Answer to reviewer 2

We wish to thank you for the interesting and very detailed comments. It would be our pleasure to make the modifications you addressed in detail and implement them in the new version of the manuscript. In the revised version we will try to cover all of the issues suggested by you. Here, we answer to the each comment individually. Two Tables and 6 Figures are included in this answer.

1.- Data and methods, Page 2688, line 2-... The paper uses the ECHAM5/MPIOM and the GISS-E2-R simulations as part of the PMIP3-CMIP5 experiments. While this is true for the GISS-E2-R runs, the forcing specifications of the ECHAM5/MPIOM do not comply with the PMIP3 specifications. This part should be rewritten accordingly.

Line 19-20 'The forcings in the ... ECHAM5/MPIOM simulations' This is nor strictly correct. At least the text should explain/discuss in which sense they are similar. For instance CO2 in the ECHAM5/MPIOM is calculated interactively. For other forcings it may be worth discussing the differences/similarities.

We have used the fully coupled Community Earth System Models (COS-

MOS) from Max Planck Institute for Meteorology coupled to the ocean model MPIOM. In the IPCC WGI fifth Assessment Report the model is included in Pre PMIP3/CMIP5 experiments as ECHAM5/MPIOM model. We draw your attention to a phrase from [Jungclaus et al., 2010]: *" The experiments presented here are among the first ESM simulations that* ***comply*** *with the protocols of the Paleo Modelling Intercomparison Project Phase 3 (PMIP-3, `https://pmip3.lsce.ipsl.fr`) and the upcomming Paleo carbon Model Intercomparison Project (PCMIP)."*

In the Millennium project [Jungclaus et al., 2010], an ensemble of five simulations covering the time between 800 and 2005 AD have been calculated starting from different ocean initial conditions. The model simulations have been forced by: solar variability [Solanki et al., 2004] and [Krivova et al., 2007], volcanoes [Crowley, 2008], land cover changes [Pongratz et al., 2008], orbital variations [Bretagnon, 1988](, greenhouse gases [Fortuin, 1988] and [Marland, 2008], and aerosols [Tanre, 1984]. A detailed description of the Millennium simulations is documented in the paper from [Jungclaus et al., 2010].

As already cited in the paper, the forcings for the GISS model are also described in the homepage of NASA. We summarize the forcings for GISS model in Table 1. For more information about the forcings, we refer to the Table 5.A.1 of WG1 AR5 of the IPCC, 2013. Therefore, we change the sentence "The forcings in the ... are similar" to "The forcings in the... are nearly similar".

2.- Section 3, Page 2690, line 1-8.

I like the approach of using model ensembles. However there are some issues related to this approach that I think are worth discussing

or at least being considered by the autors. The EOFs in Fig 1 are indeed similar. However the model EOFs are obtained from ensemble average fields in which the internal variability is canceled or diminished by averaging. Reality (i.e. MADA) is comparable in those terms to only one realization of the system, i. e one model simulation. If the MADA is compared to ensemble averages, the behavior of each ensemble member may be an important issue to report. Additionally, the statistics obtained may be not only related to the model behavior but also to the ensemble average itself. For instance, it would be expected that a mode explains more variance if the ensemble average is taken over a larger number of members (like in the case of ECHAM/MPIOM for which the ensemble is larger than GISS-E2-R) or that the pattern correlations are larger. I think it is worth discussing the implications of using model ensembles in this work relative to individual members and how this affects the results.

You have pointed to a very interesting issue. In a recently published paper [Polanski et al., 2014] , we have presented the PC1 time-series of precipitation and PDSI for each member of the ECHAM5/MPIOM simulations. There, we mentioned that the EOF patterns for each member is very similar to the one from MADA. However, the time expansions show a large variability. Using Ensemble Mode Decomposition (EMD) method, we extracted the nonlinear trends within the time-series (supplementary materials of [Polanski et al., 2014]). Two out of five members showed very similar and coherent trends as in the MADA.

