Dear Referee #2

We would like to thank the reviewer for the valuable comments to improve the quality of our manuscript. These comments effectively clarified the analyses and embedded the results in a small window for misinterpretation. Please find enclosed a point-by-point reply to the reviewer' comments and suggestions.

Answer to reviewer comments in BLUE

Title

The title has been modified:

A modified seasonal cycle during MIS31 superinterglacial favors stronger **interannual** ENSO variability

Abstract

"*weaker seasonal cycle of the wind stress*"

We have removed the sentence above from the Abstract to avoid misunderstanding.

Introduction

General comment

Similar comment has been raised by the reviewer#1. We believe that the new Introduction covers and explores more properly the issues investigated in the Manuscript. It also points out the need for understanding in more details relevant climate mechanisms that have been overlooked in previous publications.

line 4, pg. 2: "*... temperatures that were* ***several*** *degrees...*": a number would be helpful.

This has been modified:

This interval was characterized by boreal summer temperatures that were several degrees greater than modern climate (up to 6C), with a substantial recession of the Northern Hemisphere (NH) sea ice \citep{melles,justino2017}.

line 11, pg. 2: "*distubances*" → disturbances

Modified

line 21, pg. 2: "*Yin etal. (2014) indicates*" → indicate

Modified

lines 23-24, pg. 2: “... *warmer* ***conditions*** *during the MIS13, ...,* ***amplifies ...*** *and* ***contributes***” → amplify, contribute

Modified

line 26, pg. 2: “... *sea surface* ***temperatures*** *(SSTs)...* ***contributes***” → contribute

Modified

line 30, pg. 2: “**(Sun etal., 2010b) based** on ..., demonstrated...” → Sun etal. (2010b)

This paragraph has been modified as shown below:

Based on paleo-reconstruction of wind and precipitation on the Chinese Loess Plateau, \citep{sun2010seven} demonstrated that monsoonal fluctuations at orbital-to-millennial scales is dynamically linked to changes in solar insolation, and internal boundary conditions. Therefore, it can be assumed that changes in insolation or increased temperatures as occurred during interglacial stages may trigger a distinct pattern of global monsoon, likewise can be expected in the future \citep{hsu2102}.

line 7, pg. 2: “*The effect* *of ocean dynamics also modify...*” → modifies **line 13, pg. 3:** “distict” → distinct

This paragraph has been removed to avoid misunderstanding.

2. Coupled Climate Simulations

line 28, pg. 3: It would be nice if the authors mention here what are the source of the “*present-day boundary conditions*”.

line 29, pg. 3: Missing brackets ‘)’. Also, it isn’t clear to me the link with “*Fig. 1 of supplementary material by Justino etal. (2017)*”. This figure shows the MIS31 WAIS topography and the differences of incoming solar radiation between CRT and MIS31 simulations. Are these the only two differences between the CRT and MIS31 runs? I recommend the authors make it clearer all differences between both experiments. I think it is a bit boring to the reader search for a key information in another manuscript, but this is only my personal opinion and I leave to the authors to decide whether to incorporate a relevant figure to this manuscript as well.

lines 29-31, pg. 3: The experiments were run to 2000 (1000) years to equilibrium and the analyses were based on the last 500 years. What are the total time spans for each run: 2500 and 1500 years?

We have included in the revised MS the paragraph below:

Two simulations are evaluated: a modern climate driven by present-day boundary conditions (CTR) and a second experiment for the MIS31 forcing. The CTR simulation was run to equilibrium for 2000 years, and our modern climate is the time average of the last 500 years of the CTR simulation. The CTR is run under present day orbital forcing and CO$_2$ concentration of 325 ppm as it characterizes emission by the year 1950. The

MIS31 run starts from equilibrated CTR conditions, including modifications of the WAIS topography based on \citet{pollardnature}, and the planetary astronomical configuration of 1.072 Ma according to \citet{coletti}. It has been carried out for 1000 years and the analyses take into account the last 500 years of the simulation.

