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Response to the Anonymous Referee #2 comments

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Abstract

This is the response to the reviewers. So far I keep the same format of the paper to produce the latex document
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1 Introduction

The authors would like to thank the reviewers for their constructive suggestions and the time they devoted in reading and proof-reading the manuscript. We have tried to integrate all suggestions and think that the manuscript has improved with them. We do really appreciate their contribution.

Please find below a detailed point by point response to the reviewer's comments, (quoted in italic and blue color).

2 Anonymous Referee 2

Summary: This manuscript presents a characterization and comparison between large-scale temperature reconstructions and pre-PMIP3 last millennium (LM) simulations. In addition to a very thorough and detailed review of the reconstructions and the simulations, the authors perform several analyses to test the agreement between the model responses, the forcing estimates and the temperature reconstructions and to estimate climate sensitivity.
General Remarks: This is a well-done paper that compiles a tremendous amount of disparate information, particularly on the pre-PMIP3 LM simulations, all of which will be very useful to the community. The ensemble of LM simulations has grown very rapidly in the last few years and a compendium of the now-available simulations has not been previously completed. In many cases, some of the model and simulation information (e.g. forcing choices) is very tedious to track down for the LM runs, and it is nice to see the simulation descriptions now all in one

place. I should also add that it makes sense to provide this pre-PMIP3 compilation as a benchmark for the now larger collection of PMIP3 LM runs, given that the pre-PMIP3 collection used a wider range of forcings and model configurations (e.g. resolutions). All of this is to say that the collection of information in this manuscript is timely, thorough, and very useful.
If there is a criticism of the manuscript, it would be that the latter analyses of the reconstructions and simulations are a bit underdeveloped. The authors propose and characterize several straightforward means of comparing the reconstructions and simulations, as well as estimating climate sensitivity in the models and the reconstructions. This is all done rather simplistically using large-scale temperature indices and linear assumptions about the climatic response to forcing. The authors are nevertheless transparent about their assumptions. Moreover, it should be said that regardless of the level of sophistication, this is the first study to do this with so many reconstructions and model simulations and there are already some interesting results to ponder. I therefore do not think it would be fair to criticize the authors for not achieving an ideal concept of what might be done, given the novelty of their first step and the large challenge of assimilating so many reconstructions and simulations into their analyses.
With all of the above as summary, I suggest that the paper be published with just a few minor revisions listed below.

AUTHORS' COMMENT:
The authors welcome the positive perspective of the reviewer on the paper. We are grateful for the reviewer's comments.

REVIEWER #2 COMMENT #1:
Minor Suggestions
Consider the following title change: Large-scale temperature responses to external forcings in simulations and reconstructions of the last millennium

RESPONSE:
We like the suggestion made by the reviewer and we have modified the title accordingly.

REVIEWER #2 COMMENT #2:

Pg. 4007, Ln. 6: The Jones et al. (Holocene, 2009) review article is appropriate here.

RESPONSE:

This reference has been included in the text as suggested.

REVIEWER #2 COMMENT #3:

Pg. 4007, Ln. 14: The Tingley et al. (QSR, 2012) methodological review article is appropriate here.

RESPONSE:

This reference has been included in the text as suggested.

REVIEWER #2 COMMENT #4:

Pg. 4007, Ln. 28: Why isn't Ammann et al. (PNAS, 2007) included here?

RESPONSE:

The reason is that we intended to provide some examples and not to be exhaustive with a long list of references. It has been included as suggested in the new version of the manuscript.

REVIEWER #2 COMMENT #5:

Pg. 4010, Ln. 9: Forward modeling of what? If general, Evans et al. (JGR, 2006), Thompson et al. (GRL, 2011) and Ohlwein and Wahl (QSR, 2012) should be included as additional examples of forward-modeling proxy studies.