By using a simple arithmetic averaging, we assign equal weight to each ensemble and consider the systematic errors of the model. Our analysis shows that for the ECHAM5/MPIOM ensemble, two of the members present a better performance with respect to MADA. However, the results were biased towards one of the individual members when using other methodologies than a simple averaging. This may lead to over-tuning of the model ensemble average to the reconstruction of mega-droughts. Tuning the ensemble average to PDSI from proxies is not a good reason that the model will also perform better for other variables.

On the other hand, using a single model simulation will rise the question that, to what extend the agreements with the reconstructions are happened by chance. The other issue is that the models do not use data assimilation and the timings may be not accurate. The precise timing of the droughts is also uncertain in the proxies. Here we decided to use as many realizations of the climate as possible, instead of single member, in order to cover a larger space of the possible solutions and reach a more accurate estimate of the climate.

According to [Kalnay et al., 1996], the ensemble average is more accurate than a single deterministic climate simulation. [Lambert and Boer, 2001] concluded that the mean climatological fields from ensemble average agree better with observations than the fields produced by any single member or model. It would be a hard task to identify the best performing model, as different simulations present varying performance quality over different regions and climate variables [Giorgi and Mearns, 2003, Krishnamurti et al., 1999, Krishnamurti et al., 2000, Palmer et al., 2000]. The PC1 of PDSI from individual GISS-E2-R model experiments were already presented in the answer to reviewer 1. The trends for the two members of the GISS model are very similar. The Pearson correlations between the trends is 0.61 ($p - value < 0.01$). As you mentioned, it is very likely that the larger number of members in ECHAM5/MPIOM results in a better pattern correlation with MADA. However, the GISS model performs a coherent behavior in the time expansions of the EOF patterns.

Section 3, Page 2690, line 14-21: It is also important to state how mega-droughts are defined in this paper. I have not seen the definition so far. There seems to be indeed some agreement in the time evolution of the index in Fig 2, both if we consider the Pcs or if the periods of 'active' and 'break' phases are considered. I would suggest discussing this a bit more in detail. How good is this agreement in terms of correlation, perhaps for high and low (multi-decadal) timescales. How different is the agreement of the ensemble averages relative to the individual members and how important is this for the credibility that the coincidence is not by chance and arguably related to external forcing?

Thank you for pointing this out. We have investigated the same megadroughts mentioned in the study of [Cook et al., 2010a]. The following phrase is added to describe mega-droughts. "Megadroughts are defined as prolonged period of dry conditions which last decades to centuries" [Cook et al., 2010b, Cook et al., 2010a, Coats et al., 2013]. You asked about the correlation of PC1 time-series of each member and if the averaging increases the correlation for high and low time-scales. Neither proxies nor models are presenting the "truth". We did not aim to tune or fit the ensemble average to the reconstruction space. We tried to find the probable agreements between these two spaces, namely proxies and models. We want to explore how model and reconstructions are reproducing the timing and patterns of possible mega-droughts which are historically recorded. As indicated in the previous answer, the PC1 time-series for ECHAM5/MPIOM are already presented in the paper of [Polanski et al., 2014]. Two out of five ensembles showed significant correlation with MADA for longer time-scales. However, averaging all members did not present significant correlation. On the other hand, as mentioned in the paper of [Jungclaus et al., 2010], "the ensemble simulations reproduce temperature evolutions consistent with the range of reconstructions."

But in the case of GISS model, as the two ensemble members are used for analysis, which are coherent based on the first Principle Component of PDSI, the averaging shows a higher correlation with MADA compared to ECHAM5. Note that the differences in ECHAM5 members are rising from different initial conditions of oceans and not the forcings. Here we plot the time-series for GISS-E2-R in Figure S1 with their correlations for the filtered time-series. To follow the same methodology in [Polanski et al., 2014], we take the period of 1400 to 1860 (Little Ice Age) for Pearson correlations. The numbers in parenthesis indicate the Pearson correlations with MADA.