The implementation of MIS31 Antarctic topography differs from the CTR counterpart primary by the absence of the WAIS, which according to \citet{pollardnature}, was induced by changes in ocean melt via the effect on ice-shelf buttressing that coincides with strong boreal summer insolation anomalies. In all experiments, the CO$_2$ concentration was set to 325 ppm which is based on boron isotopes in planktonic foraminifera shells for the MIS31 interval \citep{Honisch}.

lines 5-6, pg. 4: "**...** *but a brief discussion of the* **...** ***are*** *provided below*" → is provided

Modified

line 7, pg. 4: Define HadCRUT4

We have noted that our discussion in the previous version about observed SST was based on NOAA Extended Reconstructed SST V3b instead of HadCRUT4. In the current revised version, however, we have removed all discussion involving the NOAA SST data, but kept the comparison of the CTR run with ERA-I.

lines 7-15, pg. 4; Table 1: The comparison among averages is much more meaningful if followed by the respective standard deviations. The values can be similar (as the authors argue for CRT and ERAI), but global and hemispheric averages can hide important regional differences. I think global maps of mean and std temperature for each of the products (HadCRUT4, ERA-I, CRT, and MIS31) would provide a much more complete assessment on the differences/similarities among them. The authors could add such a figure to the supplementary material.

The suggested figure is shown at the supplementary material and shown below.

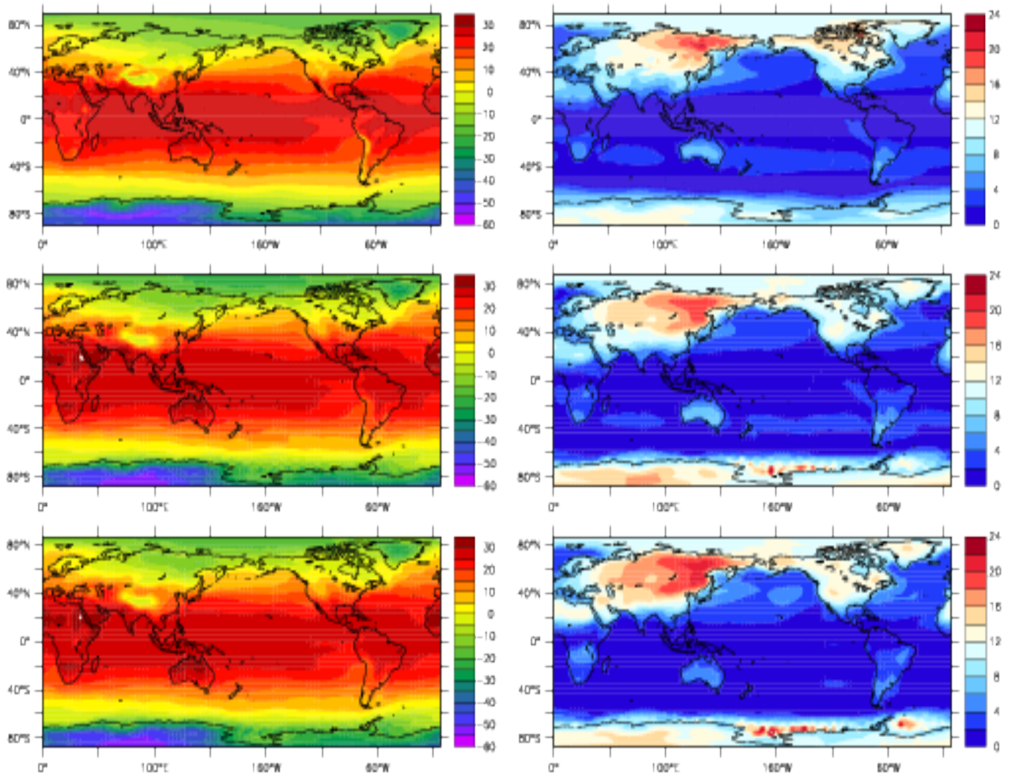


Fig. 1 Supp. Material. Time averaged surface temperature for ERAI (top right), the CTR (middle right) and the MIS31 simulation (bottom right). Top, middle and bottom left are the standard deviation delivered by the datasets.

As discussed in the MS, the ICTP-CGCM is able to reproduce the main features of global temperatures insofar as time averaged is concerned. The ICTP-CGCM performs fairly in reproducing the monthly variability of temperatures as shown by the standard deviation (STD). It is demonstrated that higher values are over Asia and North America primary related to the high seasonality associated with the landmass. Larger values are also observed over oceanic regions along storms preferential track. However, due to the model resolution, limitation is noted over steep topographies such as Tibet plateau, Andes and Rocky mountain.

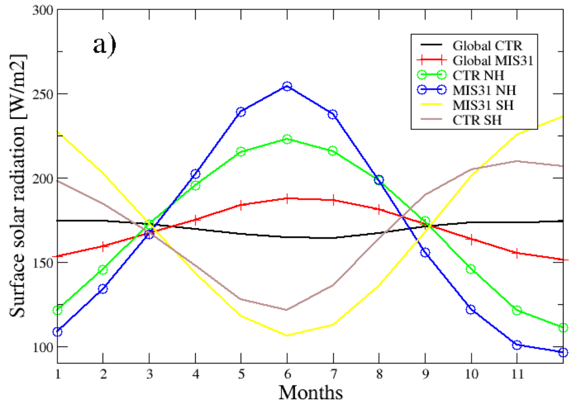
These considerations have been included in the revised MS.

