

Reply to
2nd Reviewer

*Russo, E., Fallah, B., Ludwig, P., Karremann, M., and Raible, C.C.:
The long-standing dilemma of European summer temperatures at the
Mid-Holocene and other considerations on learning from the past for
the future using a regional climate model*

Dear referee,

thank you very much for accepting to review our manuscript and for the time you dedicated to its revision.

Below we go point by point through your technical corrections, presented in *italic*, detailing how we dealt with your concerns reported in **Bold**.

Sincerely,

Emmanuele Russo

General Comments

This paper presents the results of a set of simulations performed with the COSMO-CLM limited area model, with a focus on Europe and the Mid-Holocene (MH) climate. The main objective is to shed light on the question why climate models produce warmer summer conditions in Southern Europe relative to the preindustrial (PI), in contrast with pollen-based reconstructions that suggest cooler conditions. This is a highly relevant issue in (paleo)climatology, as it is important to know to what extent climate models can reproduce climate's sensitivity to a change in radiative forcings. The first step taken by Russo et al. was to investigate the sensitivity of the results to different model configurations. For this purpose, 30 experiments were performed for PI and MH with different parameter values and perturbed model physics (so 60 in total). The results of these experiments were rather similar, suggesting limited sensitivity. Therefore, it is unlikely that the noted model-data mismatch is related to the set of parameters used. As a next step, short experiments were performed for the MH with prescribed different soil moisture contents in spring. As expected, more humid soils produced lower surface temperatures during summer, in closer agreement with proxy-based reconstructions for Southern Europe. The authors suggest that possibly the

use of more complex soil schemes in models could result a longer retention of soil moisture during summer and thus lower summer temperatures. A final analysis concerns an evaluation of the model performance in the 60 different experiments, showing that the optimal performance is different for different variables and periods. This highlights that a model setup that is performing well for the present day, is not necessary producing an optimal performance for the past or the future.

I expect these results to be of interest for paleoclimate modelers, making this potentially a useful contribution to the literature. However, in my opinion, the manuscript requires a substantial revision before it can be accepted for publication. My main concerns are the following:

- *A more balanced discussion of the MH summer temperatures in Southern Europe is required. The study assumes that the pollen-based reconstructions are correct, but it is important to make clearer to the reader that there are other proxies that agree with what the models show.*

Here we agree with the referee that more information should be provided about what other proxy-based reconstructions show for European summer temperatures at the MH. It is true that from the way we structured the text it might result to the reader that we think that pollen-based reconstructions are the only correct answer. We aim to revise the text accordingly, trying to provide a more balanced overview of the status of research on the topic. At the same time, though, we believe that the same attention should be paid in the literature to the fact that models have different answers. For this we take a chance here to make it clear that our goal is not to discuss which proxy-based reconstructions are more reliable. On the contrary, playing with a climate model physics and configuration, we want to test the plausibility of some of the hypotheses that were proposed in the literature for explaining the picture drawn from pollen-based reconstructions. These could work in principle in both ways, for either supporting or denying the evidence derived from them. This is a quite an important point deserving more attention, since model uncertainties are not always exhaustively considered in a paleoclimate context. We believe that our manuscript could be helpful in this sense.

- *A few additional experiments with a more sophisticated soil scheme should be performed to make the conclusion of Section 3.2 much more convincing.*

We do not totally agree with this referee's comment. We think that our current experiments already provide an important contribution to the research on the topic. We therefore decided not to perform additional experiments in this regard. Acknowledging that this is a very important point, though, we provide a very detailed and robust motivation for our choice in the corresponding answer to the specific comments of the referee below. At the same time, following the referee's comment, we realized that our conclusions should be carefully reviewed in case there are too strong statements that we cannot exhaustively demonstrate, given the outcomes of the presented experiments.