Why do the 5 historical mega-droughts not coincide with the minima in both series, or at least in the MADA (red) line?

We indicated that the PC1 of PDSI, can capture droughts which show the dipole pattern between India and arid central Asia (the droughts which present patterns like the EOF1 of PDSI). The EOF analysis is a linear method for dimensionality reduction [Hannachi and Turner, 2013]. Therefor, it can not capture all the possible drought patterns in the data-set. It is still a mystery if all the megadroughts are caused by monsoon failure, oceans, volcano, local effects or a nonlinear mixture of all. Figure 2 is showing the evolution of the dipole pattern throughout the past millennium. This pattern is also captured in observational PDSI of recent decades, but all the megadroughts may not follow this pattern. Therefore the time-series from proxy and models must not show minima at the same time.

Discussion of Fig 3: What is the meaning of dots? Fig3 caption indicates that dots stand for grid points that agree with MADA. Can you be more specific? How can it be that some gridpoints in c are in blue (positive) while in MADA are in brown (negative) and still have a dot indicating agreement?

The dots indicate both proxy and model show similar sign in PDSI. We have checked Figure 3.c and the code again and again but we found no dot that is placed on a grid with no agreement! For making it more clearer we plot Figure 3.c again in this answer as Figure S2. Plus values are in green and minus in brown.

Fig 3c indicates a different pattern to that of MADA (Fig 3a) and GISS (Fig 3b). This is discussed in the text. How can it be that in Fig 2 MADA and ECHAM5 seem to be in phase while GISS is not? Similarly for the following Fig4-Fig7 panels. The comments on the reconstructed droughts and their importance are welcome, although I would suggest discussing their consistency with Fig 2 in terms of the PC and the 'active' and 'break' phases.

As discussed in the previous comment, the time-series in figure 2 will mostly capture droughts with patterns like figure 1. Figures 3-7 are showing the composites of PDSI over drought periods mentioned by [Cook et al., 2010a] which originate from historical records. Figure 3.c presents a broad dry pattern over central Asia and wet pattern over East India which has similarities with Figure 1.b. PC1 time-series of ECHAM5 are negative but very near to zero and agreements with MADA are mostly due to wet spells. Figure 4 for example is showing the clear dipole pattern between India and Central Asia in MADA, GISS and ECHAM5. The time-series of all data-sets (Figure 2) also indicate a clear drop within this period. We will revise the text to make the interpretations clearer.

MCA analysis, Figs. 8, 9 Section 4.1, Page 2692 I think this discussion is also interesting, but it should also be improved at various levels. For instance, the correlations of the PDSI series are reported to be significant. The relation with GISS is indeed suggestive. It would be good to indicate significance in table 1 as an alternative to the largest value in each row. Are correlations calculated over annual values or low pass filtered values?. This should be indicated, also if autocorrelation has been taken into account and, if not, I suggest it should be.

This point was also asked by reviewer 1, we have added the significance level. The correlation of GISS PDSI with Temperature is also corrected (0.52 instead of 0.19). In the section Data and methods we indicated that the time-series are smoothed using a 31-yr filter. We will add the sentence to the caption of the figures. Autocorrelation has also been taken into account.

Additionally, after reading Section 3 where the synchronicity between Asian drought in reconstructions and simulations is discussed

(Fig 2), I think it may be interesting to include in this analysis the reconstructed pdsi in order to link it to changes in simulated SST. This is based on the argument in the previous section that reconstructed and simulated PDSI are related.

We tried to make the most direct comparison in the model space since the modeled PDSI is responding to SSTs from the model. Model and proxy have agreements in longer time-scales (smoothed time-series) and MCA captures the maximum variances within the coupled data-set. The MCA part is inspired from the previous study done by [Dai, 2013] who investigated the coupled patterns from CMIP5 simulations for the present and the future.