Also, I was puzzled by the amplitude of the seasonal cycle in the Southern Hemisphere. See the table below, which is based on the values from manuscript's Table 1.

If we (simply) assume that the amplitude of the seasonal cycle is the difference between summer and winter averages, the Southern Hemisphere shows larger amplitude during CRT conditions (5.2 C) compared to MIS31 conditions (3.8 C). Could the authors comment on that and explain why such a difference happens? Is the enhanced amplitude of the MIS31 seasonal cycle expected only in the Northern Hemisphere? Also, being the MIS31 a super-interglacial, is there a reason to explain why the summer temperatures are higher during CRT conditions (17.4 C) compared to the MIS31 conditions (16.4 C) in the Southern Hemisphere?

This is a very interesting point raised by the reviewer and certainly needs clarification. Figure 2 (Supp. Material) shows the monthly averaged hemispheric pattern for surface solar radiation (SSR) and surface temperatures delivered by the MIS31 and CTR simulations. This figure demonstrates an inter-hemispheric seesaw emphasizing the substantial increase in the boreal SSR during the summer season in the MIS31 experiment, and similar situation occurs in the Southern Hemisphere during DJF in the extra-tropics. It has to be argue that the reason for larger seasonality in the SH is related to the excess of SSR in DJF but deficit in JJA as compared to the NH (Fig 2a,b Supp. Material). Thus, much warmer summer conditions and colder winter/spring in the SH increase the annual amplitude.

We have to make clear that according to Table 1 *“summer temperatures are NOT higher during CTR conditions (17.4 C) compared to the MIS31 conditions (16.4 C) in the Southern Hemisphere”* . In fact, the summer values in Table 1 are in brackets: in the NH (SH) the MIS31 is 2.2C (0.4C) warmer than CTR simulation. In the SH MIS31 is 0.4C warmer.



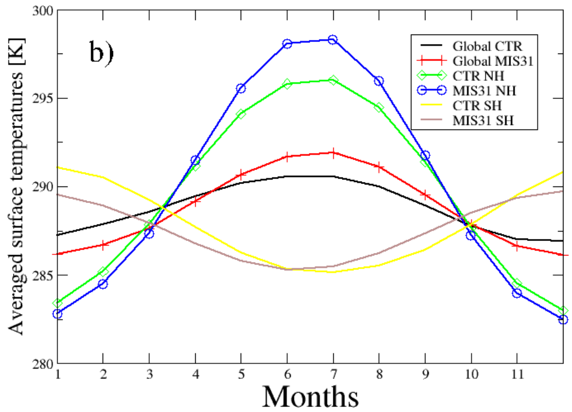


Figure 2 Supp. Material. a) Zonally averaged surface solar radiation for the MIS31 and CTR simulations. b) The same as in a) but for surface temperatures.

lines 16-23, pg. 4: Still related to the comment above, this paragraph would be more complete with the suggested global maps.

Modified

line 17, pg. 4: *"... differences between the MIS31 and CTR **simulation**... "* → simulations

Modified

lines 25 and 28, pg. 4; also in other parts of the manuscript: "(not shown)". For 5 times in the manuscript the authors use "not shown". Maybe the authors should consider to show some of the "not shown" results.

We have included in the Supp. Material additional figures useful to clarify the MS results.

line 29, pg. 4: Consider to define SLP. It may help a non-specialized reader.

