The manuscript provides a broad overview on how modified seasonal cycle, as such from superinterglacial MIS31, could potentially impacts the variability of major climate players, as for instance the ENSO and the Monsoonal systems. The manuscript is interesting and address a timely topic, perfectly suitable to *Climate of the Past,* and it sheds some light on our understanding of future climate changes. Nevertheless, I miss some robustness in the analyses what I tried to highlight in the comments below. I believe that a major revision of the manuscript is needed, but it should be feasible taking into account what the authors have presented so far.

Title:
"*A modified seasonal cycle during MIS31 superinterglacial favors* ***stronger*** *ENSO variability*"
Isn't your title in conflict with your results? The authors argue that the MIS31 conditions intensify the 3-7 year (interannual) variability, while the 15-30 year (multidecadal) variability vanishes. Since the authors are addressing both bands of variability in the manuscript, I suggest they make it clear in the title what band of variability is intensified by MIS31 conditions.

Abstract:
The authors clearly specify that the MIS31 is characterized by "*enhanced seasonality…* " (line 2). Afterwards, it is mentioned that the MIS31 is marked by a "*weaker seasonal cycle of the wind stress*". So, not all climate players have an "enhanced seasonality"(?). Maybe it is worth to add few words in order to explain what parameters (climate features) have the seasonal cycle intensified. At this stage, things are not clear in the abstract

1. Introduction:

General comment: The authors presented a convincing story to explain how the enhanced seasonal cycle from the interglacial MIS31 can be used as a proxy for understanding the potential impact of increased atmospheric CO2 in the climate players, as for instance the ENSO. Nevertheless, in my opinion, the introduction could also make it clear the main and specific scientific questions that the authors are going to address. As it is, things are a bit vague.

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

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

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

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

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

line 30, pg. 2: "**(Sun etal., 2010b) based** on …, demonstrated..." → Sun etal. (2010b), …, demonstrated… Also, this sentence sounds a bit confusing. For instance, "… *based on seven million years of wind and precipitation variability*" sounds like a variability band of 7 million years. Also, "*monsoonal* ***fluctuations****…* ***is***" → are. Please, reconsider to rephrase it.

line 7, pg. 2: "*The effect* *of ocean dynamics also modify...***"** → modifies

line 13, pg. 3: "distict" → distinct

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?

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

line 7, pg. 4: Define HadCRUT4

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.

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.

	HN, CTR	**HN, MIS31**	**HS, CTR**	**HS, MIS31**
Summer	22.4	24.6	17.4	16.4
Winter	10.6	10.2	12.2	12.6
Summer - Winter	11.8	14.4	5.2	3.8

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?

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

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

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.

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

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_E. In my view this is an important find of the manuscript and deserves more attention.

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).

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.

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.

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).

line 12, pg. 6: Define HF.

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

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

I miss a discussion regarding Fig. 2f.

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.

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.

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

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

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?

line 24, pg. 7: *"claims"* → claim

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.

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.

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

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.

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