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Manuscript prepared for Clim. Past Discuss.
with version 2.2 of the LaTeX class copernicus_discussions.cls.
Date: 17 December 2012

Response to the Anonymous Referee #1 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 to the manuscript. We have addressed 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 1

The authors describe the simulations covering the last millennium performed with GCMs before the CMIP5-PMIP3 coordinated experiments as well as a comparison of those simulations with reconstructions. The description of the models, experimental set up is very precise and helpful to propose hypothesis explaining the different results of the different models. In particular, the discussion of the influence of the magnitude of the solar forcing is instructive. I would have been happy of course to see also a comparison with the new CMIP5-PMIP3 simulations but the paper is already quite long and adding new experiments would have certainly make it harder to follow. The present work could then be considered as a basis with which the new simulations can be compared. On the same lines, some parts of the manuscript may have been included in supplementary material to shorten the text but, as the structure is clear, the reader can go quickly to the parts s/he is interested in, so I am fine with the present way of presenting the information. I suggest thus only minor changes before the final publication.

AUTHORS' COMMENT:

The authors welcome the positive perspective of the reviewer on the paper. It is indeed a lengthy manuscript and, as stated in the text and commented by the reviewer, one of the motivation is to contribute with a review/discussion of the temperature response in pre-CMIP5/PMIP3 simulations. Applications to CMIP5/PMIP3 are currently being developed.

REVIEWER #1 COMMENT #1:

From the disagreement between the model results and reconstructions, the abstract suggest a large role of internal variability in the spatial distribution of the changes between MCA and LIA. If it was the case, by chance, a few members of the whole ensemble should have a pattern similar to the one reconstructed. This does not seem to be the case (see Fig. 7). We could of course not rule out the fact that the real observed pattern is related to the occurrence of a rare event but the disagreement could also be due to a bad representation of the response to the forcing in models, to uncertainties in the forcing time series, uncertainties in the reconstruction, or to a bad representation of internal variability in models. This should be indicated in the abstract (and where needed in the main text) to have a fair balance between the hypotheses.

RESPONSE:

We agree with the reviewer. In the present version we have modified the abstract so that it represents the same balance of arguments than in the text. Regarding uncertainties in forcing, it is not evident how these can contribute to the mismatch between reconstructions and simulations at regional scales in the MCA-LIA changes. Thus, the abstract has been modified as follows:

"Internal variability is found to have an important influence at hemispheric and global scales. The spatial distribution of simulated temperature changes during the transition ~~of~~from the Medieval Climate Anomaly to the Little Ice Age disagrees with that found in the reconstructions~~, thus advocating for internal variability as~~. Thus, either internal variability is a possible major player in shaping temperature changes through the millennium or the model simulations have problems to realistically represent the response pattern to external forcing."

REVIEWER #1 COMMENT #2:

Page 4012, line 19. It is stated that the range of forcing goes beyond the one of the CMIP5-PMIP3 exercise. Is it still valid when all the forcing proposed in Schmidt et al. 2012 are included?

RESPONSE:

That sentence was misleading and has been modified in the present version. The message we intended to convey was that for some specific forcing factors the pre-PMIP3 simulations used some forcing estimations that are not included in Schmidt et al. (2011, 2012) and therefore complement the variety of forcing estimations considered therein. Particularly, in the case of solar forcing, TSI reconstructions that present a percentage of change from the LMM to present between ∼0.2% and ∼0.3% (STSI simulations in the manuscript) are not incuded in Schmidt et al. (2011, 2012). The case with even a larger %TSI change between LMM-present provided by Shapiro et al. (2011) is suggested in Schmidt et al. (2012) as of potential use in sensitivity experiments and argued (Feulner, 2011) not to produce results compatible with proxy evidence. We have modified the text, in order to clarify the message, as follows *(Section 2, paragraph 2)*:

"~~The range of forcing factors and magnitudes considered herein goes beyond those included in the CMIP5-PMIP3 exercise.~~ The variety of forcing factors and forcing reconstructions considered herein complement those used for the CMIP5-PMIP3 experiments, by considering some estimations not included in Schmidt et al. (2011, 2012)."

