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Interactive comment on "The South American Monsoon Variability over the Last Millennium in CMIP5/PMIP3 simulations" *by* M. Rojas et al.

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Dear Editor,

Concerning the revision of "The South American Monsoon Variability over the Last Millennium in CMIP5/PMIP3 simulations" by M. Rojas, P. A. Arias, V. Flores-Aqueveque, A. Seth, and M. Vuille. Please find below our responses the two anonymous reviewers of our manuscript. First of all, we are grateful for the reviewer's insightful comments and the time and effort they spent reviewing our manuscript. We feel that their comments have helped us to significantly improve our paper.

Reviewer 1:

Summary The study analyses the South American Monsoon System (SAMS) variabil-

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ity in the PMIP3 simulations spanning the period from 850 to 1850 AD. The models' ability is assessed by comparing the results to proxy data. The study focuses on the difference between the Medieval Climate Anomaly (MCA) and the Little Ice Age (LIA). The authors argue that the simulations show a stronger Monsoon during the LIA, resembling proxy data. Still, simulated precipitation in the SAMS region seems not to be consistent with proxy records. General commentâĂÍ Although the scientific relevance of using past information from models and proxy reconstructions to better understand variations in the SAMS is given, the study lacks of severe shortcomings (see below) which renders its usefulness. Therefore, I recommend to reject the manuscript.

Major comments I. Certainly, the manuscript needs to be proofread by a native speaking person – there are numerous strange formulations (only a few are listed in the specific comments).

A: We have asked our two native speaker co-authors to thoroughly revise the English in the manuscript. The English usage is now significantly improved.

II: The selection procedure presented on page 5656 seems to be awkward. The comparison MCA LIA implies that the authors focus on a forcing signal. As the forcing is very similar for all model simulations a definition according to the cumulative forcing is thus appropriate. If the authors would hypothesize that the changes are more due to internal variability they shall use a classical composite analysis, i.e., using a fixed length of a period (say 100 yrs) which defines the timescale of interest and assess all periods with exceed or fall below one standard deviation of an index (e.g. NH temperature). The method proposed does not have a clear motivation (hypothesis). Further, it remains unclear how the authors obtain different lengths of the periods. Also the reference period from 1250-1450 seems to be not well motivated (given the fact the eruption of 1258 is included where most of the models show a very strong response). I would suggest to use the entire period 850 -1850 as reference. The second criterion of the temperature gradient seems to be selected in particular to find ITCZ shifts, so there is a danger that the authors make circular analyses and statements.

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A: Indeed the selection of the periods was based on a somewhat subjective ad-hoc procedure. However, we believe that such a selection is justified based on the rationale put forth by the latest IPCC report, which concluded, that the “Medieval Climate Anomaly (950 to 1250) that were in some regions as warm as in the mid-20th century and in others as warm as in the late 20th century. With high confidence, these regional warm periods were not as synchronous across regions as the warming since the mid-20th century”. And “...but also internal variability, contributed substantially to the spatial pattern and timing of surface temperature changes between the Medieval Climate Anomaly and the Little Ice Age”.

Given these conclusions our hypothesis is that the changes observed during those periods result from a combination of external forcings and internal variability. The forced component of the response could in theory be expected to coincide in all the simulations, but the internally generated variability, if simulated in the models, cannot be expected to occur at the same time. It is also worth pointing out that even in the PMIP3 setup for the Last Millennium simulations there are a number of options for the solar and volcanic forcing. Hence even the forced response is subject to uncertainties and differences depending on choice of forcing combinations. Therefore our criterion for selecting these periods was to “select within the period defined by the IPCC as the MCA and LIA the time in which the warming and cooling was strongest”. This approach results in a “conditioned composite”, ensuring that we extracted the largest possible signal in each of the simulations considered. We have rerun all our calculations with the reference period as suggested: 1000-1850 AD, which is the largest common period of all simulations (see the Table for length of simulations for individual models). With regards to the second criterion, we verified that both criteria, for example warmest years in the 950-1250 period, and the temperature gradient coincided. This guarantees that no circular arguments were made, as the reviewer correctly pointed out. We wish to point out that there was no need to change the LIA and MCA periods of the individual models.

