$ Fluctuation in the paper), their proposed mechanism of carbon burial fluctuations pertains to the time period from 500 ka to 100 ka whereas we describe the period of time leading up to MIS 13 (i.e. prior to 500 ka). That said, the Hoogakker et al. (2006) modeled results are indeed relevant to our research and worth of discussion. As such, we have added sentences regarding their conclusions as they relate to circulation changes across the MBT, as described below.

Also, the two result sections (“ $\delta^{13}\text{C}$ ” and “ $\delta^{13}\text{C}$ gradient”) should be revised because they are difficult to follow (see my specific/ minor comments below).

Changes were made and outlined in the Specific/Minor comments section.

About ocean circulation changes in the Atlantic basin, there is some confusion. The authors interpret that the larger north-south latitudinal gradient of $\delta^{13}\text{C}$ during pre-MBT is as a sign of greater northward penetration of AABW thus less contribution of NADW compared to post-MBT. This interpretation is odd because the North Atlantic $\delta^{13}\text{C}$ record does not show significant change through MBT (Figure 12a). It is more reasonable to assume that the latitudinal gradient is caused by changes in water properties in the south Atlantic (Figure 12b and 12c). Indeed, reconstructed seawater Nd isotopic composition from a core in the equatorial Atlantic suggests a similar proportion of NADW during the interglacials of pre-MBT and post-MBT (Howe and Piotrowski, 2017). Therefore the authors’ statement is inconsistent with that of Howe and Piotrowski (2017) that is cited in the present manuscript.

We note that the proposed changes in circulation outlined in our manuscript relate to the reduced presence of AABW north of the equator after the MBT as suggested by the contour plots of interglacial $\delta^{13}\text{C}$ in Figure 15. As the reviewer acknowledges, there is no noticeable change in NADW values north of the equator, thus the changes must either be in AABW or isotopic values of AABW. However, the modeling results of Hoogakker et al. (2006) suggest a long-term depletion of $\delta^{13}\text{C}$ over this interval, thus the opposite trend of isotopic values necessary to explain the observed enrichment. As such, it is less likely to be changes in isotopic composition of AABW and therefore more likely to be changes in circulation as proposed in our manuscript.

We have added the following sentences to the manuscript to make this point:

“An alternative explanation for the observed decrease in latitudinal gradient could be changes in the isotopic composition of AABW across this time period. However, modeling results of long-term carbon fluctuations across this interval suggest that changes in the burial rate of organic and inorganic carbon caused the $\delta^{13}\text{C}$ depletion – the opposite signal necessary to create the increased similarity between northern- and southern-sourced waters (Hoogakker et al., 2006). Thus, it is more likely explained by changes in AABW influence north of the equator.”

Minor or specific comments

Line 11. Delete “benthic oxygen isotope records” and go directly “sea level” like Chalk et al. (2017). This is because benthic $\delta^{18}\text{O}$ records contain bottom water temperature and other

component not related to sea-level changes (Elderfield et al., 2012; Rohling et al., 2014).

We left the line as “benthic oxygen isotope records” as it is more explicit in what was analyzed, but added “higher sea levels and warmer temperatures” to acknowledge the controls of both temperature and ice volumes on $\delta^{18}O$.

Lines 17-18. Which physical mechanisms could create “the onset of high-amplitude variability in sea level at ~430 ka that was preceded by changes in ice sheets during MIS 14 and 13”? This sentence is unclear.

The scope of this research was to characterize the global climate system across the Mid-Bruhnes Transition. While we do not propose a particular physical mechanism that induced the changes in glacial-interglacial cycle amplitude, our results provide a fuller picture of the climate variability which we hope will assist further research into determining the physical mechanism.

Lines 90-95 and Figure 3. I am not convinced by the necessity to show the results of Blackman-Tukey power spectral analysis because the results of wavelet analysis are presented in Figure 5.

We have removed Figure 3 from the manuscript. However, we feel the description of power spectral analysis adds necessary support to the characterization of each record and our conclusions, and have moved the figure to Supplementary Information.

Lines 171-174. “Factor: : : spectral power”. This part is unclear.

“Factor scores” were changed to “factor loadings” to be consistent with the previous discussions of these statistical results.

Lines 176-177. It is unclear why “ $\delta^{13}C_{Atl}$ PC2 is a record of changes in the isotopic values of the North Atlantic carbon reservoir rather than circulation changes”. The result section contains interpretation that is not sufficiently explained.

*We added the sentence below to connect the logic between the results of the factor loadings and the interpretation of PC2 representing carbon reservoir changes of the North Atlantic:
“As such, these results suggest that PC2 exhibits the dominant mode of variability recorded in the benthic $\delta^{13}C$ of North Atlantic waters shallower than 2000 m depth.”*

Lines 191-194. In relation to the previous point, it is unclear why the residual time series (deep north Atlantic $\delta^{13}C$ – intermediate north Atlantic $\delta^{13}C$) reflects only the relative influences of AABW and NADW in the north Atlantic. Consequently, the meaning of Figure 10 is not obvious.

*The paragraph is modified to more explicitly describe the hypothesis:
“As discussed, the intermediate North Atlantic (INA) signal is predominantly controlled by changes in the carbon reservoir over orbital time scales. In contrast, the deep North Atlantic (DNA) is controlled by changes in the relative influence of isotopically more positive NADW and isotopically more negative AABW, as well as any $\delta^{13}C$ changes to reservoir that feeds the deep basin from shallower and surficial waters (i.e., INA). Subtracting the INA from the DNA record (i.e. depth gradient) removes the influence of reservoir changes, with the residual time series reflecting only the relative influences of AABW and NADW on the isotopic values of carbon in the deep North Atlantic”*

Additionally, the sentence below was added to emphasize the significance of the results in interpreting circulation in the Atlantic basin:

“The isotopic similarity between the two records suggest adequate removal of reservoir influences with the North Atlantic depth gradient thus reflecting changes in dominant water mass influence (i.e. circulation).”

Line 216. “These proxies” are unclear.

Changed to “The same climate variables mentioned above...”

Line 264. Add reference(s) after “through a glacial cycle”.

Harden et al., 1992, Dynamics of soil carbon during deglaciation of the Laurentide Ice Sheet: *Science*, v. 258, p. 1921-1924.