REVIEWER #1 COMMENT #3:

Page 4014, line 5. How the radiative forcing of land use changes is calculated? Does it include only the albedo effect or also the role of modifications in exchanges between land and the atmosphere (of water, momentum)? See also page 4020, line 21.

RESPONSE:

The radiative forcing associated to the land use changes is available only for the EC5MP (see text). For this model, it was calculated off-line with the ECHAM5 isolated radiative transfer

code. The radiative forcing at the top of the atmosphere is defined as the change in radiative fluxes at the top of the atmosphere due to the change in one single variable (Jungclaus et al., 2010), land-cover-change in this case. The related radiative forcing reflects only the effect of changing surface albedo.
The text has been modified including this information as follows *(Section 3.2, paragraph 6)*:

"~~Here, the estimation of the forcing includes biogeophysical interactions,~~ <u>The radiative forcing associated to these land use changes was calculated off-line using the radiative code of the ECHAM5, thus including only the albedo effect (Jungclaus et al. 2010),</u> and it causes a long term cooling that adds to that of aerosol forcing (Pongratz et al., 2009, 2010)."

REVIEWER #1 COMMENT #4:
Page 4016, lines 3-7. CMIP5-PMIP3 protocol suggests several reconstruction of the TSI, not just one.

RESPONSE:
The reviewer is correct. The different estimations of TSI are based on several reconstructions (Wang et al., 2005; Muscheler et al., 2007; Delaygue and Bard, 2011; Steinhilber et al., 2009; Vieira et al., 2011) as is described in Schmidt et al. (2011). All of them show a %TSI change between LMM and present lower than 0.1, thus in the same range as the ssTSI simulations in the manuscript. According to this, the text should be modified, either including all the original references or leaving just the one of Schmidt et al. (2011) that includes all this information. We consider that it is not necessary for the message to cite all the references, so we prefer to leave just Schmidt et al. (2011).
The text has been modified as follows *(Section 3.1, paragraph 5)*:

"Figure 1a also shows the TSI reconstructions suggested by Schmidt et al. (2011)~~, based on Wang et al. (2005)~~, to serve as boundary conditions for the CMIP5-PMIP3 last millennium simulations [...] Such variability is difficult to reconcile with the ~~Wang et al. (2005)~~ <u>solar forcing estimations in Schmidt et al. (2011)</u> ~~estimations~~ and with comparisons of climate

reconstructions with simulations ~~of the~~ using the Climber-3α EMIC driven by the Shapiro et al. (2011) estimates (Feulner, 2011).”

REVIEWER #1 COMMENT #5:
Page 4022. I do not understand well the last sentence, please rephrase.

RESPONSE:
We have tried to clarify the message of the sentence and modified the text accordingly *(Section 3.3, paragraph 3)*:

”~~This forcing is the only one not suffering from the noise of Gibbs oscillations due to the contribution of land use which induces variability at all timescales.~~ The radiative forcing associated to the land use changes in the EC5MP introduces variability at high and low frequencies (Jungclaus et al. 2010; Fig. 1a), thereby contributing to avoid Gibbs oscillations in the EC5MP spectra of anthropogenic forcing (red line in Figure 2b).”

REVIEWER #1 COMMENT #6:
Page 4024, line 24. I think it is important to explain here how the uncertainties are obtained.