- *Not all figures and tables provide additional value to the text and could thus be omitted, especially Figure 6 and 7, and Table 4. A section should be added to part 3 to discuss the results against earlier modelling studies on the MH climate in Europe.*

We address the comments to each individual figure and table more in detail in the specific comments section. We agree that figure 6 could probably be moved to the supporting material section, as well as table 4, and we will consider to do so in the next version of the manuscript, if opportune. However, we really like and think that Figure 7 should be part of the main text. Beside acknowledging the possibility of expanding the discussion on the results against previous MH modeling studies, we do not think that an additional section would add value to the presented analyses. We will motivate this choice, providing more details on the way we aim to address this comment as exhaustively as possible in the specific comments section below.

Further details are provided below.

- *P. 1, line 6. I propose already mentioning here what RCM is used in this study.*

We will correct this point accordingly to the referee's comment.

- *P. 2, line 1-2. Please explain the different variables (T2, PRE, TCLC)*

We will remove the acronyms of the variables in the abstract, simply saying that we consider different variables.

- *P. 2, line 21. Please note that only pollen-based reconstructions suggest cooler summers in S Europe, and there is a good explanation why, see for instance Samartin et al. (2017).*

As said above, following the referee's comments, we realized that we need to make it clearer to the reader that there are other proxies that show something different. The text should be modified accordingly.

- *P. 2, line 23. Here references to other climate modelling studies should be included. For instance, Strandberg et al. (2014).*

We will add more references here, including the one proposed by the referee.

- *P. 2, line 26. "... overall simulated warmer conditions, that finds no continental analogue in the proxies". This is incorrect. The cooler summer conditions at MH are primarily suggested by pollen-based reconstructions. Other proxies (e.g., chironomids, glacier records) do provide evidence for warmer summer conditions during the MH (e.g., Samartin et al., 2017). This point should be discussed more clearly here. It is understandable that the extensive temperature reconstruction of Mauri et al. (2015) is used to evaluate climate models, but it is important to realize that there are also proxies that suggest a contrasting result for summer conditions during the MH. So, there is an alternative explanation to the model-data mismatch, i.e., that the models are correct and the reconstructions are wrong.*

We agree with the referee on the fact that this period should be reformulated, making clearer that the picture is highly debated in

the proxy community. At the same time, though, we take a chance here to emphasize again the importance of realizing that models also suffer from uncertainties and they are far from correct. This is true for the present day and, very likely, also for the past. We hope that our results could give an important contribution in this sense, highlighting the need of carefully taking into consideration possible models' misbehaviour.

- *P. 2, line 29. Please clarify the dipole mentioned here.*

Here we refer to the dipole structure of summer temperatures over Europe derived from pollen-based reconstructions. We will make this clearer in the new version of the manuscript.

- *P. 3, line 9. Please provide more information on the overestimation of the summer temperatures. Overestimated by how much?*

The overestimation changes for different models and areas. In some cases it exceeds 5°C [Russo and Cubasch, 2016]. We will add such information in the new version of the manuscript. We will add here also references for additional studies highlighting RCMs common warm biases for summer temperatures over the Mediterranean region such as: Christensen et al. [2008] and Boberg and Christensen [2012]. Finally, we will also add references for CMIP5 and newly available CMIP6 experiments with GCMs, usually showing a similar warm bias in summer temperatures over the Mediterranean region for the present day [Carvalho et al., 2021, Cattiaux et al., 2013].

- *P. 3, line 14. Is this hypothesis really tested in this manuscript? I would rather say "evaluated".*

We agree and will correct the text accordingly.

- *P. 3, line 22. "However, this is just an assumption, since there is no guarantee on whether the best model configuration for the present will*

be the same for other periods of time characterized by different forcing.” In addition, different parameter sets can produce similar present-day mean climates in agreement with observations, but with different sensitivities to radiative forcing perturbations. See for instance Loutre et al. (2011, Clim. Past 7, 511-526). I propose to mention this here.

We agree and will mention this point in the new version of the manuscript, adding relevant references on the subject, including the one proposed by the referee.

- *P. 3, line 24. Please explain RCM and GCM.*

We will provide the full explanation for the two acronyms here.

- *P. 4, line 2. Please clarify what “two methods” you mean.*

The methods we refer to are calibration versus the generation of Physically Perturbed Ensembles (PPEs). Following the referee’s comment, we will modify this sentence in the new version of the manuscript, trying to make the distinction between the two methods clearer.

