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Interactive comment on "Mid-to-late Holocene Temperature Evolution and Atmospheric Dynamics over Europe in Regional Model Simulations" *by* Emmanuele Russo and Ulrich Cubasch

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Reply to
1st Reviewer
Mid-to-late Holocene Temperature Evolution and Atmospheric Dynamics over Europe in Regional Model Simulations by Russo, Emmanuele; Cubasch, Ulrich cp-2016-10

Dear reviewer,

Thank you very much for your effort in reviewing our paper.

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

Thank you.

- **General Comments**

  The paper can be broadly divided into three parts: i) validation of the simulation, ii) comparison with reconstructions iii) search for explanations of the disagreements. In all these three elements, I can identify caveats that in my opinion should be improved with different/complementary analyses. I have tried to review them in a comprehensive, yet constructive way, as detailed below. Besides the technical aspects, I think there is room in the manuscript for improvement regarding writing style. It was challenging for me to read and understand many parts of the paper. This is in part due to incomplete information in the captions and the main text, wrong labelling in the figures, and the misleading use of some concepts as "observation" or "validation". The internal structure in the paragraphs is confusing: paragraphs loosely connected, overly short, or in a misleading order respect to the panels in the figures. These issues add complexity that makes the lecture of the paper uncomfortable. Despite this rather negative view, I try to be constructive giving a list of points that develop the aspects that in my opinion can be improved in the manuscript. Note however that this list is not comprehensive.

We propose to develop a clearer structure of the manuscript as suggested by the reviewer. We will divide the manuscript in three main parts: the first based on the validation of the model configuration for present-days, the second one based on the comparison against proxy-data, additionally providing analyses and discussion on the advantages of the use of highly resoluted simulations for the comparison against reconstructions, and the third one in which we will provide explanations for possible mismatches. We also provide improved/complementary analyses accordingly to the referee's comments. In addition, we propose to correct the manuscript, keeping in mind that, as mentioned by the referee, the comparison against proxy data is by no means a validation. We aim at providing such comparison in a clearer way, cautious on the use of proper terminology. Additionally, as detailed in the comments below, we will try to improve the grammar of the manuscript and its presentation, in order to better tie together the entire discussion.

1. **Abstract/Introduction:**
   L1-3: In the first line, "is always been mentioned" is grammatically wrong. Despite that, it sounds a bit loose, almost sceptical. Is it an important factor or not? This ambiguous tone of the first sentence of the abstract is manifest through the whole manuscript. By the way, the authors do not make an attempt to demonstrate that this is indeed the case for these simulations. More on this below.

   We reformulated the sentence according to additional analysis we aim to provide within the text. In the revised version of the manuscript we will present a section in which we conduct a detailed comparison between the models at different resolutions and proxy-data, elucidating possible advantages of Dynamical downscaling. Further details on such analysis are presented in the comments to the second referee. We aim at implementing our discussion and the manuscript consequently, following a common suggestion of both the authors.

   L5-6: The paper is somewhat optimistic regarding the use of "for the first

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time". It is true that, as far as I know, there are no other set of time slide simulations. However there are various high-resolution simulations for the last millennium for Europe. Actually there exists at least one transient simulations for the last two millennia, in fact driven by the same ECHO-G run used by the authors of the manuscript. The authors should not ignore such previous, yet scarce efforts in this topic in the intro, but also the discussion of the results.

We agree with the referee and we aim at modifying the previous sentence. As the the referee mentioned, there are other paleo-simulations for Europe for mid-to-late Holocene. Nevertheless, such simulations often investigate only a time slice or do not cover the entire mid-to-late Holocene. Even if they do so, as the case of the ECHO-G simulation used for this study, their low resolution is often mentioned as one of the possible reasons for the disagreement between model results and reconstructions (Fischer & Jungclaus (2011);Bonfils et al. (2004)). With the previous sentence, we wanted to highlight the fact that no previous simulation exists for Europe, at such high resolution, covering different time-slices of mid-to-late Holocene. Our optimism, in this sense, regards the fact that these simulations could contribute in clarifying the debate on models and proxy disagreement. Additional discussion and more references (e.g. Strandberg et al. (2014),Schimanke et al. (2012),Braconnot et al. (2007a),Braconnot et al. (2007b)) will be added within the text.