RESPONSE:
The process to calculate the shading in Figures 3 and 4 is the same as the one applied in Jansen et al. (2007). To obtain it, the temperature axis was divided into 0.01 ^{o}C pixels and for each year, a score of 1 (2) was given to pixels lying within the range of the reconstructed temperatures ±1.645 standard deviation (±1 standard deviation). The scores are summed over all recontructions considered and scaled to range between 0 and 1 by considering the number of reconstructions included. The resulting number indicates the degree of overlap (being a value of 1 equivalent to a 100% of overlap) among the reconstructions.
A couple of sentences have been introduced in the text to explain this process *(Section 4, paragraph 3)*:

"The grey shading ~~represents a measure of~~is the overlap among the ensemble of reconstructions taking into account their uncertainties as in Jansen et al. (2007). For each year, the temperature axis is divided in 0.01°C pixels that receive a score of 1 (2) if they lie within the range of the reconstructed temperatures ±1.645 (±1) standard deviation. The scores are summed over all recontructions considered and scaled to range within 0 and 100% of overlap. The resulting uncertainty distribution is the basis for the model-data comparison in Sect. 5.1."

REVIEWER #1 COMMENT #7:
Page 4027 and Figures3-4. I do not see why changing of reference period so I suggest keeping the same one.

RESPONSE:
The decision of changing the reference period is based on two arguments: i) the fair reference period to show the reconstructions is the instrumental one in which they have been calibrated; and ii) the fair reference period to show all the simulations is the pre-industrial one in which the most relevant forcing factors applied (solar and volcanic; GHG are fairly stable in this period) are considered by most of the simulations (except for a few solar only runs). During the industrial period, models use different forcing configurations, thus making this period not appropiate to be used as a reference as this induces different trends (added to the different model sensitivities) in the 20th century temperature responses (see Sect. 3).
We have slightly modified the text in order to clarify a bit more the message *(Section 5.1, paragraph 1)* as follows:

"Figure 4 shows hemispheric and global temperature anomalies with respect to the period 1500-1850 AD for the suite of simulations listed in Table 1. The reason for ~~the choice of this reference period is that after 1850 AD~~this choice instead of the 1850-1990 AD period used for reconstructions (Fig. 3) is that during 1850-1990 AD the various simulations use different forcings (see Sect. 3) and therefore their trends during this period are ~~therefore~~ not comparable and render this time interval as invalid as a common reference for model

simulations. Additionally, the choice of a longer period (e.g. the whole millennium) is precluded by the fact that some of the simulations in Table 1 only span the last 500 yr. Thus, the choice of 1500-1850 AD as a reference secures a period of data availability for all the simulations and in which the forcings applied were similar."

REVIEWER #1 COMMENT #8:
Page 4028, line 4. Personally, I do not see the minima in the Wolf, Spörer, Maunder and Dalton intervals in all the simulations so this sentence should be more precise or the amplitude of the minima quantified to convince the reader.

RESPONSE:
We agree with the reviewer and our sentence was not correct because not all the simulations show minima in the solar intervals described. For example, the IPSL does not simulate the Dalton minimum and neither IPSL nor CNRM show a decay in the simulated temperature in the Wolf minimum. This is not surprising since internal variability can act in the direction to dump or to reinforce the forced response. This becomes visible if several simulations with the same AOGCMs are considered (e.g. Fig. 4a). Therefore, we have slightly modified the sentence as follows *(Section 5.1, paragraph 2)*:

"In spite of the relative differences among the inter-model forcing configurations, the trajectory of all simulations shows a high degree of similarity. Most of the simulations show minima during~~Minima are simulated in~~ the Wolf, Spörer, Maunder and Dalton intervals, albeit modulated by the presence of volcanic activity for those simulations that incorporate it."

REVIEWER #1 COMMENT #9:
Page 4028, line 23-25. The agreement between data and simulations using different reconstructions of the solar forcing should be quantified. Sentence like "STSI ensemble seems to follow most closely the reconstruction ensemble average" are not precise enough. Furthermore, is the agreement better for all the members of the STSI ensemble or just for a few? For instance, a comparison of the variance over the pre-industrial period, in an additional table, may be in-

structive. The discussion of those points could also be related to the one of figures 5 and 6 and to section 6 (last sentence of the section for instance).