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III. Section 2.2 and 3.2: The Hadley circulation is not defined for sectors only as zonal mean. This is text book knowledge and I am amazed that the authors are not aware of this fact. The reason is simply that if one averages only over a section mass can be exchanged in longitudinal direction. So I strong recommend to read e.g. the book of Holton 'An Introduction to Dynamic Meteorology'. As the Hadley circulation is not defined for sectors the entire analysis and interpretation is useless.

A: We recalculated the global Hadley Cell (figure attached) and also calculated the local Hadley Cell by using the methodology described in Zhang and Wang (2013). In their paper, in order to evaluate the local Hadley Cell they separated the horizontal winds into their non-divergent and irrotational components and only used the irrotational part to evaluate the meridional flow. Both figures are qualitatively the same. But we now include the local Hadley Cell in the paper as well. Conclusions did not change.

IV. Definition of the ITCZ, page 5658: The authors use max. precipitation to define the ITCZ. This is problematic as authors have shown (Nicholson, S. E., Clim. Dynam. 32,1155-1171, 2009; Laederach, Tellus A, 65, 20413, 2013.). More importantly the authors extrapolate to a finer gird which makes no sense at all: (i) the model resolutions are coarse (maybe up to 1 degree) and there is no information gain when extrapolating gridded data to finer grids, (ii) precipitation can depend on very local structures also over the ocean (e.g. atmospheric waves) and may be affected by the numerics (e.g. Gibbs phenomenon). This can lead to problems when extrapolating the data.

A: For the definition of the ITCZ we used the precipitation centroid method, following Frierson and Hwang (2012), as well as Donohoe et al (2013). We are aware that each method of ITCZ definition has is own merits and problems. We thank the reviewer for the interesting papers on this regard. In order to minimize the problems over the continents, we explicitly only focused on the oceanic part. Also, given that we are interested only in the difference between the two periods, if there are any systematic errors, these will likely cancel out by looking at the difference.

V. Most of the results in section 3 and figure lack a significance test and it is not clear how the significance is performed. This is important as the changes are rather low, e.g. in Fig. 7, 6, 4, 2. I doubt that most of the changes shown are not significant and thus not relevant. This may also be related to the obscure definition of the periods.

A: The differences between LIA and MCA composites were tested using a bootstrap test (Efron, 1979). We performed 1000 iterations with the threshold for a statistical significance set to 5%, and used the bias-corrected and accelerated percentile method to estimate the confidence interval. Figures 2, 3a, 4, 5, 6 and 7 include this statistical significance test.

Specific comments

5651, title: The Authors use only PMIP3 simulations and not CMIP5, so please remove this from the title. PMIP3 LM simulations are part of CMIP5, but we are also using the HadCM simulation, which does not belong to the PMIP3/CMIP5 model ensemble. Therefore we changed the title to: ….Climate Simulations during the …

5652, 2: 'South American Monsoon System (SAMS) variability in the Last Millennium' Changed in text.

5652, 8: What is a small forcing? Do you mean external forcing? Yes, we refer to small external forcing. Included in the text.

5652, 11: The sentence starting with 'However' is unclear. We have changed the sentence and hope it is clearer now: "Therefore we used an ad-hoc definition of these two periods for each model simulation in order to maximize their differences. With this definition, several coherent large-scale atmospheric circulation anomalies were identified."

5652, 16: 'poleward shift of the South Atlantic Convergence Zone' Changed "in" to "of".

5652, 13-19: This sentence is too long and unclear.

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We have divided this statement into two separate sentences. We hope it is clearer now. It reads:"The models feature a stronger Monsoon during the LIA associated with: (i) an enhancement of the rising motion in the SAMS domain in austral summer, (ii) a stronger monsoon-related upper-troposphere anticyclone, (iii) activation of the South American dipole, which results to a certain extent in a poleward shift of the South Atlantic Convergence Zone and (iv) a weaker upper-level subtropical jet over South America. The diagnosed changes provide important insights into the mechanisms of these climate anomalies over South America during the past millennium."