L6: In line 6, "validation" is used in a wrong context. The model is validated normally against observations. But you can not validate the model looking at a reconstruction. Neither you validate a reconstruction looking at a simulation. You can only compare them, and try to gain insight through the disagreements. The use of "validation" in this wrong context is spread through the manuscript and should be avoided.

Accordingly to the referee's comment, we think that the terminology previously employed was incorrect. We aim at correcting the term "validation" with "comparison" here and throughout the manuscript, when referring to the comparison with proxy data .

I think at least the first four paragraphs can be safely merged.

We agree with the reviewer and propose merging such paragraphs, reformulating them in a more concise and clearer way.

L39-41: it is argued that then changes in solar irradiation were "negligible", and latter than "we expect that such changes would imply relevant variations...". It sound contradictory.

In this sentence our goal was to highlight the fact that during mid-to-late Holocene yearly variations in insolation, over northern latitudes in general, were neglicible when compared to the seasonal variations. The latest are expected to imply "relevant changes in the seasonal values of surface variables". We will re-formulate this period in a more comprehensive way.

L42-45: The paragraph in lines 42 to 45 is made out of a single sentence, which is too long. Still, the paragraph itself is short and can be merged with the former. Further, such sentence demands references.

We agree. We will reformulate the paragraph and join it with the former. We will also add further references (i.e. Cheddadi et al. (1997), Bonfils et al. (2004),Braconnot et al. (2007a),Braconnot et al. (2007b)).

In the paragraph starting in line 89, some examples of RCM simulations in palaeoclimate applications are outlined. It's strange to see that no simulation for Europe is referred. Examples of such simulations are: Gómez-Navarro et al. (2011, 2012, 2013, 2015a, 2015b) and Schimanke et al. (2012).

We agree. Apart from the ones suggested by the referee, we will also take into consideration, in the revised version, other works in which high resoluted paleo-simulations for Europe were performed (i.e. Strandberg et al. (2014);Renssen et al. (2001)).

2. **Model Validation:**

It is not clear how long is the control period used in section 3.1. The only hint is the label in Figure 2, 1990-2000. Is that the case? It should be clearly stated, not only in the main text, but also in the caption of the figure. Actually, the length of this period is CRITICAL for the model evaluation, a fact that is not acknowledged in the discussion of the results. A 10-year period of a GCM simulation is strongly populated with internal variability. Under this scenario, a comparison with observations is tricky. The model could be "by chance" going through a cold or warm phase, which would have a strong impact in the validation, at least in the way it has been established in the paper, focused on mean values. In this sense, the validation does not look at important aspects such as the variability. How is the variance reproduced by the model? I'm not sure due to the short length of the simulation, but it could make sense to look at the variability modes of temperature and precipitation.

As indicated by the referee, the control run is 10 years long and covers the period 1991-2000. We propose to add more informations on the length of the simulation throughout the text. We will also add such specification in the caption of previous Fig.2, Fig.3 and Fig.4. We are aware that the length of this simulation is CRITICAL. Unfortunately, due to computational reasons, we were not able to cover a longer period. We will now acknowledge such choice within the text. Additionally, realizing that we have not been properly explicit in the description of our experiment, we want to clarify that the regional simulation was driven by the

ERA-Interim reanalysis dataset and not by the GCM, as mentioned by the referee. We will implement our text accordingly, with the main goal of better specifying such technical details. Since the main goal of the present-day experiment is to test whether the changes applied to the model routine, for this particular case of study, allow to obtain reliable results in comparison to the outcomes of other studies, we think that the validation focusing on mean values is a useful tool in this context. Nevertheless, we also conduct now an analysis of model and observations variability that we aim to provide as supplementary material in the revised manuscript. Additionally, we now also consider the E-OBS dataset (Haylock et al. (2008)) as a benchmark for the validation of our results, and present the mean climatology of temperature and precipitation, for both winter and summer, as reproduced in the three datasets.

The new analyses are shown in Fig.1, Fig.2., Fig.3, Fig.4 and Fig.5. A more detailed description is provided in the captions of such figures. In addition to previous conclusions based on the bias of seasonal mean, the new analyses show that, the model is able to reproduce, with a certain degree of accuracy, the climatology of the observations. Additionally, the analysis of the standard deviation (Fig.4 and Fig.5) shows that the area with the larger bias are the ones where the model is not able to correctly reproduce the variability of the observations, in particular for precipitation.

We will replace the previous analyses of precipitation and temperature with the one presented in Fig.1 and Fig.2 of this text, and develop our discussion accordingly.