I think that another issue that is relevant for this work is to, once again, illustrate or report on the behavior of individual simulations. How does this influence on the reported explained variances (lines 9-10)?. Also, the time series in figures 8 and 9 suggest an influence of external forcing (volcanic) in the mid 15th and early 19th century. I recommend this should be discussed and would likely be more evident in the ensemble average than in the individual simulations.

Thank you for the advice. We report the behavior of individual simulations in Table 2. We have used the motivation you gave to plot the figures again with superimposed volcanic forcing. Figure S3 and S4 are showing the MCA analysis for ECHAM5 and GISS model with volcanic forcing (W/m2) from [Mann et al., 1999] superimposed on the right axis. As you already suggested, the volcanic forcing has an influential effect on the coupled atmosphere-ocean patterns for both ensemble average of the models. The Square Covariance Fraction (SCF) has not changed largely for ensemble average but the correlations show an interesting improvement especially for GISS model.

Regimes, Section 4.2

Page 2693: regarding the different explained variances in both models, this is noteworthy and deserves some more comments, for instance in relation to the variance explained by the individual ensemble members and the effect that averaging over a different ensemble size may have on the result.

The averaging has small influence on the explained variances in the EOF analysis of ensemble means.

Concerning the analysis performed later with ECHAM5/MPIOM and GISS-E2-R I can think of several issues that I would like the authors to discuss about or consider:

The discussion about the distribution of the two regimes in Fig 10 and Fig 13 is interesting. In Section 4.2.1 the authors describe the spatial variability of both regimes. I think the figure and discussion would gain from showing the precipitation patters associated to this regime, perhaps also the PDSI from the model. Also the distribution of associated PDSI events to each regime can perhaps be shown if the authors consider it of use. It makes more sense to me to trace the actual behavior of these variables from the available model runs, that in fact show differences between them, than rather argue only from the literature based on different analysis by other authors.

That is a great suggestion! The selection of OLR, as a proxy for convection, had two reasons: 1- it is a very good indicator for monsoon activity as the monsoon is mostly caused by convective rainfall. 2- previous studies already found an existence of bimodality and regime behavor in OLR from observations and reanalysis data (ref. [Turner and Hannachi, 2010]). Here we are searching for potential dynamical drivers of moisture changes in the model space. 850 hPa wind is also selected as an indicator of moisture transport into the monsoon region.

As suggested by you, we show the composite patterns of PDSI for each regime in Figure S.5 and S.6. The PDSI regimes in ECHAM5 are more different resembling the active and passive monsoon phases. This was predictable as the bimodality in the PDF estimates of OLR is more clearer in the ECHAM5 than GISS.

Regarding the use of pdfs of the regimes for the individual simulations and for the ensemble averages: I have reservations here about the use of these pdfs for the ensemble averages. First I would suggest to argue and discuss the changes from the individual ensemble members to the ensemble average.

As you suggested a discussion will be added to the final version about the PDFs of ensemble average and individual members.

Second, I would like the authors to discuss the meaning of the regimes obtained from the ensemble average. The model is expected to produce very different states for a given time step in different simulations due to internal variability. What is in this case the meaning of the regime state for a given time step of the ensemble average?. I think discussing this a bit more and why the probability of regime 2 diminishes is pertinent.

When using a single member to calculate the histograms, the distance between the two peaks are larger as can be seen in figure 10 and 13. By mixing the simulations, the product would be a better estimate of the mean state, thus the PDF tends to a normal distribution with a slight shoulder over the regime in positive values. In ECHAM5 simulations the regime 1 (left) is more frequent and therefore the average PDF shows a peak near this center. For GISS, as a result of less numbers of members, the ensemble average presents a peak near zero. This could be clearly seen in regime 1 of PDSI with near neutral drought conditions (PDSI = 0) for most of the region (Figure S.6.a).

