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CPD

Interactive comment

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
2nd 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 **b**olt.

Thank you.

1. **Main Comments:**
   The grammar and spelling can be much improved. There are many long sentences that are hard to read. I have indicated a few below. I strongly suggest to have the text thoroughly checked by a native English speaker.

   We propose to improve the structure and the grammar of the paper in order to make it more easily readable. We also aim at shortening long sentences and express complex periods in a more concise and robust way.

   I propose to compare the results of COSMO-CLM to the results of ECHAM5. The latter results have already a relatively high spatial resolution (T106 or 1.125x1.25 degr) compared to previous GCM studies. This resolution is actually close the resolution of the reconstructions (1x1 degr). In the manuscript, the authors have regridded (up scaled) their regional climate model results from 0.44x0.44 degree resolution to 1x1 de- gree to make the comparison in Fig 5. It would be interesting to see to what extent the COSMO-CLM produces a better match. Is it, from a paleoclimate perspective, worth- while to make the considerable effort to nest the

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regional model in the high-resolution GCM results? Or do both models produces very similar results? In my view, address- ing these questions would strengthen the paper. To make room for such a comparison, Figures 2, 3 and 4 could be moved to the supplementary information, as these figures do not directly concern the core topic of this study (mid-to-late Holocene temperatures and atmospheric dynamics).

According to the IPCC (2007) report: "Paleoclimate data are key to evaluating the ability of climate models to simulate realistic climate change". In particular, since the details added by high resolution models can help in the interpretation of proxy data that are often influenced by processes taking place on smaller scales than the ones resolved in coarser models, they are considered a particularly suitable tool for paleoclimate studies.

Within this context, in our discussion we try to highlight the importance of using high resolution models, and in particular Regional Climate Models, for the simulation of past climate change. Aiming at investigating the value added by highly resoluted simulations for the comparison of near surface temperatures against proxy-reconstructions, we follow a two steps approach:

(a) Firstly, we conduct a qualitative analysis of the simulations performed with three models at different resolutions in order to detect visible differences in the reproduced signals.

(b) Secondly, we employ a quantitative approach in order to estimate the skills of the RCM, in comparison to the driving GCM, in reproducing the same changes in temperature during mid-to-late Holocene as derived from proxy-reconstructions.

As a benchmark for such comparison we use the pollen-based temperature reconstructions of **?** In this way we aim at establishing whether the representation of smaller scale processes and improved orographic features of the region of study, could lead to results that are in better agreement with the mentioned proxy-reconstructions.

In Fig. 1 we present the anomalies of summer and winter seasonal mean temperatures between 6000BP and the Pre-industrial period, as reproduced by the different models. From these maps we first notice as, in both the seasons, a similar signal of climate change is present for all the simulations. This is expected, beeing, in every case, the data constrained by the coarser resoluted models. Nevertheless, while the higher resoluted simulations allow to catch a warmer bias over Northern Europe in winter, also present in the proxy data, the ECHO-G does not show such behaviour. Additionally, the land-sea area in the ECHO-G is considerably different than the ones of the other models. Regions such as Southern Spain and the Black sea area, Italy and Scandinavia are partly or completely masked-out in this case.

Consequently, we reasonably suggest to focus further analyses on the comparison between the ECHAM5 and the CCLM results. In both seasons additional details are easily detectable in the CCLM pattern. The coastline is also better reproduced in this case, resulting in more suitable informations for possible comparison with proxy-data. Nonetheless, the CCLM shows better defined patterns as a consequence of higher resolution, being able to discriminate higher spatial variability.

In the successive step, we try to quantify how better the CCLM reproduces the reconstructed temperatures in comparison to the ECHAM5. Under the mentioned considerations, we use a similar approach to the one employed by Zhang et al. (2010) and based on the work of Goosse et al. (2006). After upscaling the RCMs results and interpolating the ECHAM5 ones on the reconstructions grid, we introduce a Cost Function defined as:

$$CF_{mod}^{k} = \sqrt{\frac{1}{n}\sum_{i=1}^{n} \omega_i{}^k (T_{rec,i}^{k} - T_{mod,i}^{k})^2} \quad (1)$$

