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Interactive comment on "Climate evolution across the Mid-Brunhes Transition" *by* Aaron M. Barth et al.

Anonymous Referee #1

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General comments

This work proposes to study the variability of reconstructed SST (15 records), benthic d13C (26 records) and dust records (9 records) though Mid-Brunhes Transition (MBT) using a statistical characterization. It is based on the compilation of already published data and reconsideration of age-scale to use the common LR04 d18O age. No new proxy data are provided. The main finding is that the MBT was a global event but the changes were not synchronous. The authors' basic idea to use statistical characterization may have a potential. However, the same research subject with more compiled data was already treated: for instance, 46 d13C records in Lisiecki (2014) and 49 paired SST-planktonic d18O records in Shakun et al. (2015). Therefore, critical points of the present work are robustness of representability of PC1 record of each proxy and

original finding about climate mechanism inferred from the PC1. I am afraid that the authors do not succeed in these points. I will develop my concerns below.

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. 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. 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. 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. At last, the use of d18O to obtain a common age model is not sufficiently explained. It is unclear whether only benthic foraminifera d18O values were used to tune to LR04 or planktonic d18O values were also considered. Since offset between benthic and planktonic d18O may exist, the use of planktonic d18O could add further uncertainty of the representability and timing of compiled records.

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 d13C 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. 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 d13C 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. Also, the two result sections ("d13C" and "d13C gradient") should be revised because they are difficult to follow (see my specific/minor comments below). About ocean circulation changes in the Atlantic basin, there is some confusion. The authors interpret that the larger north-south latitudinal gradient of d13C 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 d13C 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 .

The manuscript contains 17 figures, which is too many regarding the messages. More efforts should be paid to select information to establish a coherent story.

Taken together, this manuscript would not be published with the present form. Overhaul reorganization is necessary to improve abovementioned points. Below I will cite non-exhaustive minor or specific comments if the authors consider a resubmission of the manuscript.

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 d18O records contain bottom water temperature and other component not related to sea-level changes (Elderfield et al., 2012; Rohling et al., 2014).

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

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.

Lines 171-174. "Factor… spectral power". This part is unclear.

Lines 176-177. It is unclear why "d13CAtl 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.

Lines 191-194. In relation to the previous point, it is unclear why the residual time series (deep north Atlantic d13C – intermediate north Atlantic d13C) reflects only the relative influences of AABW and NADW in the north Atlantic. Consequently, the meaning of Figure 10 is not obvious.

Line 216. "These proxies" are unclear.

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

Figure 2. SST sites will be shown by distinguishing different proxies (with colour code, for example). Figure 8 would be deleted. Figure 9 is almost the same as Figure 3 of Lisiecki (2014).

Supplement: Table S1: The latitude and longitude of core CLP and Lake Baikal are missing. It is necessary to add the depth in water column at core location.

Reference list for supplement is missing.

References

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