I do not think the choice of target for the validation is the best one. Using ERA Interim for the validation of precipitation in particular is a bad idea, since it is not constrained by precipitation observations, so there is no warranty that this dataset is bias free. I think it would be wiser to use the E-OBS dataset, which was developed specifically for the validation of RCMs

in Europe (Haylock et al. 2008).

We agree with the author that the ERAInterim Reanalysis is not the best choice for model's validation, in particular for precipitation. For this reason, as mentioned above, we now compare the model's results against the CRU observational dataset and the E-OBS dataset.

The way the similarity between the model and the observations is presented is a bit confusing to me. When the difference between two normally distributed variables is shown, the standard and intuitive approach, which steams from the application of the Central Limit Theorem, is to apply a t-test. The KS test is more suited for testing the shape of PDFs when the mean is know to be the same. For example if two dataset have the same mean, but different variance, the figure would show null bias (yellow colour here), but still the test would produce significant differences, which is misleading for the reader.

For the comparison of mean values we agree with the referee that a T-test is better suitable. We now perform the Student's T-test for the validation of the considered variables (Fig.1; Fig.2 and Fig.3).

I think the maps showing precipitation difference are not very useful. A difference of 5 mm/day might be huge or tiny depending on the mean precipitation. I think changes in precipitation are more meaningful when shown as perceptual deviations with respect to the mean.

We agree with the referee on the fact that changes in precipitation are more meaningful when shown as percentual deviations with respect to the mean. In the new maps (Fig.2), we now present precipitation biases as the percentual deviations from the observations values.

It is mentioned that the dots indicate grid where differences are "significantly not different". That's not exactly true. They indicate areas where

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Discussion paper

the null hypothesis of the data being sampled from the same underlying distribution could not be ruled out, i.e. where they are "not significantly different".

We agree. The previous sentence was not totally correct. We propose now to reformulate the sentence as follows: "the dots show the points where the null hypothesis of a Student T-test, at a significance level of 0.05, assuming that the data being sampled could be drawn from the same underlying distribution, is true".

I agree with the hypothesis used to explain the model deficiencies in Souther Europe regarding soil- atmosphere feedbacks. The particular role of these processes in RCM simulations in areas with strong water deficit was investigated in detail by Jerez et al. (2010, 2012).

We aim at proposing additional references as the ones indicated by the reviewer and listed at the end of this text.

L206: reads "These findings CONFIRM that... are MOST PROBABLY...". This is an example of doubtful and confusing sentence that should be avoided.

We agree. The previous sentence was doubtful as indicated by the reviewer. We will correct our sentence accordingly. Our analysis in fact confirms that model performances are influenced by its scarce capacity to reproduce soil-atmosphere exchanges correctly. This has consequences on both temperature and precipitation (particularly in summer when the biases are more pronounced) presenting a similar pattern of anomalies.

Maybe is worth to mention that generally the model skill resembles that identified in similar simulations for Europe (Schimanke et al. 2012, Gómez-Navarro et a. 2011, 2013).

We agree. Although a few works that generally propose similar model skills have been already considered within the discussion paper, it is reasonable to include

additional bibliography, that would help in strengthening our conclusions.

3. **Comparison with Pollen Reconstructions:**

   **As pointed out above, this comparison is by no means a model valida-
   tion. This should be made clear in the wording. As such, all sentences
   like "CCLM performs well" should be modified. The maps in Figure 5 are
   calculated as means with respect to which period?**

   We are aware of the incorrect terminology employed, as already highlighted be-
   fore. We propose to correct this period substituting the term "validation" with
   "comparison", being the pollen reconstructions not an observational dataset. We
   also aim at using better expressions in order to indicate the good or the bad
   agreement of the two datasets. We now modified Figure 5 of the discussion
   paper (also accordingly to the comments of the 2nd reviewer). In the revised
   manuscript, we aim at presenting the maps of the anomalies as represented in
   the two datasets, calculated for every investigated period with respect to the pre-
   industrial times. We also propose to accompany them with the corresponding
   maps of the pollen-based reconstructions uncertainties. Please refer to the 2nd
   reviewer response for further details.