5652, 25: The sentence starting with 'Because' is awkward.

We have changed the wording. "Because on precessional time scales summer insolation in both hemispheres is in anti-phase (for example, when Northern Hemisphere (NH) summer insolation is at its maximum, summertime insolation in the Southern Hemisphere (SH) is at its minimum), it weakens the monsoonal circulation and precipitation in one hemisphere while enhancing it in the other."

5653, 20: 'Vuille et al. (2012) reviewed'âĂÍ Changed "reviews" to "reviewed"

5653, 25: I suggest to write meridional temperature gradient. Suggestion implemented.

5654, 1: 'Pacific during the LIA'âĂÍ Changed "through the" to "during the". 5654, 3: 'regional ITCZ favors'

Changed. 5654, 11: Better use 'Moreover, modelling studies support a southward (northward) shift' Suggestion implemented.

5654, 21-25: This sentence remains unclear.âĂÍ We have change the sentence to: "Furthermore, the modelling experiments discussed by Broccoli et al. (2006) and Lee et al. (2011) indicate that when cooler-than-normal temperatures are imposed in the North Atlantic domain, as occurred during the LIA, the Atlantic ITCZ shifts southward. In their experiments, this in turn is related to a strengthening of the northern Hadley cell in austral summer and a slight southward shift of its rising branch".

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5654, 26: ‘approaches suggest that the particular’ Changed.

5655, 1-2: ‘have been incorporated in the third phase’ Changed.

5655, 5: Please make a line break here. Line break introduced. 5655, 7: ‘insights in the response’ Changed

5655, 9-10: The sentence is unclear, what is meant by near-global temperature anomalies’, what are the main features of South American climate and in which sense main, temporal, spatial???

We changed the sentence to: “We focus on the models’ ability to simulate the variability observed in a few key aspects of the South American climate during two periods of near-global temperature anomalies. These aspects include precipitation, temperature and atmospheric circulation.”

5655, 11: Please make a line break here. Line break introduced.

5656, 4-5: ‘past millennium are the MCA (950–1250CE) and LIA (1450–1850CE). This report also’ reads better Changed as suggested.

5656, 1. Paragraph: Just to let you know that there are new studies on the way or published assessing simulated and reconstructed temperatures: PAGES2K-PMIP3, Climate of the Past, 11, 1673-1699. Fernandez-Donado et al., Clim. Past, 9, 393-421. I think the authors should include this in the introduction, here and the conclusions as they are fundamental publications on how to compare models and reconstructions

Thanks for these references. We now discuss them in the text.

5656, 10-15: Why do the authors only use three reconstructions, this seems to be not justified given the fact that IPCC makes a much more comprehensive comparison. Another point is that this exercise is not new and the reason why the authors make the comparison for NH temperatures is also not justified.

We have updated the figure with the complete set of reconstructions used in the IPCC

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AR5.

5656, 17: ‘mostly a result’ Changed.

5656, 27: Wrong unit, a temperature gradient has NOT the unit degree C. Changed “gradients” to “differences”.

5657, 6: Distribution of which variable? The sentence now reads: “Figure 1b shows the Gaussian fit of the frequency distribution of NH temperatures of all the years defined as LIA years (red curve) and MCA years (blue curve) respectively.”

5658, section 2: It remains unclear how the authors combined the model output to a common grid. We have included the followng sentence: “All variables have been re-gridded using a simple linear interpolation to a common 2x2 degree grid”

5662-5663: This paragraph (in comparison to the first paragraph of the section 4) sounds like that PMIP3 simulations use different models than CMIP5. This is not the case. PMIP3 uses the CMIP5 models.

We have clarified which model simulations we are using.

5665, 1-2: There are no proxy archives, which directly record circulation. The archives are mostly either temperature or precipitation sensitive and then authors try to say something about circulations, which may lead to circular statements/interpretations.