RESPONSE:

We accept the comment made by the reviewer and we have added a couple of references more as well as modified slightly the text. We have not included Thompson et al. (2011) bacause it is rather a pseudoproxy study than a forward modelling approach. Instead we have included Baker et al. (2012) *(Section 1, paragraph 9)*:

" Strategies to circumvent this problem may be derived based on upscaling (e.g. Jones and Widmann, 2003), downscaling (e.g. Wagner et al., 2007) or forward modelling of proxy variables (e.g. Evans et al., 2006; González-Rouco et al., 2009; Ohlwein and Wahl, 2012; Baker et al., 2012)."

REVIEWER #2 COMMENT #6:

Pg. 4017, Ln. 7: It would be useful to expand why the knowledge of the forcing is considered to be a "low Level of Scientific Understanding" and what exactly the criteria for this classification were.

RESPONSE:

This classification stems from Forster et al. (2007). According to Forster et al. (2007), the level of scientific understanding (LOSU) is a subjective measure of structural uncertainty and represents how well understood the underlying processes are. Classifying the volcanic forcing as low LOSU (Forster et al., 2007) indicates that the uncertainty related is high, not only in magnitude but also in dating (Plummer et al., 2012). However, we think that it is not neccessary to expand upon this whithin the text, so we have modified the paragraph to clarify the main message as follows *(Section 3.1, paragraph 9)*:

"The reconstructions of stratospheric aerosols from volcanic eruptions are based on ice-core data from Antarctica and Greenland ~~and t~~. The derived time series of volcanic forcing ~~sets (Figure 1b)~~ tend to display consistent timing for major eruptions (Figure 1b). However, they often present differences on the magnitudes of individual events. Our knowledge of volcanic forcing over the past millennium is poorly constrained, particularly in regard to the strengths of individual eruptions (Forster et al. 2007, Schmidt et al. 2011). ~~present a similar behaviour in the timing of events. The magnitude of the volcanic eruptions shows high variability depending on the reconstruction considered. (Forster et al. 2007) classify the knowledge of this forcing with a low Level of Scientific Understanding.~~"

REVIEWER #2 COMMENT #7:

Pg. 4023, Ln. 3-4: "...is not to be expected in the model response." At the 11-yr cycle?

RESPONSE:

Correct. The sentence has been modified indicating this information *(Section 3.3, paragraph 4)*:

"Firstly, the relative contribution of the 11-yr solar cycle has been greatly diminished in all cases~~, thus indicating~~. This suggests that ~~a large~~only an small simulated global or hemispheric signal, at these frequencies, is ~~not~~ to be expected in the model response for the last millenium."

REVIEWER #2 COMMENT #8:

Pg. 4023, Ln. 22: I think the authors mean the Ammann and Wahl (2007) paper here. Also, while there were very slight improvements in that paper over the implementation of the Mann et al. (1999) paper, the method itself has been abandoned by Mann and coauthors for an alternative (RegEM). It therefore is a little strange to call Ammann and Wahl (2007) the replacement choice, when it is arguably the Mann et al. (Science, 2009) paper that represents an update from previous work, both methodologically and in terms of the expanded network that was used.

RESPONSE:

The reviewer is correct and the reference has been changed to Ammann and Wahl (2007).
We understand the reviewer's point of view about whether the Ammann and Wahl (2007) reconstruction can be considered the replacement of Mann et al. (1999), however the criterion we have applied is different. As it is indicated in the manuscript, the ensemble of reconstructions considered leans on identifying approaches that incorporate significantly different data bases or methods. Updates that improve previous methodological versions are considered as the most recent methodological variant of a given approach to reconstruct past climate and selected as the version that superseeds previous reconstruction attempts of the same type. Ammann and Wahl (2007) is one of this cases that is regarded as the reconstruction version that superseeds the Mann et al. (1999) allowing for some improvements on the same methodological approach. Mann et al. (2009) applied a new methodology, thus it is considered as a new reconstruction in

Table 4, regardless of the fact it was produced by the same group of authors.