- *P. 4, line 6: I suggest providing references and an explanation of the COSMO-CLM acronym where it is first mentioned, so here, instead of in Section 2.1*

We agree and will modify the text accordingly.

- *P. 4, line 22. I suggest mentioning here the calibration approaches to be assessed.*

Here we wanted to refer to calibration approaches in a general sense and not to a specific method. Following the referee’s comment, we propose to review this part of text in the new version of the manuscript. We will try to clarify here that our goal is to test the stationarity assumption proper of calibration methods used for regional climate models.

- *P. 4, line 9. The sentence starting with “Acknowledging the findings...” complex and hard to read. Consider revising.*

We agree with the referee and will revise this part of the text accordingly.

- *P. 5, line 3. Please note that although obliquity is an important astronomical parameter, it does not describe changes in the Earth’s orbit around the sun as suggested in the manuscript.*

We agree and propose to modify this part accordingly in the new version of the manuscript.

- *P. 5, Section 2.2. I propose to mention information (including the resolution) on the ocean model here as well. In addition, I suggest mentioning here that the same values for astronomical parameters and greenhouse levels are used in the driving GCM and COSMO-CLM.*

We agree and will include all the missing information highlighted by the referee about the configuration of the GCM in the new version of the manuscript.

- *P. 5, line 32. On what is “the reference configuration” based? Please explain.*

We actually provided very detailed information on the reference simulation at the end of subsection 2.1 of the former version of the manuscript. We preferred to have the description of the reference simulation in subsection 2.1 rather than in 2.3, since this is the reference (the baseline from which all other experiments are conducted) for all the experiments proposed in the different subsections. Therefore, we think that it would be a very legitimate choice to keep the same structure for the description of the reference simulation in the new version of the manuscript.

- *P. 5, line 31. The text mentions 31 experiments, but Table 3 shows 30 different experiments. Is the 31st experiment the reference run?*

Yes. We will try to make this clearer in the text.

- *P. 6, line 19. I do not really see the rationale behind the experiments described in Sections 2.4 and 3.2. As discussed on page 9 (line 10), it is already known for a few years that, when using the default set up, COSMO-CLM has problems retaining the spring soil moisture and that this results in dry soil conditions in summer and anomalously high surface temperatures. So, there is really no need to show that again here. Davin et al. (2016) solved this by applying a more sophisticated soil scheme that performed much better. So instead of the experiments discussed in Sections 2.4 and 3.2 I would suggest evaluating the more complex soil scheme of Davin et al. (2016) for the MH climate. I therefore propose that the authors perform additional experiments with MH conditions and the soil scheme of Davin et al. (2016) and to show the results in figures that replace current Figure 6.*

The experiments of section 3.2 are not aimed at showing that COSMO-CLM has problems in retaining spring soil moisture, as the referee suggests. The main goal of the proposed experiments is to test the plausibility of the hypothesis that soil-atmosphere interactions could be responsible for the bipolar behaviour of summer temperatures over the Mediterranean region at the MH, as evinced from pollen-based reconstructions, eventually shading more light on the possible reasons for model biases. This hypothesis has been often suggested in the literature, without being effectively tested in practice. In this context, we believe that the presented experiments are very useful, confirming that there is a strong sensitivity of summer temperatures, in our model, from the soil moisture available in spring, in particular over areas such as the Balkans and North of the Black Sea. For these areas we can affirm with our results that more attention should be paid when comparing model results against proxy-based reconstructions. In fact, even though our general results (Fig. 3 and Fig. 4 of the former version of the manuscript) fit very well with the results of previous modeling studies, and they would be in total agreement with the outcomes of some of the most recent study on the subject [Samartin et al., 2017], the same model presents very disparate outcomes as a result of perturbed spring soil moisture, leaving space to different interpretations. This is an important outcome