We have clarified that the large-scale circulation is consistent in particular with expected changes in precipitation. “Our results indicate that the CMIP5/PMIP3 models quite accurately reproduce changes in the large-scale circulation that in turn are consistent with proxy evidence of precipitation changes over the past millennium”.

Figures: Fig. 1 b: Which temperature is shown, NH annual mean temperature? Yes, NH annual mean temperatures. This has been clarified in the caption. Fig. 2: Color scale makes no sense as no regional structures are visible, also apply a significance test and increase the labels of the color bars We have included the significance test

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and larger labels. Fig. 3a: Orange lines are not visible. We have eliminated the orange lines. Fig. 4: Unit arrow is missing so changes in the wind are not assessable. Include significance test, preferable a non-parametric test. Unit arrow is included. Significant changes are coloured in red. Fig. 5: Makes no sense as the mass stream function is not defined over a sector. We now show the regional Hadley Cell by using the irrotational part of the wind field. Fig. 6: Unit arrow is missing. Include significance test, preferable a non-parametric test. Unit arrow is included. Significant changes are coloured in red. Fig. 7: Include significance test, preferable a non-parametric test. Significance test is included.

References discussed in response to reviewer 1 Donohoe, A., Marshall, J., Ferreira, D., McGee, D., 2013. The relationship between ITCZ location and cross equatorial atmospheric heat transport; from the seasonal cycle to the last glacial maximum. J. Climate 26, 3597–3618. Efron, B. (1979), Bootstrap Methods: Another Look at the Jackknife. The Annals of Statistics, 7(1), 1-26. Frierson, D. M. W., and Y.-T. Hwang, 2012: Extratropical influence on ITCZ shifts in slab ocean simulations of global warming. J. Climate, 25, 720–733. Zhang and Wang, 2013: Interannual Variability of the Atlantic Hadley Circulation in Boreal Summer and Its Impacts on Tropical Cyclone Activity. Journal of Climate, 26, pgs 8529-8544, DOI: 10.1175/JCLI-D-12-00802.1âĂÍ

Reviewer 2:

This study analyzes climate model simulations from the CMIP5/PIMP3 to investigate the variability of the South American Monsoon with emphasis on the Medieval Climate Anomaly (MCA) and Little Ice Age (LIA) periods. The study is interesting and can be considered after major revisions. The main comment I would like the authors to consider regards the identification of the MCA and LIA periods. The criterion used (described on page 5656) considers the temperature variability in each model separately, although all models were forced with similar forcings. However, the spread among the time periods for all models is very large (Table 1) sometimes differing by 150-200 years. The ensemble model mean and uncertainties need to be considered in the analysis.

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A: Indeed the criteria for selecting the periods were not the same as used by the IPCC. Instead we defined the MCA and LIA, through a more subjective ad-hoc method, which results in a “conditioned composite” analysis. The reason for this approach is twofold: a) The IPCC concluded that the MCA in particular (but also LIA) are characterized by an important contribution from internal variability; hence we cannot expect that all models produce an MCA-like state at the exact same time. b) The simulations we use have similar but not identical forcings. For example there are various options for solar and volcanic forcings used in the implementation of the various simulations (see Schmidt et al 2012).

Given these two reasons, we expect that much of the variability seen in these simulations occurs in response to internal variability. Hence in order to maximize the extraction of a signal we choose to select the warmest years in the 950-1250 period for the MCA, and the coldest time in the 1450-1850 period for the LIA.

We now include a measure of the significance of the differences by applying a bootstrap test in all Figures where appropriate.

Other comments: Page 5657 Line 2: please replace "that" by than Corrected.

Page 5658 line 6: please explain why the precipitation was interpolated to 1 degree

To identify the ITCZ we used the method defined by Frierson and Hwang (2012): Precipitation centroid. In their own words: “The precipitation is interpolated to a 0.18 grid over the tropics to allow the precipitation centroid to vary at increments smaller than the grid spacing”.