RESPONSE:

In order to quantify the relationship between the NH reconstruction ensemble average and the simulations, the Taylor diagram (Taylor, 2001) for these variables is presented in Fig. 1 in this reply for the pre-industrial period. In the Taylor diagram, the angular position is assigned to the correlation coefficient while the radial (along-axis) distance from the origin is assigned to the ratio of the simulated to the reconstructed standard deviations. The gray dot corresponds to the ideal situation in which there is a perfect concordance between simulations and reconstruction (correlation and ratio of the standard deviations equal to one). The distance form the gray point to each individual point represents the RMSE for each model. This plot confirms the statement made in the manuscript: most of the STSI models show values of correlation around 0.6, while the ssTSI ones correlate in the range 0.4 - 0.5. And also, the STSI models have standard deviations closer to the standard deviation values of the NH ensemble reconstruction than the ssTSI models, that present lower values, around 0.5, i.e. half of the standard deviation value of the reconstruction ensemble average. The paragraph has been modified taking also into account the next comment of the reviewer (see response to comment #10) in which the possible role of internal variability in the discrepancies between simulations and reconstructions is highlited. Although the information provided by Fig. 1 is interesting and supports the statement made in the manuscript, we would prefer not including this quantification of the agreement in the text. A table with the values or a plot like the one included in this reply would increase the length of a manuscript that is already long. We think that the better agreement of the STSI group in this figure is already visible in Fig. 4 in the main text. Additionally, we would rather keep a more in depth treatment of this analysis usinga Taylor diagrams in a follow-up manuscript that is currently under preparation.

Regarding the statement at the end of the Sect. 6:

"No conclusive statements can be drawn regarding which level of solar variability is more realistic based on this assessment of consistency between reconstructions and simula-

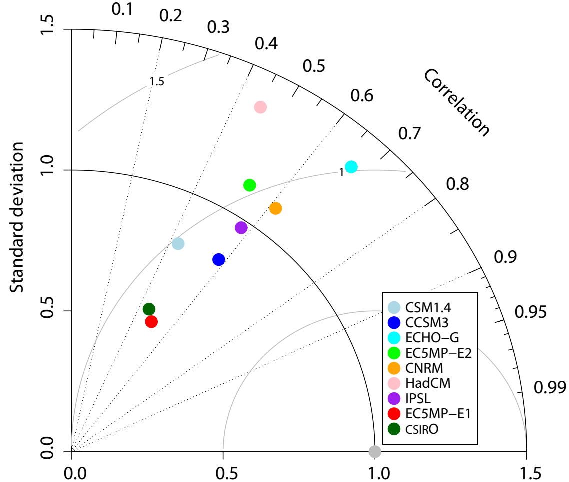

Fig. 1. Normalized Taylor diagram comparing the NH average reconstruction against the average of all simulations for each model (time series in Fig. 4b).

tions.”

We would rather keep this statement because, as it is explained in the next comment of the reviewer, the better agreement of the STSI group could be also for the wrong reasons since the internal variability could also play a role in the discrepancies between simulations and recontructions, thus we do not have conclusive information about the most realistic level of forcing.

REVIEWER #1 COMMENT #10:

Page 4029, line 1. The role of internal variability is mentioned several times in the manuscript but maybe it could play a role in this discrepancy too.

RESPONSE:

We understand the concern of the reviewer. A comment has been incorporated in the direction suggested wihin the text, and the whole paragraph has been slightly modified to make it more clear *(Section 5.1, paragraph 3)*:

"Firstly, the areas of larger density in the reconstruction spread tend to show more low frequency variability than the simulations. This areas are mostly a contribution of the reconstructions showing larger amplitude changes at multi- decadal and centennial timescales (Fig. 3). T ~~t~~he STSI ensemble seems to follow most closely the ~~reconstruction ensemble average.~~low frequency changes in the reconstruction spread while the~~The~~ ssTSI ensemble ~~therefore~~ shows somewhat less low frequency variability ~~than the reconstructions~~, particularly in the MCA transition to the LIA. It is difficult to ascertain whether this could indicate a problem on the side of the models or on the side of the reconstructions~~, due to the many factors contributing to the spread as discussed above~~. If the ~~reconstructions~~latter were to be taken as a reliable estimate of the amplitude of past low frequency variability, this would suggest that either models underestimate the real world sensitivity or that forcing changes in preindustrial times in the ssTSI group (Fig. 1) are underestimated. Alternatively, low frequency multi-decadal and centennial changes produced by internal variability at hemispherical scales could optionally also account for the reconstructed spread. However such low frequency variability is not simulated in any of the available control runs (not shown) and would mean that AOGCMs underestimate internal variability at these timescales."