REVIEWER #2 COMMENT #9:

Pg. 4024, Ln. 2: What is meant by annual borehole data? I do not believe these data are ever presented as such. Table 4 also presents Huang (2004) as a replacement for Pollack and Smerdon (2004). That is not entirely accurate (AR4 could have used the Huang paper instead of Pollack and Smerdon given that they were both published in 2004). Both papers are based on the same borehole data, but the Huang paper tries to blend them with the Mann et al. (1998) reconstruction. There are differences of opinion on whether or not it is a good idea to do that. One should therefore not be presented as a replacement of the other.

RESPONSE:

We have removed the word 'annual' from the text *(Section 4, paragraph 1)*:

"Even if records have annual resolution, they may not represent the real time resolution, for instance, ~~annual~~ borehole data (Huang et al., 2000),~~huang2008~~ provide information on multicentennial trends."

Regarding the second point raised by the reviewer, Pollack and Smerdon (2004) and Huang (2004) reconstructions are both based on the same borehole information as the reviewer points out. We have chosen the Huang (2004) reconstruction because it includes high frequency information, unlike the Pollack and Smerdon (2004), thus allowing us to include the borehole reconstruction also in the subsequent analysis of the manuscript (i.e. the regression based calculations). The Pollack and Smerdon (2004) reconstruction was the one that was used in the AR4. However, it is correct, as the reviewer indicates, that Huang (2004) reconstruction is not the replacement for Pollack and Smerdon (2004), thus we have included a note in Table 4 to clarify this:

"(*)In AR4 Pollack and Smerdon (2004) was considered. Instead, the reconstruction provided in Huang et al. (2004) has been selected herein because it includes high frequency

variability that will be useful for the analysis in Sect. 6."

REVIEWER #2 COMMENT #10:

Pg. 4024, Ln. 3: It is strange to say that many of the reconstructions that follow have "low-variance on inter annual timescales." Many of these reconstructions explicitly have been filtered or only target decadal or lower signals.

RESPONSE:

We agree with the reviewer that some reconstructions have been filtered or developed to specifically target decadal or lower frequency variability, despite of the data being provided at annual resolution. However, this is not the case for all the reconstructions and it was precisely the different behaviour among the reconstructions what we tried to remark with the statement. We have modified the sentence to clarify the message *(Section 4, paragraph 1)*:

"Even if records have annual resolution, ~~they~~this may not represent the real time resolution, for instance, in the case of~~annual~~ borehole data (Huang et al., 2000~~, 2008~~) that provide information on multicentennial trends. This also occurrs with~~Also~~ some ~~of the~~other reconstructions in the table that, ~~have~~show low variance on interannual timescales despite of the data being provided at annual resolution (Briffa et al., 2001; Hegerl et al., 2007b; Loehle, 2007; Mann et al., 2009; Leclercq and Oerlemans, 2012; Christiansen and Ljungqvist, 2011)."

REVIEWER #2 COMMENT #11:

Pg. 4027, Ln. 14: It strikes me that there may be another reason for excluding the earlier part of the simulations: initial conditions. This brings up a larger point: how consistent were the initial conditions across the simulations or how consistently were they chosen? This is worth mentioning and potentially something that could be included in one of the simulation summary tables.

RESPONSE:

The earlier parts of the simulations are not excluded, neither for the runs starting around 1000

AD nor the ones starting around 1500 AD (CCSM3, HadCM3). If excluding the earlier part would have been based on avoiding the influence of initial conditions, using the 500 yr long runs would have been inconsistent with that decision. The only reason for choosing 1500 AD as the beginning of the reference period is (as stated in the text) that not all the simulations start before 1500 AD.