of our work, based on the presented experiments, that must be properly acknowledged in future studies on the subject. As for COSMO-CLM, the mentioned bias in summer temperatures over Southern Europe characterizes a large set of regional and global climate models for the present day. The reason for this bias is often suggested to be related to a poor representation of soil processes in the model [Christensen et al., 2008, Boberg and Christensen, 2012, Carvalho et al., 2021, Cattiaux et al., 2013, Kotlarski et al., 2014, Russo and Cubasch, 2016]. Nevertheless, this is something never carefully considered in Mid-Holocene studies for Europe such as the one of Samartin et al. [2017], where the models are considered as generally "correct". We are sure that our experiments could give an important contribution in this sense, highlighting the need of properly considering the skills of the soil component of climate models applied to the study of European MH climate, when interpreting their results. Concerning the comment on the need to show COSMO-CLM misbehaviour in depleting moisture in the soil too quickly, we thought that it was important to provide an explanation to the reader on why, when reducing soil moisture, there is a clear linear trend of the response in summer temperatures, while in the case of wetter soils the response looks very similar in all of the considered cases. These analyses are important in our opinion, since they demonstrate that more complex soil schemes are necessary for the study of MH summer temperatures. Or, more precisely posed, we show that models with low skills in retaining spring soil moisture should be dismissed when investigating summer temperatures at the MH over Europe. In fact, even if a model would present wetter soil in spring, as a consequence, for example, of enhanced late-winter/early-spring precipitation over southern Europe at the MH, its effects on summer temperatures would be lost if the given model had low skills in retaining soil moisture during spring, such as in the case of COSMO-CLM using TERRA_LM. This is something that was never acknowledged before in the literature and represents a fundamental milestone for future and previous studies, again giving weight to our experiments. Since large differences are expected in near surface temperatures depending on soil moisture conditions in spring, as suggested by the referee some lines above, we hope that our results could set the basis for new investigations with models including more complex soil-schemes. For all these reasons we think that

our work deserves publication and no additional experiments using different models are needed. At the same time, we believe that the sensitivity of climate models to soil-moisture perturbation needs to be better assessed, but in a larger context, making use of a plethora of climate models of different complexity. This is beyond the objectives of our paper. This said, we also have to acknowledge that it might not be true that simply considering more complex soil models would lead to the solution of the problem, as we stated in the former version of the manuscript. Even if we demonstrate that soil-atmosphere interactions could have a large impact on summer temperatures over Europe, this would surely not be the only condition for a good match with pollen-based reconstructions: as seen some lines above, a model producing wetter late-winter/early-spring conditions would still be needed. We realize, following the referee's comment, that our conclusions are in this sense too strong and that they need to be revised accordingly.

- *P.* 6, line 28. Please provide a bit more information on `TERRA_LM`. What are the 8 soil types for example, and how do they differ? How is the moisture holding capacity of these soils established? Are the soil types and their characteristics fixed during this study?

This was a point also highlighted by the first reviewer. The soil classes of `TERRA_LM` are: ice, rock, sand, sandy loam, loam, loamy clay, clay and peat. The model is uni-dimensional and the soil type for a grid box is the same over all considered vertical layers. 15 different soil parameters such as the pore volume, field capacity and the plant wilting point are fixed for each soil category and prescribed to the model. A table with all the values of the given parameters for each soil class is provided in Doms et al. [2013] and Guillod et al. [2013]. We will provide more information about `TERRA_LM` and the different soil types in the new version of the manuscript, together with corresponding references.

- *P.* 6, line 30. The experiments are initialized with 50% relative soil moisture, and then the soil moisture is decreased or increased by 25, 50 or 75%. To avoid confusion, please explain what these percentages

mean exactly. For instance: obviously, the initial 50% soil moisture content cannot be reduced by more than 50% if the percentages refer to the same reference soil moisture content. Presumably the initial 50% refers to the volume dictated by the soil water holding capacity, and the 75% refers to “75% of this 50%”, but this is not clearly explained.

We agree with the referee and will provide more detailed information on what the given percentages exactly mean in the new version of the manuscript.

- *P.* 7, line 16. Why not assigning all points in the domain? Why leaving some points out? Please explain.