REVIEWER #1 COMMENT #11:

Page 4029, line 2. Large discrepancies are also found during the 20th century.

RESPONSE:

In the 20th century, the simulations lie within the uncertainty band built from the reconstructions (Figure 4a). Figure 4b clarifies this behaviour. The NH reconstruction average shows a trend that is within the range of the ones described by the suite of models, mainly due to the different models and external forcing configurations available. The largest discrepancies between simulations and reconstructions are found in fact in the 10th and 11th centuries. The evolution of the simulations lie within this period out of the area of maximum overlap of reconstructions during these times (Figures 4a and 4b), and this is what is highlited in the manuscript.

REVIEWER #1 COMMENT #12:
Page 4031, line 14. Do the models also underestimate the variability compared to instrumental observations during the last 150 years?

RESPONSE:
In order to address this question we have calculated the normalized spectra of the simulations and reconstructions for the last 150 years (1850-2000 AD). Figure 2 in this reply shows these spectra for the simulations (Figure 2a), for the reconstructions (Figure 2b) and for the instrumental data (inset in Figure 2b). Reconstructions show a very similar behaviour to the instrumental data, which is not surprising since this is the period of calibration. However, simulations present still a decay in the spectral density at high frequencies instead of the stable variance level, shown by instrumental data and reconstructions. Thus, Figure 2 evidences that the arguments in the manuscript are still valid for the last 150 years.

REVIEWER #1 COMMENT #13:
Page 4031, line 21. The difference of variance in the 10yr timescale between models and observations does not appear to me as outstanding compared to other periods. Could you please quantify this point?

RESPONSE:
Maybe the text was misleading when we refered to ”observations”, we meant the instrumental observations (inset in Fig. 5b). On the other hand, following the suggestion of the reviewer, we have modified the paragraph in order to avoid overinterpreting the spectra *(Section 5.1,*

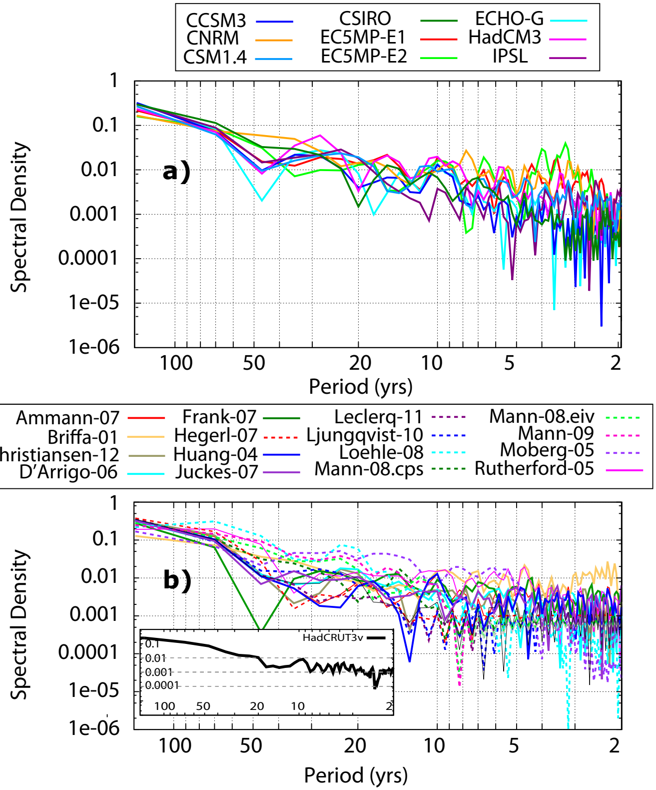

Fig. 2. As in Figure 5 of the manuscript, but for the last 150 years of the simulations and reconstructions (1850-2000 AD).