   **In Figure 6, error bars are provided for the pollen data, but not for the
   simulation. I'm aware it is not easy to stablish them. However, such er-
   rors/uncertainties should not be neglected in the discussion of the results.
   The model has deficiencies that introduce systemic biases. But on top of
   then, there are non systematic biases introduced by unpredictable internal
   variability. This factor might lower or rise mean temperature in the simula-
   tion quite significantly, as pointed out by Gómez-Navarro et al. (2012) in a
   very similar scenario. Thus, this should be discussed at least qualitatively
   in this part of the text.**

It has been a choice of the authors not to include the model's uncertainty interval to the plots of Figure 6 of the discussion paper. Since the uncertainties of the 25 years CCLM simulations are way smaller than the ones of the proxy-reconstructions, even if their values need to be considered for the computation of regional means, we think that neglicting them, in this figure, was an appropriate choice.

Nevertheless, following the suggestion of the reviewer, we will add such considerations within the manuscript. Additionally, we will propose a new analysis of the trends of temperature in which the uncertainties are taken into consideration by means of a weighted least squares method. Further details are presented in the next point.

We also will add more discussion of model's uncertainties based on the results of Gómez-Navarro et al. (2012).

Many conclusions are drawn from Figure 6 regarding matchings of trends. I'm not sure at what extent such conclusions have any statistical significance, since in almost all cases the simulation lies within the uncertainty of the reconstruction. Having an almost perfect match between the reconstruction and the simulation is still perfectly possible within the range of uncertainty of the reconstructions.

Following the referee's comment, we realyzed that the computation of the mean and of the relative uncertainties presented in Fig.6 of the discussion paper should be re-performed. In particular, the plots we previously proposed and the conclusions we have drawn from them had no statistical significance. In fact, in a first place, we simply calculated the error as a mean of the provided uncertainty for every point. Realizing that this procedure is not correct, we tried to be more cautious with our analyses.

We present now new maps in Fig.6, representing the trends of seasonal means of 2 meters temperature calculated, for every grid box, by means of the weighted

least squares method. The points where the trends are not significant, according to a F-test at a significance level of 0.1, are additionally masked out. We provide again more details in the caption of the figure. We think that these maps are better suitable for our discussion. In fact, they are statistically more robust, allowing to consider trend and relative uncertainties for every grid box and time slice, resulting in a better suitable benchmark for the comparison against the pollen-based or other kinds of reconstructions. In the revised version of the paper we will replace Fig.6 and Fig.11 of the former manuscript with Fig.6 of this text.

Something I miss in this analysis here is the GCM simulations used to drive COSMO. I wonder how the ECHO-G and later ECHAM5 compares also with reconstructions. Is the RCM adding anything relevant to these simulations? If the answer is "certainly yes", then the use of the RCM is fully justified and the paper would gain interest. If the answer is "mostly no", it would be still interesting, since it would imply that the many GCM simulations available for the last millennia are still relevant at rather regional scales. I'm sure the PMIP community would be very interested in answering this question.

A similar comment has also been addressed by the 2nd reviewer. We refer to the answer to his comment as an exhaustive response to this point. As suggested by the referee, we think that answering this question would definitely strengthen the paper. In the revised manuscript we will add a section in which the possible advantages of highly resolved simulations for the comparison of change in 2 meters temperature against proxy reconstructions will be investigated. Also more analysis will be presented accordingly.

4. **Interpretation of Paleo Records:**

   Generally it was difficult to follow the arguments in this section. It would significantly help to label the maps as Fig 6b, Fig 6c, etc. and use such labels extensively through the manuscript. In this regard, the discussion of

the results starts with summer, whereas the first row shows winter. Small inconsitencies like this, although non critical for the scientific message, have a dramatic impact in the reading pace.

We agree. In order to make the manuscript more easily readable we propose to label the maps accordingly to the referee's suggestion. We will also correct the order of the seasonal analyses within the text.

The EOFs for MSLP are shown and used in the discussion. They are used to argue regarding NAO and SNAO, for instance. I'm not totally comfortable with that, since the NAO is defined as the leading pattern for a spatial window that is not that of the RCM. This explains in my opinion why the NAO pattern does not stand out as the leading mode in winter, and second mode in summer just "resembles" the SNAO pattern. I think a more orthodox approach would be to calculate the EOFs within the GCM, in a window that properly encompasses the North Atlantic. This is justified since the large scale circulation is fixed by the GCM, and thus the NAO simulated should be consistent with the climate variability within the RCM domain. Hence, such patterns could still be used to discuss about regional variability within the RCM domain.