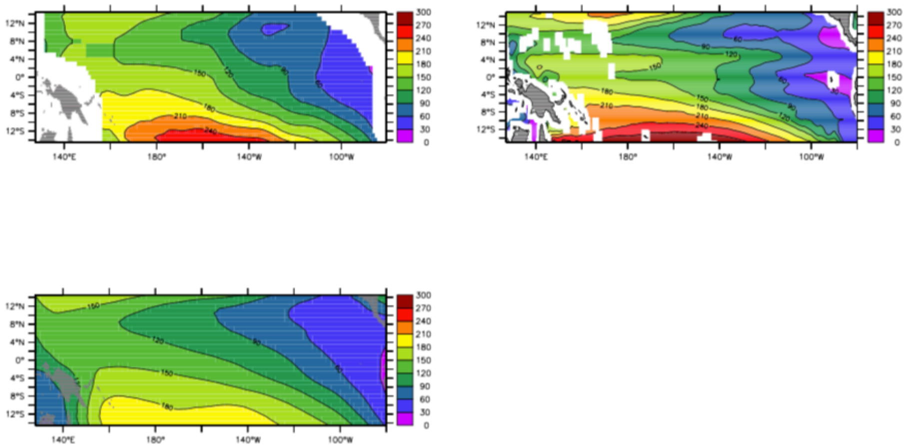
Included

lines 9-10, pg. 5: I like the analysis regarding the changes in the wind and the resulting equatorial upwelling. Maybe an improved analysis in terms of Ekman Transport and Ekman Pumping would improve further the manuscript. Also, as Fig. 1c is plotted (scales and spacing between wind vectors), sometimes is hard to compare the text against the results. The authors could consider to plot also the differences in the wind stress curl or alternatively the differences of vertical Ekman Pumping velocities – W. In my view this is an important find of the manuscript and deserves more attention.

We agree with the reviewer that plotting the vertical velocities would bring benefits to the article. However, changes in the thermocline depth (Fig. 1d) is very much related to upwelling, vertical velocity and modifications in the sub-tropical cell, therefore similar results may arise from the calculation of Ekman dynamics. Figure 1c shows the wind anomalies between MIS31 and CTR simulations.

line 13, pg. 5: Levitus etal. (2010) is fine but I would suggest a more up to date product, as for instance the World Ocean Database 2018 (WOD18).

It is shown below the thermocline depth for Levitus (top left), GLORYS reanalysis from 1993-2015 (top right) and ICTP-CGCM (bottom). Based on these plots we note that no large differences appear between the reanalyses (Levitus and GLORYS) and the ICTP-CGCM. Our CTR climate, however, shows a much shallow thermocline off the equatorial region in the SH.



lines 9-10, pg. 5: I also like the approach the authors used by applying the Sverdrup conceptual model in order to inspect the changes in the subtropical gyre. This is a straightforward and elegant way to look at the changes in the poleward transport at the continent's western boundaries. *I would just add a line to explain that even though the wind grid-resolution is coarser than the horizontal scale of the western boundary current (ie, the Kuroshio Current), the Tx used in the calculation is a representation of the zonal-averaged wind stress so that it is still fine for this analysis*. But, I leave it to the authors.

We have included the suggested statement.

3. Harmonic analysis of MIS31 and CTR climates

General comment: As already mentioned by the other reviewer, I also missed a better explanation on why the authors are using the proposed methodology. I reinforce that this should be a major point to be addressed.

We have included the following paragraph to describe in more details the choice for using harmonic analyses.

The use of harmonic analysis allows the identification of dominant climate signals in the space–time domain, separating small and high frequency processes (e.g diurnal cycle) from large-scale features (e.g. seasonal). Analyses conducted on the frequency domain can capture and differentiate the contribution of all time-scales. Thus, different climate regimes and transition regions can be characterized. The 1st harmonic shows the dominance of the annual cycle when most of the variance is represented by this harmonic. It has to be stressed that investigations based upon area averaged time series are embedded with small and large-scale processes dictated by distinct periodicity, this in turn hampers the identification of periodic climatic signals in the space–time domain \citep{justino-ijoc,cli4010003}.

line 2, pg. 6: "The first order harmonics of meteorological parameters show long-term effects...". Only meteorological? You are also applying this analysis to SST (Fig. 2c-d).

Modified

line 12, pg. 6: Define HF.

Defined

line 20, pg. 6: *"(2b-d)"*. 2b-c?

2b-c is correct. It has been modified in the MS.

line 20, pg. 6: *"displyed" → displayed*

I miss a discussion regarding Fig. 2f.

It was discussed but the Figure was not cited.

4. MIS31 – Temporal and spatial characteristics of ENSO

lines 12-13, pg. 7: Isn't clear why the authors are using the HadISST data (I guess to have an observational reference). If so, that is fair and appreciate. Please, clarify.

As mentioned previously we have removed all comments and discussion on the HadISST

lines 13-15, pg. 7: *"This is achieved by applying the MTM... fill limitations of conventional Fourier analyses"* Since the authors mentioned it, I think it is worth to specify what are these limitations.

The limitation we were referring to is because the Fourier formulation assumes that the individual coefficient represents the amplitude and phase of the corresponding frequency. We have inserted additional information of the MTM approach.

