

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

Anonymous Referee #1

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Summary

The study analyses the South American Monsoon System (SAMS) variability 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 focusses 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 comment

Although the scientific relevance of using past information from models and proxy re-

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constructions 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).

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.

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.

IV. Definition of the ITCZ, page 5658: The authors use max. precipitation to define

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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 grid 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.

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.

Specific comments

5651, title: The Authors use only PMIP3 simulations and not CMIP5, so please remove this from the title.

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

5652, 8: What is a small forcing? Do you mean external forcing?

5652, 11: The sentence starting with 'However' is unclear.

5652, 16: 'poleward shift of the South Atlantic Convergence Zone'

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

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

5653, 20: 'Vuille et al. (2012) reviewed'

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

5654, 1: 'Pacific during the LIA'

5654, 3: 'regional ITCZ favors'

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

5654, 21-25: This sentence remains unclear.

5654, 26: 'approaches suggest that the particular'

5655, 1-2: 'have been incorporated in the third phase'

5655, 5: Please make a line break here.

5655, 5: 'insights in the response'

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???

5655, 11: Please make a line break here.

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

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

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.

5656, 17: 'mostly a result'

5656, 27: Wrong unit, a temperature gradient has NOT the unit degree C.

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5657, 6: Distribution of which variable?

5658, section 2: It remains unclear how the authors combined the model output to a common 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.

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.

Fig. 1 b: Which temperature is shown, NH annual mean temperature?

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

Fig. 3a: Orange lines are not visible.

Fig. 4: Unit arrow is missing so changes in the wind are not assessable. Include significance test, preferable a non-parametric test.

Fig. 5: Makes no sense as the mass stream function is not defined over a sector.

Fig. 6: Unit arrow is missing. Include significance test, preferable a non-parametric test.

Fig. 7: Include significance test, preferable a non-parametric test.

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