In CORDEX (e.g. Kotlarski et al. [2014]) as well as in calibration studies such as the ones of Bellprat et al. [2012a,b, 2016], when applying a regionalization, not all of the points of the domain are normally considered (the number of considered points is even lower than the one used in our study). In the light of this ”modus operandi”, we deemed it suitable to apply the given domain subdivision. This choice is also supported by the fact that we actually conducted an analysis for each land point of the domain in the first step of section 2.5 (with similar conclusions). Moreover, the selection of the sub-domains introduces additional uncertainty to the ranking of the different simulations, with likely the same conclusions that could be drawn from other selection choices. Based on the above considerations, we will try to provide more information on the reasons for our choice in the new version of the manuscript.

- *P.* 8, Figures 3 and 4. I suggest using a different color scheme for Figure 3. Light red for cooling and green for warming is rather unconventional and could be confusing to the readers. In addition, I propose making the difference between Figures 3 and 4 a bit clearer. As I understand it, Figure 3 shows the mean of the anomalies of MH minus PI for experiments with the same parameters sets. Figure 4 then shows the spread around the means shown in Figure 3. Are the spreads more or less normally distributed around the mean?

We agree with the referee on the need to use a different color scheme for Fig.3, as well as making the differences between Fig.3 and Fig.4 clearer in the new version of the manuscript. At the same time, to answer the referee's second question, we include here the same figure for the ensemble spread of summer temperatures MH-PI differences (Fig.4 of the former version of the manuscript), showing in addition a point in correspondence of the grid box where the different ensemble members can be assumed to distribute normally around the mean, according to the results of a Shapiro-Wilk test of normality at a significance level of 0.05 (Fig. 1 of the current document). As you can see, for a large part of the domain the distribution of the ensemble members around the mean can be plausibly approximated with a normal distribution. Despite these results, we want to state here that the information on the distribution of the ensemble members around the mean is not relevant for the purposes of our study. In fact, the analyses presented in Fig. 4 were designed in order to see whether some of the ensemble members differ remarkably from the others, in terms of the differences in mean summer temperatures between the MH and PI periods. For this purpose, we think that Fig. 4 of the former manuscript version, showing the differences between the two most extreme members of the ensemble for each point of the domain, is entirely appropriate. Following the referee's comment, though, we realized that the description of the figure should be revised and that another term (for example simply "maximum absolute differences") should be used in this case instead of the "spread".

- *P.* 9, Figure 7. I wonder what the added value of Figure 7 is. I can see that the simulation with the best performance (nr 2) for PI has a slightly larger deviation from the reference run in MH, and for MH another simulation than nr 2 is closest to the reference run (i.e., simulation 26). In my view, this can be described in the text without showing the figure.

We actually do not agree here with the referee's comment. We think that figure 7 nicely supports the information provided in the text, giving a good example of the behaviour of the ensemble members in the different cases.