The patterns in Figures 13 and 15 are vaguely described and not discussed. I suggest the authors argue about the differences and similarities in the results obtained from both models. Are these the correct events (5 historical megadroughts) to consider? (recall the low agreement of the minima in Figs 2,8,9 with model series).

Recalling figure 2 of the manuscript, both models capture at least 4 out of 5 mega-droughts in terms of sign of the PC1 of PDSI (brown shadings). Knowing that the PC1 of PDSI presents monsoon failure, the composites of OLR and 850 hPa wind should present patterns similar to break regime of monsoon. By averaging for such a long period (84 years), the systematic model error is reduced and the patterns show similarities with the break phase. The 850 hPa wind pattern of GISS for example, presents a clear reduction of moisture transport into India especially for the Somali Jet.

I think conclusions and abstract should be rethought in view of the previous points. I would advise including in the conclusions some cross section discussion about the results obtained in each section and how they complement each other. Also, how the agree-

ment/disagreement between reconstructed and simulated drought is affected by model issues (e.g. ensemble averages vs ensemble members and the benefits of using two ensembles) and how it may be traced to external forcing in different models. The different/similar results obtained by the two models should be discussed.

We agree and try to implement this suggestion to make the conclusion coherent by adding cross section discussions.

Regarding the minor comments, we will implement all suggestions in the final version of manuscript.

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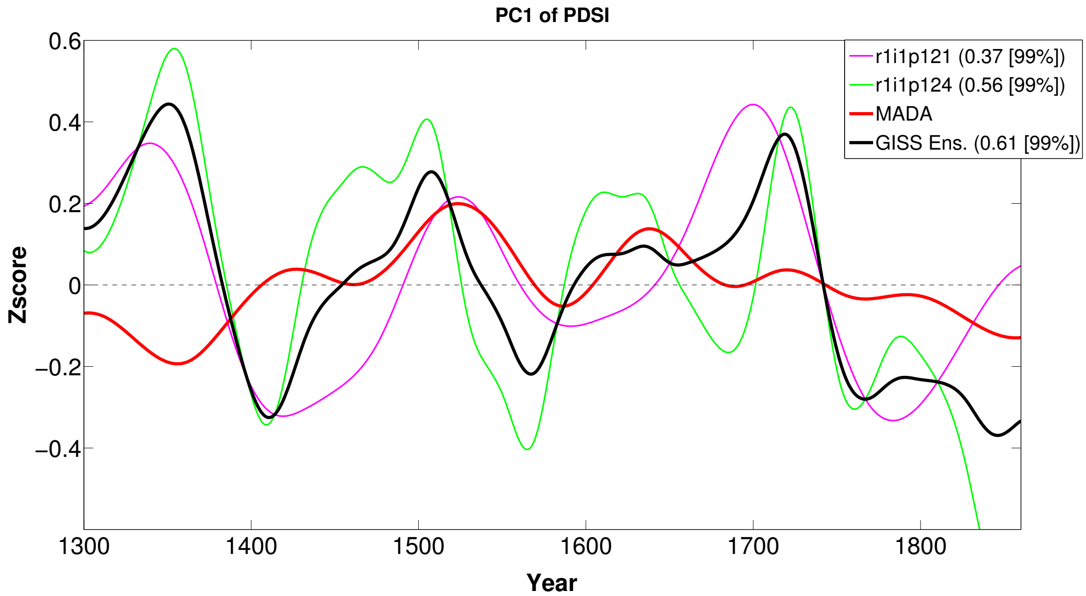

Figure 1: (S.1) Smoothed PC1 trends of PDSI for GISS and MADA. The numbers in parenthesis are correlation coefficients. Black solid line is the ensemble average.