Regarding the initial conditions issue, no systematic trend is observed in the simulations at hemispheric or global scales (Fig. 4), except for one run of the ECHO-G (cyan dashed lines in Fig. 4) and, in the first submitted version of the manuscript, for the CNRM simulation. The latter has been drift-corrected in the current version of the text (see comment #14 of this reply) and the former is already explained in the manuscript and only the corrected NH average (Osborn et al., 2006) is considered in the study in Sections 5 and 6. If we consider the whole suite of models included in the study, some of the simulations (CCSM3, CSM1.4, CNRM, HadCM3) have been corrected for climate drift by estimating long term trends from available preindustrial control runs. The rest of the models (EC5MP, ECHO-G, CSIRO, IPSL) reach a quasi-equilibrium state after a spin-up period in a control simulation, before starting the transient forced experiments. It is also important to consider that the equilibrium state observed for the case of the temperature is not always applicable for other variables, like those related with the state of the ocean. For example, even if the surface temperatures are in quasi-equilibrium, Ortega et al. (2012) show the importance of the initial conditions and the trend observed in the ocean heat content due to the intermediate and deep layers of the ocean not reaching equilibrium.
We agree that the initial conditions that are used to start the simulations play an important role, however this information is not available for most of the simulations, even in the original references. Therefore, providing and attempting to discuss this issue in detail is beyond the posibilities of the current manuscript. However, we agree that it is useful to include the information about which of the simulations are drift-corrected and which ones are not *(Section 1, paragraph 3)*:

"Some of the simulations (CCSM3, CSM1.4, CNRM, HadCM3) have been corrected for climate drift by estimating long term trends from available preindustrial control runs."

REVIEWER #2 COMMENT #12:

Pg. 4029, Ln. 7: "Since the quality of the...forcing factors...can be considered stable through time..." I think this is a dubious statement even for estimates besides the volcanos. The authors might want to provide more justification for this statement.

RESPONSE:

The basis for our statement is that there is no evidence to affirm that the quality of the forcing reconstructions is worse at the beginning of the millennium relative to other periods. Plummer et al. (2012) or Schmidt et al. (2011) have evidenced some discrepancies in the timing of the volcanic events, however, for the rest of the forcings there is not a reason to think that uncertainties or problems in the solar and volcanic forcing reconstructions at the beginning of the millennium may account for the differences between reconstructions and observations. For instance, we may consider solar forcing which is, together with the volcanoes, the most important contributor to the low frequency variations during this period. Solar variability reconstructions are based (see Sect. 3.1) on the ^{10}Be isotope from Antarctic ice cores or the ^{14}C isotope concentration in tree rings. There is no report (e.g. Bard et al. 2000; Crowley 2000; Krivova et al. 2007; Steinhilber et al. 2009) of potential problems in these reconstructions during the 10th-11th perios that would justify larger uncertainties during this time than in subsequent periods of the millennium. We have reformulated slightly the sentence to emphasize a bit more the existing discrepancies in the forcings *(Section 5.1, paragraph 3)*:

"Thus the discrepancy can be essentially established on the ground of differences between reconstructions and forcings. Although forcing factors over the last millennium are not perfectly constrained (e.g. Plummer et al., 2012; Schmidt et al., 2011),~~Since~~ the quality of ~~the~~their reconstructions ~~of forcing factors~~ (Sect. 3) can be considered ~~stable~~homogeneous through time ~~during the whole millennium (except for some discrepancies in the timing of volcanic events; Plummer et al., 2012; Schmidt et al., 2011)~~, and thus it can be argued that this

discrepancy points to a problem in the reconstructions during this period."

REVIEWER #2 COMMENT #13:

Pg. 4035: It would be useful to show the Mann et al. (2009) proxy pattern. Even better, the authors might consider calculating pattern correlations between the proxy pattern and the model patterns. This would provide a quantitative measure of the agreement (or disagreement).

RESPONSE:

This comment has been raised by both reviewers (see R#1, comment #18) and we agree that it is helpful and more illustrative to show also the Mann et al. (2009) MCA-LIA pattern. Therefore, we have modified Figure 7 to include the reconstructed pattern for comparison with the simulated ones.