We agree with the referee's comment. We now conduct the EOF analysis of MSLP anomalies of the ECHAM5 simulations in order to properly consider a spatial window that encompasses the entire North Atlantic region. We select the region in between 90W and 40E and in between 20N and 80N, as defined in **?**. This would allow us to infer about changes in the NAO and other atmospheric circulation patterns characteristic of this region. The results are shown in the attached Fig.6 and Fig.7. Since the RCM large scale circulation is "dictated" by the GCM, we reasonably think that such results can be used to argue about regional variability within the RCM. We propose to modify the discussion within the revised manuscript accordingly to the new analysis.

Line 255 reads "In summer the first EOF shows that the model reproduces similar conditions in atmospheric circulation between the mid-Holocene and pre-industrial times". I do not understand how that conclusion is drawn from the map in Figure 8.

We propose to modify the previous sentence accordingly to the new analysis presented above. Since the investigation area is different now, the results of the EOF analysis changed. Nevertheless, the time expansion of the principle components of the previously evinced pattern and its structure (representing now the the second mode of atmospheric variability during summer), mainly driven by changes in insolation, seems to be a proper product of this particular case of study. Even if it implies changes in circulation, we do not see any particularly prominent dipole structure characteristic of other well-known circulation patterns for the region. We aim at modifying the discussion within the revised version of the paper, being more cautious about arising risky conclusions as the one spotted out by the referee.

In page 8 the wording "observed" is used in various sentences, and it's not fully clear what is meant (most likely respect to the simulation, but it could also be the reconstruction). I think "simulated" is more appropriate and precise.

As highlighted in previous points we agree with the referee and propose to correct the sentence accordingly in the revised manuscript .

Some inferences about the "clearness" of the sky are made which are based in indirect evidence such as EOF analysis. I think it is not necessary to make such risky affirmations. We have direct information that can tell us exactly how cloudy the simulated climate was. After all, in the simulation we can check directly variables such as cloud cover, which give a direct measure of what is being argued. I would go for a direct measure whenever possible, as it is the case. Similarly, in the paragraph between

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lines 277 and 279 (and Fig. 11) the more pronounced positive phase of the NAO can be directly tested within the GCMs, rather than indirectly inferred through a map of temperatures.

We propose to modify the previous sentence within the revised manuscript, accordingly to the fact that, even if the SNAO shows a trend that in this case is positive troughout the mid-to-late Holocene, such trend is not significant and presents high variability. We propose to review our previous discussion and to avoid any conclusion on the trend of cloud cover due to the high variability of the emerged pattern throughout the investigation time, eventually presenting alternative analyses if necessary . As already mentioned, we also preferred to merge Fig.11 together with fig 6 of the discussion paper, considering also summer analysis.

Finally, I think there are more powerful statistical tools than the one used here to study the co- variability between temperature and MSLP. Canonical Correlation Analysis could be used to derive relations between the variability of MSLP and temperature, and it would produce a picture of such co-variability more robust that the one provided by maps in Figure 10, for instance. An example of the application of such a tool in a very similar context is Gomez-Navarro et al. (2015b)

We agree. We investigated the covariability of MSLP and temperature by means of Canonical Correlation Analysis and present the results of such analysis in Fig. 7 and Fig.8. We will also refer to the study of Gómez-Navarro et al. (2015b) as a good example of application of such method for the investigation of the relations between atmospheric variability and temperature.

5. **Comments regarding Figures Figure 1: The colour scale shows everything below 1000 meters as green. I think a palette with stronger contrast could be chosen.**

We improved the previous plot accordingly to the referee's suggestion. We add

the modified picture to this discussion and modify it within the paper.

Figure 2: The reference period should be stated in the caption. I think the limits of the palette can be adjusted to better span the range of temperatures.

We agree. We added the reference period within the caption and further details. We also provide additional analysis and improve the palette in order to better span the range of temperature.

Figure 3: The colour palette provides barely any contrast all. Everything is yellow in the maps.

We modified the plot accordingly to the previous point

Figures 4 and 5: Same comments as in former figures

Figure 4 has been adjusted accordingly to the referee's comment. Figure 5 of the discussion paper has now been modified. The new plots are presented in the response to referee number 2 (Fig. 2 and Fig. 3).

Figure 6: Please label panels as 6a, 6b, etc. I do not think using colour in the caption is an orthodox approach. Note that the caption does not agree with the order of panels. First row does not show North, but it is the first column which does, etc.

We agree. We now label the panels of the new map presented in Fig.6 as 6a,6b, etc., as suggested by the referee. We also avoid using colours in the caption. We also modified the order of the captions, accordingly to the figure.