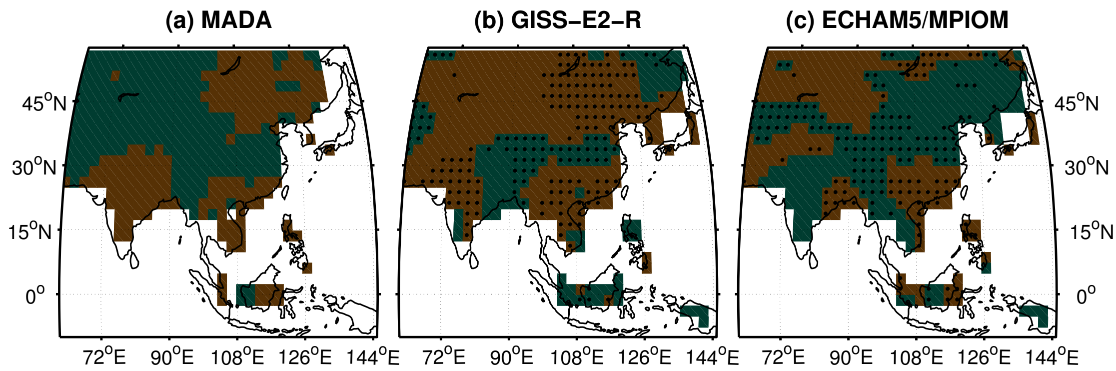

Figure 2: (S.2) Khmer Emipre megadrought. **Note** that in MADA some grids are missing in the north-east of the domain.

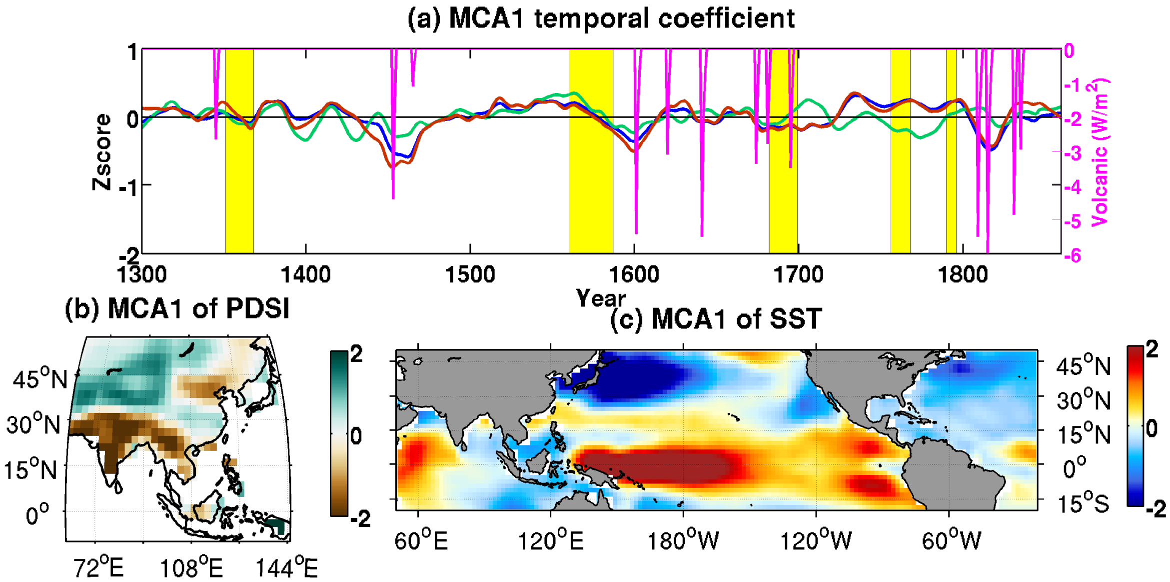

Figure 3: (S.3) MCA for ECHAM5/MPIOM.