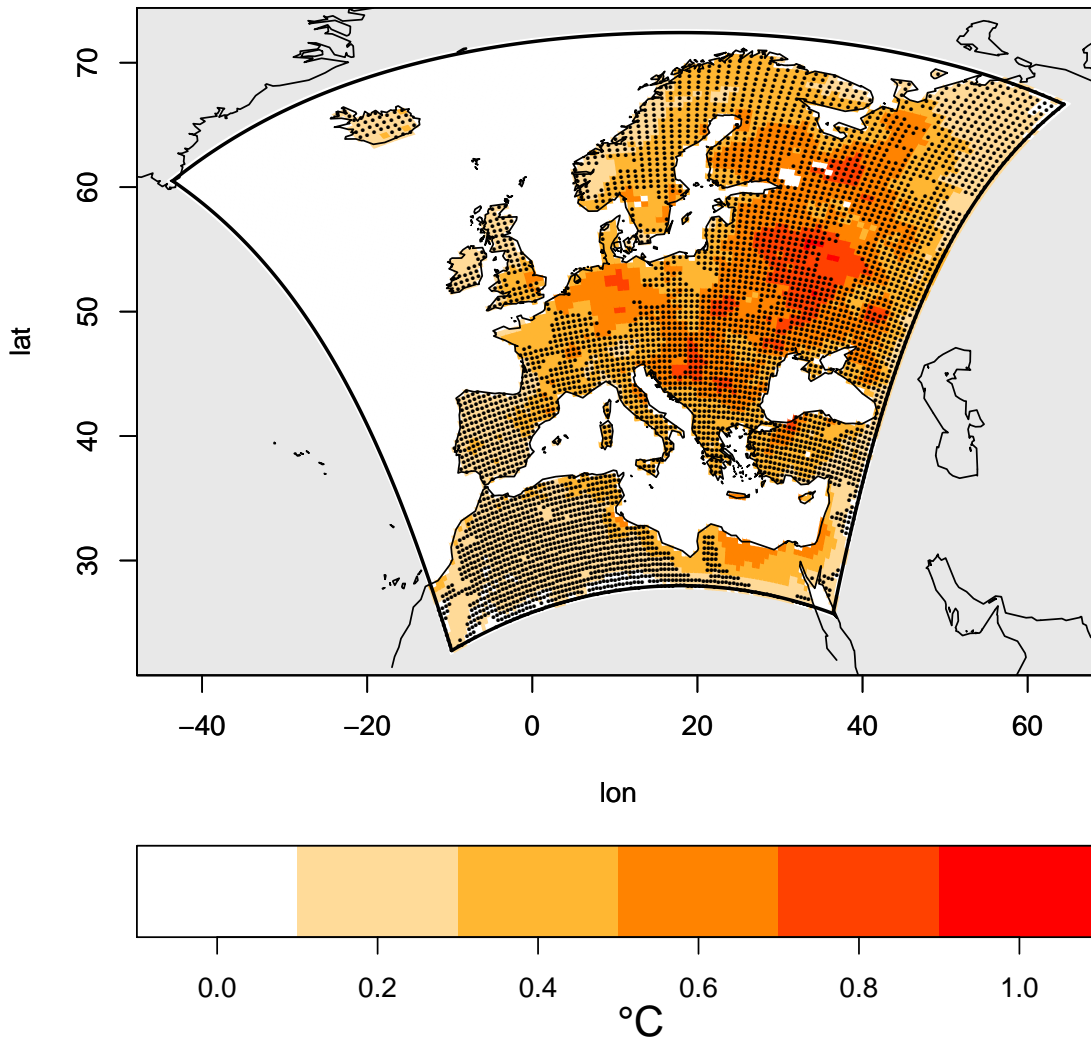


Figure 1: Maximum absolute differences in summer mean temperature MH-PI anomalies, calculated between the different ensemble members. The dots represent the points for which the distribution of the considered ensemble can be approximated by a gaussian distribution, according to the results of a Shapiro-Wilk significance test at a significance level of 0.05.

- Section 3.2. In this paper, it is suggested that climate models may simulate too warm summer conditions during the MH because of inadequate representations of soil processes and the inability of models to retain soil moisture during summer. Indeed, Figure 5 shows that summer temperatures are reduced when additional soil moisture is artificially added in MH simulations, which would produce a better match with pollen-based reconstructions in Southern Europe. However, what is not discussed here, is that, at the same time, more humid soils decrease the modeling performance in the northern part of Europe, since here the “standard” result with warmer summers during the MH is in good agreement with proxy-based reconstructions. So, with an increase in soil moisture, summer temperatures would also be reduced here, which is definitively not improving the match with proxies. Consequently, in Northern Europe enhanced soil humidity would not provide a solution. This point should be discussed in Section 3.