paragraph 8):

"The instrumental observations accumulate noticeable variance in the 10 yr timescale. This is evident for many of the reconstructions (see solid lines in Fig. 5b) and less so in the simulations. The EC5MP-E1 ensemble are the forced simulations with more variability at this timescale as expected since they incorporate an 11 yr cycle in the solar variability throughout the millennium (Sect. 3.1). ~~In spite of this, there is no clear signature of anomalous decadal variability for NH averages. This suggests that either the mechanisms that should generate this signal in the atmosphere are not well reproduced by AOGCMs (Gray et al. 2010) or that this variability receives contributions from other internal sources in the system.~~"

REVIEWER #1 COMMENT #14:
Page 4032, line 20. Is a word missing in this sentence?

RESPONSE:
This sentence was too long and perhaps therefore misleading. It has been slightly rephrased as follows *(Section 5.2, paragraph 1)*:

"Despite the aforementioned discrepancies in the MCA temperatures, reconstructions and simulations (Figures 3 and 4) evidence higher (lower) temperatures during the MCA (LIA) which are also seemingly in agreement with larger (lower) values in TEF~~, thus suggesting that external forcing made an important contribution~~. Therefore, external forcing (mostly solar and volcanic variability) may have been an important contributor to the energy balance in the MCA to the LIA transition (e.g. Crowley, 2000; Bauer et al., 2003; Goosse et al., 2005). ~~External forcing, i. e. solar and volcanic variability in this case, may have not been the only important factor in driving the transition from the MCA to the LIA, particularly at regional scales where~~However, other factors like land cover changes (Goosse et al., 2006) or internal variability (Goosse et al., 2012a,b) may have also been relevant, particularly at regional scales."

REVIEWER #1 COMMENT #15:
Page 4034, line 16. "Teleconnection" is probably not the best word in this framework.Maybe "pattern" would be better.

RESPONSE:
This has been changed as suggested by the reviewer.

REVIEWER #1 COMMENT #16:
Page 4034, line 28-29. Why choosing those two models compared to the others? This does not seem obvious to me that they are clearly different.

RESPONSE:
This part of the text is discussing the results in Mann et al. (2009) where maps for MCA-LIA change were shown for those two models. Perhaps this was not clear in the manuscript and we have made slight changes to clarify it *(Section 5.2, paragraph 5)*:

"Mann et al. (2009) showed a reconstructed pattern of MCA-LIA temperature change indicating enhanced and pervasive cooling in the Eastern equatorial Pacific cold tongue region, often referred to as La Niña-like background state, as well as positive anomalies dominating at mid and high latitudes of the NH. Mann et al. (2009) also showed that ~~T~~the negative anomalies in the Eastern equatorial area were not reproduced by forced simulations with the GISS-ER and CSM1.4 models."

REVIEWER #1 COMMENT #17:
Page 4035, line 12. Why selecting one member of EC5MP-E1 ensemble?

RESPONSE:
We have selected three members of the EC5MP-E1 ensemble, not one. As it is stated in the text, out of the 5 members in the EC5MP-E1 ensemble, we have selected the ones that present the most different spatial patterns. We have modified a bit this sentence to clarify this *(Section 5.2, paragraph 6)*:

"For the ssTSI models a selection of the ensemble members is made considering either the runs with a more complete configuration of external forcings (i.e., the three members including volcanoes) in the case of the CSIRO), or the three ones that represent the most different spatial patterns in the case of the EC5MP-E1 ensemble."