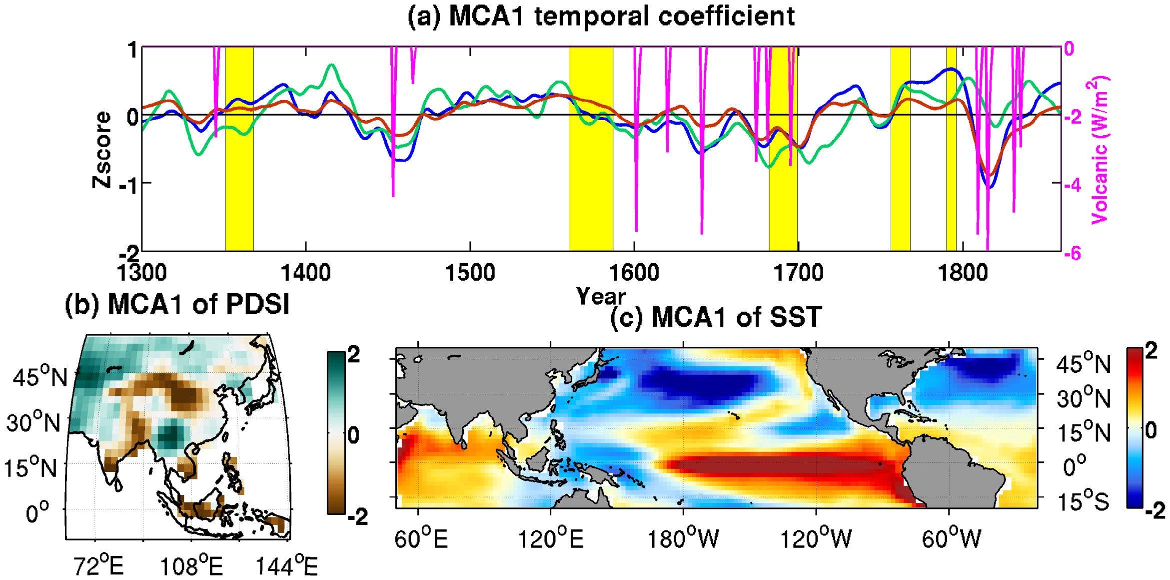

Figure 4: (S.4) MCA for GISS-E2-R.

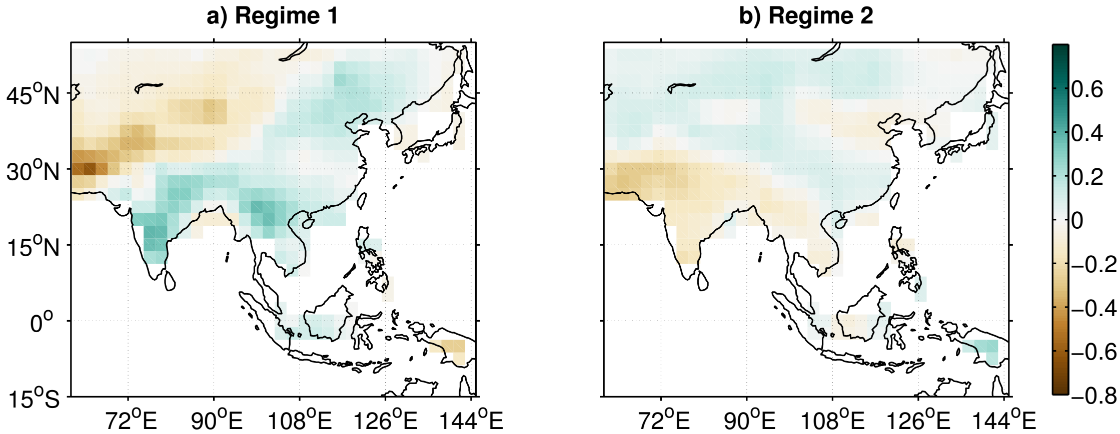

Figure 5: (S.5) PDSI composites for the two regimes from ECHAM5/MPIOM.