Regarding the second point of the reviewer, we have also calculated the spatial correlations between the reconstructed and the simulated patterns. The spatial correlation values (see Fig. 7 in the current version of the manuscript) between the STSI (ssTSI) simulations and the Mann et al. (2009) MCA-LIA pattern of temperature differences range between 0.01 and 0.36 (-0.18 and 0.23). The relatively low correlation values are not surprising based on the discrepancies already indicated in the manuscript. If we calculate the spatial correlations of the MCA-LIA temperature differences between all the possible combinations of two simulations (coming from different models) within the STSI (ssTSI) group, the values obtained range between -0.09 and 0.69 (-0.14 and 0.55). The higher values of correlation are motivated by the spatial distribution of temperature differences associated to the forced response, showing larger values over the continental and ice covered areas that are overall consistent among the models (see also Zorita et al. 2005). For the STSI group this produces higher values of correlation (e.g. r=0.69 in the case of CNRM with EC5MP-E2) than for the ssTSI subensemble (e.g. r=0.55 in the case of a EC5MP-E1 member with one of the CSIRO runs). This is due to the lower amplitude of the solar forcing in ssTSI, that induces a weaker temperature response, thus lower correlation values. Additionally, the regional differences due to internal variability and large cooling/warming areas related to ocean convection at high latitudes of the SH contribute to lower down the correlation

indices. This produces really low values of correlation not only for the STSI group (e.g. r=-0.09 between CNRM and CSM1.4) but also for the ssTSI (e.g. r=-0.14 between an EC5MP-E1 run with one form the CSIRO model).
Also, additional to the previous inter-model comparison, it is interesting to consider the intra-model spatial correlation indices, i.e. the spatial correlations obtained from a simulated pattern when correlated with tose from the same model subensemble. In this case, for the ssTSI group the values vary between 0.21 and 0.52 for the EC5MP-E1 and between 0.05 and 0.52 for the CSIRO. For the STSI, the EC5MP-E2 varies from 0.60 to 0.81. The lower values obtained in the ssTSI group (and the wider range) indicate that internal variability plays a major role here than for the STSI in which the response to the forcing applied becomes more prominent.
This discussion has been included in the text *(Section 5.2, paragraphs 7-8)*.

REVIEWER #2 COMMENT #14:
Pg. 4035, Ln. 19: It would be really nice to know what is going on with the CNRM model. I do not expect a full diagnosis, but it is such an outlier that the authors might want to address it. Do they have any thoughts on why it shows such different behavior in the SH?

RESPONSE:
The reviewer here actually identifies an important issue in the manuscript. The CNRM presented an anomalous behaviour, focused mostly on the SH (see previous Figures 4c and 7). The evolution of the temperature in the SH presented a significant trend for the first six centuries of the millennium, showing temperatures much lower than the rest of the simulations. This outlier behavior was even more noticeable in the spatial representation of the MCA-LIA temperature change, in which almost the whole SH showed negative anomalies. During the review process, a failure in the correction of the drift for the CNRM simulation was detected, and consequently, the data were modified according to the new drift-correction. The new version of the drift-corrected temperature simulated by the CNRM has mainly affected Figures 4 and 7. In the current version, the CNRM does not present any outlier behaviour and the general cooling observed in the SH in Figure 7 has been reduced just to some regional features. Also Figures 5, 6, 8 and 9 have slightly changed due to relatively small changes in NH averages (mostly due

to changes in lower latitude temperatures). However, for the NH the corrections are not very important and the final results and conclusions of the work are not modified. All comments in the manuscript dealing with this issue have been deleted/revised (for instance *Section 5.1, paragraph 6 or Section 5.2, paragraph 7).*
This question was also raised by Reviewer #1 (see Comment #19).

REVIEWER #2 COMMENT #15:

Pg. 4036: It would be appropriate to point out a few things regarding the spatial pattern of the Mann et al. (2009) reconstruction. While it is a valuable estimate, I am a little nervous to point to it as the pattern estimate gold standard. There are of course many uncertainties in the reconstructions and the estimated spatial patterns are subject to the largest of them. A few recent studies in the literature have begun to articulate this view (Smerdon et al., GRL, 2011; Li and Smerdon, Environmetrics, 2012; Annan and Hargreaves, Clim. Past, 2012; Werner et al., J. Clim., 2012). The authors should at least point out these potential spatial uncertainties as a caveat regarding the pattern estimates in the reconstructions.