Since the statement below does not contribute to the paper results it is not included in the revised version (…*fill limitations of conventional Fourier analyses*⋯).

from line 16 pg. 7, also pgs. 8 and 9: Fig. 3 should display the significance levels (95%, for instance). It is hard to evaluate the authors analysis without these information. For instance, I can't exactly spot the significant bands of variabilities in Fig. 3. We do see the peaks, but Fig. 3c is marked by broad band and not necessarily the entire band is over the significance level.

The new Figure 3 provides the significance levels 99, 95 and 90%.

Also, further information is missed on the preparation of the time series before applying the MTM. Are the time series detrended and/or normalized?

It has been included in the revised MS.
This is achieved by applying the Multi-Taper method to detrended timeseries, 3 tapers have been used to resolve spectral fluctuations at frequencies greater than the Rayleigh frequency \citep[MTM;][]{thomson}.

line 20, pg. 7: *"... attributes"* →attributes

lines 21-22, pg. 7: *"It is interesting to note that... weakest in NINO4"***.** Do the authors have an explanation for that?

The weakening of decadal variability in the NI\~NO4 region may be related to wind variability in the off-equatorial tropics as proposed by \citet{nonaka}. This assumption has been verified by computing the correlation pattern associated with the NI\~NO indices. It turns out that the NI\~NO4 relationship with the zonal windstress within 10-30°N is considerably weaker than that of NI\~NO34 or NI\~NO3. Moreover, this weaker correlation between the NI\~NO4 and windstress is not confined to the equatorial region but extends to the extratropics.

Also, as suggested by Fig. 3a, the spectrum of NINO4 is shifted to higher frequencies compared to the other two indeces. Please, clarify.

The reason to this slightly shift to higher frequency by the NINO4 is not clear, however, because the NINO4 is located much closer to the warming pool region, which is dominated by weak seasonal cycle with the 1st harmonic explaining by about 30% of the total variance, may indicate that higher order harmonics play a role to induce some power at higher frequency. The NINO4 power spectrum in the MIS31 run does not show dominant periodicity at interannual and interdecadal timescales.

This is included in the MS.

lines 22-23, pg. 7: *"The HadISST does not show any periodicity on decadal time scales...* ***the length of the timeseries does not seem to capture this lower frequency****"*. This explanation does not sound convincing. A time series with 147 years (1870 to 2016) should be long enough to capture several cycles (147/30≈5; 147/15≈10). It seems that the multidecadal variability is not present in HadISST. Could the authors explain the potential reason for that? It may be due to the coarse data distribution (*in situ* observations) until the incorporation of satellite data (1982) to this product?

As mentioned previously we have removed discussion on HadISST.

line 24, pg. 7: *"claims"* → claim

Modified

lines 24-25, pg. 7: "*... zonal asymmetry related to the decadal variability in the HadISST observations is weaker and not as regular as for instance in the ECHO-G model*". Again, maybe this absence could be justified by the few data available (in spatial and temporal terms) used in the optimal interpolations for the pre-satellite period. This is just a speculation that the authors could confirm (or not ?) by searching in the literature. It is a bit confusing to mention ECHO-G, since this model wasn't referred before. If this info is really important, please provide further information.

Removed

line 32, pg. 7: *"This simulation shows stronger power spectrum at interannual time scales 3-7"*. As mentioned above, this statement needs to be corroborated by the confidence levels in Fig. 3.

Confidence levels have been show.

line 37 pg. 7: Define SOI**.**

It has been defined.

lines 4-5 pg. 8: *"This is in line... enhanced power also at interdecadal time scales (Fig. 3d)"***.** The control run shows spectral peak both at the interannual and multidecadal timescales (Fig. 3d). In the text the authors have discussed a potential reason of why the multidecadal peak isn't observed in the MIS31 run. Nevertheless, the spectrum also doesn't show a peak for the interannual variability. Do the authors have an answer for that? This is an important point that should be addressed.

The MIS31 climate shows dominant power spectrum for the NINO3 and NINO34 at inter-annual timescales distributed at a broader 3-12 year band, differing from the CTR that exhibits a shorter band, 6-8 years. According to Feldstein (2000) the power spectrum is defined by the interannual variance due to external forcing and the interannual variance from stochastic processes. The power spectrum which is dominated by the external forcing exhibits a sharper peak as compared to that driven by stochastic processes. It may be argued that despite the dominance of external forcing in the MIS31 climate random processes also play a significant role to define the temporal variability inducing the broader frequency band as compared to the CTR climate.

Caption of Table 1: *"1961-90"* → 1961-1990; Also, "June,July" → June, July.

Modified as suggested.