We want to emphasize again here that the hypothesis that models simulate warmer summer conditions over the Mediterranean region as a consequence of inadequate representations of soil processes and their inability to retain soil moisture is something not proposed in this paper, but in other studies such as the one of Russo and Cubasch [2016] and Bonfils et al. [2004], based on similar considerations on present-day biases of climate models. Here, with our experiments, we simply try to test the plausibility of such hypothesis. As already mentioned, with our results we actually prove that pronounced regional differences in European summer temperatures during the MH might be related to soil-atmosphere interactions: a strong spatial sensitivity of summer near surface temperatures to spring soil moisture perturbation is evident for the considered model. This sensitivity must be taken into account when discussing the results of climate models against proxies: over specific areas, it is more likely that models’ results would change as a consequence of a perturbation in spring soil moisture conditions. This means that all the points of the domain for which this sensitivity is high, independently from whether they are in the North or in the South, should be treated carefully in the comparison against proxy-reconstructions. This is true also for the points already showing a good match with proxy-based reconstructions. Following the referee’s comment, we will try to better assert the

latter point in the new version of our manuscript.

- *Section 3. Discussion:* I suggest comparing the obtained results here with reports from earlier modelling studies on the MH climate in Europe and to include a discussion of the impact of lateral boundary conditions on the results presented in this study.

Following the referee's suggestion, we will try to expand our discussion in the different parts of section 3, with a broader consideration of former modeling reports for the MH, whenever possible. Concerning the effect of the boundaries, this point was also highlighted by the first referee and we agree that this is a point requiring more attention throughout the text. The selection of the boundaries plays in fact a very important role for RCMs, being for several domains one of the dominant drivers [Sørland et al., 2021]. Hence, in the new version of the manuscript we will try to include a discussion on the role of the boundaries on the presented results, as suggested by the referee.

- *P. 10, Table 4.* I suggest omitting Table 4 and simply to describe the information in the text, since the numbers in this table provide little additional value, while the table takes up a lot of space. Besides, the numbers in the table just show the ranking, which does not necessarily provide information on the MAE value of MH versus PI. For example, for T2M, experiment 14 has 2nd rank for MH, but first rank for PI. But this doesn't necessarily mean that the MAE for MH is lower than the MAE for PI, it just means that there is another experiment that has a higher rank for MH (i.e., 29).

We will consider whether to include table 4 in the supplements, as suggested by the referee. However, concerning the second part of the comment, we want to emphasize that our goal is not to show that the MAE for the same experiment is different in the two periods. Rather, we want to show that the ranking of the experiments based on the MAE changes in the two periods. Therefore, we think that the information provided in the table is very appropriate for the achievement of this goal. Following the referee's comment, we will anyway review the text, trying to make our goals clearer.

Technical corrections

- *P. 2*, line 22. “proxy-reconstructions” should be “proxy-based reconstructions”.

Thanks. We will correct it accordingly.

- *P. 4*, line 8. Should be “feedbacks” instead of singular feedback.

We agree and will correct the text accordingly.

- *P. 4*, line 15. I suggest using “investigating” instead of “supporting”

We agree and will correct the text accordingly.

- *P. 5*, line 18: one bracket “)” too many

Will be corrected.

- *P. 5*, line 20 should be “is applied”

Here we are referring to the radiative transfer scheme, to `TERRA_LM` and to the ice scheme together. Therefore we think that “are applied” is a better choice than the one suggested by the referee.

- *P. 6*, line 2: should be “A set of parameters is” and “affects” on the next line

Following the referee’s comment we will revise this part of the text, correcting the suggested mistakes.

- *P. 6*, line 12: should be “from one of the reference simulations

We do not agree here. The PPE is built perturbing the configuration of the reference simulation discussed in the lines from 12 to 22 of page 5. Following the referee's comment, we will anyway review section 2.3, trying to make the description clearer whenever possible.

- *P. 6, line 13: should be "is reported" (refers to "a list")*

We agree and will modify the text accordingly.

- *P. 7, line 22. Should be "in both formulas".*

We agree and will modify the text accordingly.

- *P. 14, lines 29 and 35. The DOI's of Jungclaus et al. 2012a, 2012b, 2013 are provided two times.*

Thanks for highlighting this error. We will remove the duplicated DOIs.

- *Figure 2. The colours of number 7 and 8 are not easy to distinguish. I suggest to adjust these colours.*

We agree and will modify the figure following the referee's comment.

- *Figure 4. Caption: should be "mean"*

We agree and will correct the text accordingly.

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