Page 5659 line 29: do the proxy paleo records differentiate the transition seasons?

Unfortunately there is no adequate proxy network in place that would have the required resolution, or the sensitivity to transition season temperature or precipitation to capture such changes in the transition seasons.

Page 5662 lines 19-24: what could be the reasons for these differences? any speculations?

The main reason is likely because the other studies mentioned here all imposed a much stronger forcing over the North Atlantic domain. It may have been a bit misleading to compare the PMIP3/CMIP5 last millennium simulations (small forcing) with these other studies. We have clarified this in the revised manuscript.

References discussed in response to reviewer 2

Frierson, D. M. W., and Y.-T. Hwang, 2012: Extratropical influence on ITCZ shifts in slab ocean simulations of global warming. J. Climate, 25, 720–733. Schmidt, G. A., Jungclaus, J. H., Ammann, C. M., Bard, E., Braconnot, P., Crowley, T. J., Delaygue, G., Joos, F., Krivova, N. A., Muscheler, R., Otto-Bliesner, B. L., Pongratz, J., Shindell, D. T., Solanki, S. K., Steinhilber, F., and Vieira, L. E. A.: Climate forcing reconstructions for use in PMIP simulations of the Last Millennium (v1.1), Geosci. Model Dev., 5, 185–191, doi:10.5194/gmd-5-185-2012, 2012.

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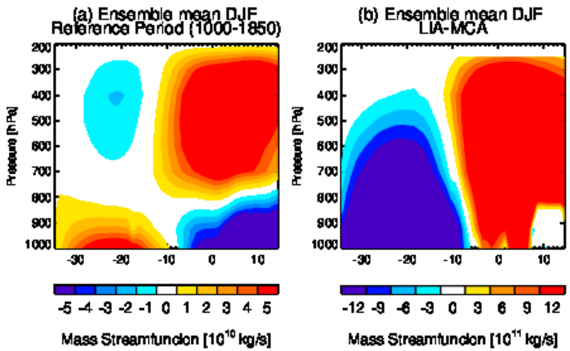

Fig. 1. Global Hadley Cell

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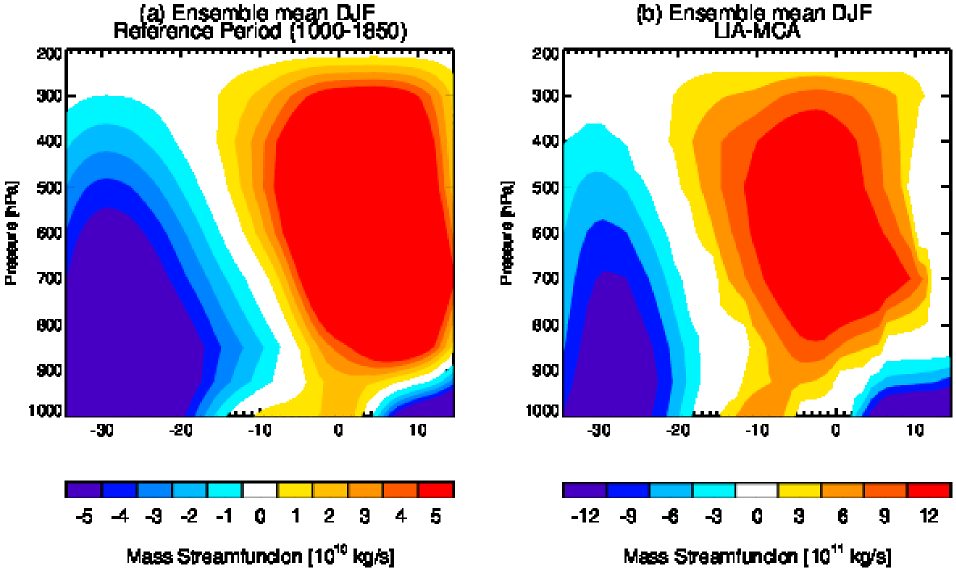

Fig. 2. Regional irrotational Hadley Cell