RESPONSE:

Regarding the comment of the reviewer about the robustness of the Mann et al. (2009) reconstruction (raised also by Reviewer #1, comment #18), some comments can be made. The submitted version of the manuscript may have not transmitted properly the uncertainties associated to this reconstruction, mostly related to methodological issues (Li and Smerdon, 2012; Smerdon et al., 2011) and the small number of proxies available over ocean basins. However, the Mann et al. (2009) reconstruction is the only spatial reconstruction that offers global scale information about the MCA-LIA temperature differences. Paralell to this, Ljungqvist et al. (2012) analyses a large network of temperature-sensitive proxy records located in NH extratropical land areas that agrees with the Mann et al. (2009) pattern in depicting the extratropical warmth during the MCA relative to LIA. However, the information provided by Ljungqvist et al. (2012) is not as spatially extensive as the Mann et al. (2009) reconstruction. Additionally, the negative anomalies shown in the Pacific by Mann et al. (2009) are supported by results of other studies (Seager et al., 2007; Graham et al., 2011). Therefore, we think it is

legitimmate to use the Mann et al. (2009) MCA-LIA pattern for model-data comparison, and, as the reviewers suggest also highlight better the uncertainties of this reconstruction *(Section 5.2, paragraph 9)*:

"Mann et al. (2009) is the only spatial reconstruction that offers global scale information about the MCA-LIA transition, and although supported by several studies (Seager et al. 2007, Graham et al. 2011), it is also subjected to important uncertainties (Li and Smerdon 2012, Smerdon et al. 2011). These uncertainties are mostly associated to the reconstruction methdology and the low proxy replication in the Pacific and North Atlantic basins.~~Based on these discrepancies,~~However, if this proxy-based reconstructions ~~are~~were to be considered reliable, two possible explanations ~~are~~could be suggested for the aforementioned model-data discrepancies~~results shown in Figure ??~~."

REVIEWER #2 COMMENT #16:

Pg. 4038, Ln. 3-6: I do not understand how the authors have accommodated the decadally resolved reconstructions in this moving window scheme. Do these correlations mean anything if the true DOF are approximately 3 in the moving window?

RESPONSE:

The phrasing of the text must have lead to some misunderstanding. No moving window was used. The whole series were filtered with a 31-year moving average and correlation calculated afterwards.We have modified the text as follows *(Section 6, paragraph 4)*:

"Figure 8b further illustrates the linear relation between forcing and temperature response by showing the correlations (dots) between 31 yr ~~moving averaged~~filtered temperatures and TEF for all the simulations spanning the whole millennium."

REVIEWER #2 COMMENT #17:

Figs. 3-4: I am not a big fan of including the Huang et al. (2008) curve in this analysis. That particular borehole inversion targeted the last 20,000 years. It did this by merging three

data types: the instrumental record, the high-quality temperature profiles used for the 500-yr inversions, and much noisier heat flux data for the period prior to about 500 years ago. This data merge has its advantages and disadvantages, but it may particularly impact the character of the smaller events like the MWP/LIA relative to the much larger LGM signal. The temporal resolution of the inversion is also much lower (and progressively less back in time), making the timing and amplitude of the MWP/LIA events less constrained than the other reconstructions. For these reasons, it is an apples-to-oranges comparison in this context.

RESPONSE:
We accept the comment of the reviewer and we have removed this reconstruction from the analysis. It was included in the submitted version of the manuscript under the attemp of using all available reconstructions targetting global scales since the number is very low, but we agree with the reviewer.

REVIEWER #2 COMMENT #18:
Language Note: The manuscript would generally benefit from some language improvements. I am not going to list all of the examples, but the authors would do well to read over the manuscript carefully for language choices. One formulation that occurs repeatedly is exemplified by the following: "can contribute to extend the knowledge." This should be replaced by: "can contributed by extending the knowledge." Similarly, "that allows to compare" should be changed to "that allows comparisons of."

RESPONSE:
A native english speaker has proof read the manuscript and we have modified it accordingly.

3 Conclusions

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