Figure 7: I can barely see the numbers and labels in the figures in the right.

We enlarged this figure in order to make it more easily readable. Accordingly to a comment of the second referee, we propose to move this picture to section two of the revised paper.

Figure 8: I think the label with the loading can be moved to inside the maps. This would allow to put the maps closer together, which would allow to make maps larger and more readable. The latter comment can be applied to almost all figures.

We agree. We moved the loadings inside the maps. We also applied similar modifications to all the pictures in order to make them larger.

Figure 9: Please label panels to indicate which represent EOF1 etc. Where are the units? Either the EOF or the PC carries the units, in this case pressure. I guess they are included in the EOF patterns in Figure 8. If so, please label the palette accordingly.

We realized, following the referee's comment, that we were not precise in our previous discussion. Consequently, we propose to add further details in the caption of this figure. In fact, here, we do not indicate units since the analysis we conducted were based on values standardized with respect to the pre-industrial period.

List of Figures

Figure1: *Analysis of Winter seasonal means of **2 meters temperature** (left panel) and **Precipitation** (right panel) for the period 1991-2000. The first column shows the mean climatology for the investigated period as represented in the three considered datasets: the **CCLM** in the first row, the **CRU** in the second and the **E-OBS** dataset at the bottom. The second column presents the anomalies between the CCLM results and the respective observational datasets. The area with a point represent the grid cells where the anomalies between the two datasets are not significant, according to a Student's T-test, at a significance level of 0.05.*

Figure2: *As Fig.1 but for Summer.*

Figure3: *Biases of seasonal means of **Evapotranspiration** (left), **Latent** (center) and **Sensible Heat** (right) fluxes, between the CCLM simulations and the*

GLDAS dataset, calculated for the refernce period 1991-2000. As in the previous figures, the area with a point represent the grid cells where the anomalies between the two datasets are not significant, according to a Student's T-test, at a significance level of 0.05. Winter results are presented in the first row, and Summer results in the second.

Figure4: *Analysis of Winter variability of Temperature (left) and Precipitation (right) as simulated by the CCLM in respect to the observational datasets. Comparison against the CRU dataset is shown in the first row, while the one against the E-OBS dataset is presented in the second.*

Figure5: *As in Fig.4 but for summer Temperature and Precipitation.*

Figure6: *Mid-to-late Holocene temporal Evolution of 2 meters temperature seasonal mean. The maps show the slopes of the linear trends calculated, for every grid box, taking into consideration the uncertainties associated to the two datasets, by means of a weighted least squares method. The area masked out in grey, are the area where such trends are not significant, according to a F-test at a significance level of 0.1.*

Figure7: *Canonical correlation pattern pairs of MSLP (left) and T2M (right) in Winter. Each panel illustrates the percentage of variance explained by each pattern and the canonical correlation associated with the pair. The results are calculated for the mid-to-late Holocene, from 6000BP to Pre-industrial times. Note that the MSLP has been obtained directly from the driving GCM, since the window of interest lies outside the RCM domain. Both the variables are standardized with respect to the pre-industrial period, and the units represent the Variance explained by the different patterns.*

Figure8: *As in Fig.7 but for Summer.*

Figure9: *Time expansion of the principal components of the first and second EOFs of winter (**1st row**) and summer (**2nd row**) MSLP anomalies of the ECHAM5 (**lower row**) simulations, standardized to the pre-industrial period.*

Figure10: *Orography Map of the COSMO-CLM simulation domain in rotated*

coordinates.

Figure11: *(Left) Anomalies of zonal mean insolation on top of the atmosphere between pre-industrial period **PI** and 6000 years BP. (Right) Trends of December and June incoming Radiation on top of the Atmosphere.*

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With kind regards on behalf of the all authors,
Emmanuele Russo

Interactive comment on Clim. Past Discuss., doi:10.5194/cp-2016-10, 2016.

Fig. 1.

Fig. 2.

C23

Fig. 3.

T 2M

PREC

Fig. 4.

Fig. 5.

Fig. 6.

MSLP

T2M

R=0.62

37.94%

19.75%

R=0.65

14.74%

25.04%

Fig. 7.

Fig. 8.

C29

Fig. 9.

meters a.s.l.

Fig. 10.

Zonal mean Anomalies 6000BP-200BP   Mid-to-late Holocene Evolution at $30^o N$ and $60^o N$

Fig. 11.