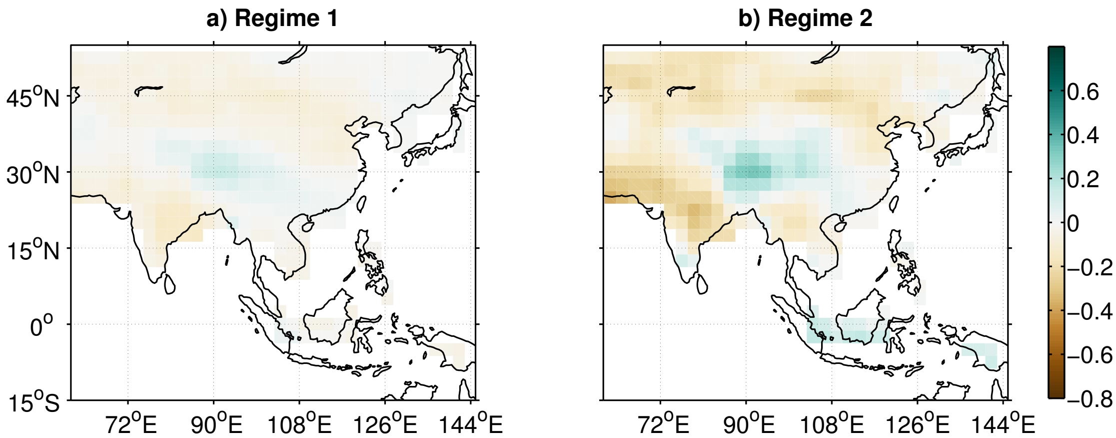

Figure 6: (S.6) PDSI composites for the two regimes from GISS-E2-R.

1	Solar: SBF, Volcanic: CEA, LULC: PEA, GHG transient, orbital
2	Solar: SBF, Volcanic: GRA, LULC: PEA, GHG transient, orbital
3	Solar: SBF, Volcanic: None, LULC: PEA, GHG transient, orbital
4	Solar: VK, Volcanic: CEA, LULC: PEA, GHG transient, orbital
5	Solar: VK, Volcanic: GRA, LULC: KK10, GHG transient, orbital
6	Solar: VK, Volcanic: None, LULC: PEA, GHG transient, orbital
7	Solar: VK, Volcanic: CEA, LULC: KK10, GHG transient, orbital
8	Solar: VK, Volcanic: GRA, LULC: PEA, GHG transient, orbital

Table 1: Forcings used for GISS-E2-R simulations from `http://data.giss.nasa.gov/modelE/ar5/`. SBF = [Steinhilber et al., 2009]; VK = [Vieira and Solanki, 2010]; CEA= [Gao et al., 2013]; GRA = [Crowley, 2008]; PEA = [Pongratz et al., 2008]; KK10 = [Kaplan et al., 2010].

MCA1 of	Niño 3.4	Niño $1+2$	Niño 4	NHT
SST of mil0010	0.68	0.39	0.71	0.49
PDSI of mil0010	0.24	NS	0.22	0.14
SST of mil0012	0.63	0.39	0.63	0.33
PDSI of mil0012	0.21	NS	0.20	NS
SST of mil0013	0.70	0.54	0.69	0.43
PDSI of mil0013	0.19	NS	0.25	NS
SST of mil0014	0.28	0.25	0.30	0.15
PDSI of mil0014	NS	0.13	NS	NS
SST of mil0015	0.61	0.57	0.64	0.41
PDSI of mil0015	NS	0.14	0.13	-0.21
SST of r1i1p121	0.49	0.51	0.55	0.25
PDSI of r1i1p121	0.28	0.35	0.33	NS
SST of r1i1p124	0.45	0.41	0.53	0.22
PDSI of r1i1p124	0.36	0.38	0.34	0.12

Table 2: Correlations of MCA timeseries and climate indices. All the coefficients are significant with $p - value < 0.01$. NS stands for Not Significant.