REVIEWER #1 COMMENT #18:

Figure 7. For me, it is necessary to show the figure from Mann et al. (2009) for a better comparison. It would also be instructive to know if the spatial structure of the changes is more consistent between the models or between any model and the reconstruction, performing a spatial correlation for instance. A short discussion of the robustness of the spatial structure displayed in Mann et al. (2009) should be included too as it is an important element to judge of the realism of the models behavior.

RESPONSE:

This comment has been raised by both reviewers (see R#2, comments #13 and #15) 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)*.

Regarding the comment of the reviewer about the robustness of the Mann et al. (2009) reconstruction (raised also by Reviewer #2, comment #15), 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 reconstruction~~s~~ ~~are~~were to be considered reliable, two possible explanations ~~are~~could be suggested for the aforementioned model-data discrepancies~~results shown in Figure~~ **??**.*"*

REVIEWER #1 COMMENT #19:
Page 4036, line 22. The cooling in CNRM model should be briefly mentioned here.

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 #2 (see Comment #14).

REVIEWER #1 COMMENT #20:
Page 4037, section 6. I personally do not like the term ”paleo transient climate respons” as this may give the feeling that only one ”paleo” response is possible while I am sure that the values would be different if one looks at changes during the deglaciation or the Holocene for instance. I would thus prefer if the authors do not introduce a new term here and simply describe their results as the correlation between forcing and temperature without proposing a new concept which is not general enough to my point of view and thus confusing.
This correlation should also be dependent on the timescale of the forcing. It is the reason why I am not convinced by the discussion at the end of page 4038. The correlation is indeed performed on instantaneous fields, if I understand well, but the influence of the forcing at one particular time will be the sum of the instantaneous response plus of the delayed one. If a forcing acts in a similar way during decades, the response will thus be larger than the one of another forcing that has higher frequency changes. The analysis proposed induced then for me a mixture of response at different time scales. It does not mean that it is not interesting but this must be clearly discussed in the text.

RESPONSE:
We understand that the reviewer may feel uncomfortable with the term ”paleo transient climate response”. Using the word ”paleo” makes it too general and not appropiate for our approach since it would be different for other timescales (e.g. glaciation). We have changed the term to ”Last Millennium Transient Climate Response” (LMTCR) that refers specifically to the timescales considered in this work. We still consider valid to refer to it as Transient Climate

Response since it is a measure of the rate of change between the temperature and the total equivalent forcing in transient simulations, which specifically focus in this case in the last millennium.

Regarding the second point of the reviewer, the calculation of the LMTCR is based just on a linear regression between the total equivalent forcing and the temperature simulations or the reconstructions. Therefore, the estimated values for the rate of temperature vs. forcing changes are based on linear correlation. Thus, the resulting values obtained from linear regression depend on the temperature variability accounted for (i.e. squared correlation) by forcing changes. This works at all timescales in as much as temperature and forcing changes can be linearly related. This fact represents in itself an advantage and a disadvantage. The advantage is its simplicity and the fact that a large part of the variability can be attributed to a linear response. The disadvantage is that it does not account for non-linear delayed responses of the system.

REVIEWER #1 COMMENT #21:

Page 4037, line 19. What is exactly meant by "major levels of forcing"?

RESPONSE:

The idea we wanted to convey was that the largest changes in the total external forcing between the MCA and LIA, tend to correspond with large changes in the simulated temperatures.
We have modified this sentence and hope the reviewer finds it clear now *(Section 6, paragraph 2)*:

"The use of this metric as a measure of consistency between reconstructions and simulations is motivated by the findings reported throughout this manuscript, to wit: (i) the broad agreement between the TEF applied in each simulation and the simulated temperature response (Figs. 1, 4, 5); and (ii) a tendency for the simulated temperature changes to cluster according to the ~~major levels of forcing~~ different magnitudes of change in forcing (Fig. 6), despite the presence of a substantiated influence of internal variability."

REVIEWER #1 COMMENT #22

Table 2, line 1 of the caption, ”forcing” is missing after ”natural”

RESPONSE:

Done

3 Conclusions

Please, delete conclusions

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