where CF_{mod}^{k} is the value of the cost function for each considered time slice *k* of mid-to-late Holocene, and each model *mod* . The parameter *n* is the number of the reconstructions grid boxes, $T_{rec,i}^{k}$ the reconstructions temperature at every location *i*, while $T_{mod,i}^{k}$ is the correspondant temperature of the model simulation. The parameter w_i^k is instead introduced for considering the uncertainties of the reconstructions at every location and time period. Its value is given by:

$$\omega_i^k = \frac{1}{(\sigma_i^k)^2 + 1} \quad (2)$$

In this way reconstructions with higher uncertainties will contribute less in the calculation of the Cost Function. We neglicted models uncertainties since they are considerably small ($\sim 0.01^oC$) in comparison to the reconstructions ones. The values of the CF for the two models are provided in Tab.1 and in Tab.2.

As we can notice, even if not particularly large differences are present, the Cost Function computed for the CCLM is in almost all the cases lower than the ECHAM5 one. In particular the CCLM results are, in some cases, closer by almost 10$\%$ to the reconstructions. It is important to mention that the scale considered in our analysis is closer to the resolution of the ECHAM5 than the one of the CCLM. As suggested by Di Luca et al. (2015), given that the main difference between the GCM and the RCM is related with their horizontal resolution, it seems natural that the results depend on spatial scale of the analysis. Additionally, is key to state that the evinced results are relative to this case of study and other comparisons should be performed, considering different couples of RCM-GCM, in order to derive more robust conclusions on the suitability of higher resoluted models for the comparison against proxy-reconstructions.

Nonetheless, the motivation behind producing higher resolution climate simulations is not only related to scientific arguments of the type described above. From a different perspective, such results, due to the greater level of detail, could be preferable for applications in studies in which human adaptation or environmental response to past climatic changes would be investigated. The need for climate information at very fine scales, for application such as archaeology or vegetation reconstructions, hence constitutes a strong incentive to perform higher-resolution climate simulations (Di Luca et al. (2015), Rummukainen (2016)).

In conclusion, the evinced results and the proposed discussion, give us concrete motivations for the choice of conducting RCM simulations for this particular case of study. Nevertheless, we aim at keeping Fig.2, Fig.3 and Fig.4 of the discussion paper within the revised version of the manuscript, as representing a satisfactory test for the reliability of the chosen model setup, they could be suitable for other studies conducting paleoclimate simulations for the region.

In the new version of the manuscript we will add a section based on the presented analyses accompanied by detailed and pertinent discussion.

The left column of Fig 5 presents maps of the winter and summer temperature anomalies (model minus reconstructions), "averaged over all the mid-to-late Holocene time slices". It is not clear to me what the authors have actually done here. Have they first averaged the maps of the different time-slices for the model and the data, and then calculated the model-data anomaly? Or have they calculated the trend between 6000 and 200 BP in both model and data, and then made a map of the difference between the two methods? The caption suggests that they have applied the first method, but in my view this would only be meaningful if the anomalies are more less constant through time, which is clearly not the case (see

Figure 6). Since the trends from 6000 to 200 BP seem approximately linear in both model and data, it would make more sense to compare maps of these trends or to show maps for different time slices. Figure 11 actually shows linear trend maps for both the model and the reconstructions, but only for DJF. It is unclear to me how to relate Figure 11 to Figure 6. Figure 11 seems to indicate a pollen-based linear warming in Southern Europe of mostly less than 0.4âŮę C, while Figure 6 shows a warming trend for the pollen-based reconstructions of 1âŮę C for Southern Europe. In addition, the pollen-based cooling trend in Figure 6 of more than 2âŮę C does not match Figure 11 which shows a much smaller cooling trend. Is there an inconsistency between Figure 6 and 11, or have I missed something? Please clarify.

In the previous analysis, Fig.5 was obtained by simply averaging the anomalies over all the time-slices. The same procedure was also applied in order to obtain a map of the average uncertainties. Following the considerations of the referee, we realized that such approach was not totally correct and we re-performed our analysis consequently. In the new case, as shown in Fig.2 and Fig.3, we compute the seasonal anomalies of 2 meters temperature between the CCLM and the pollen-based reconstructions for every single period of time. We additionally provide, together with the anomalies, the respective pollen-reconstructions uncertainties. This choice is reasonable since the uncertainties maps could result useful in the interpretation of the mismatches arising between the two datasets.

Additionally, we are now considering a new approach for the investigation of seasonal trends. We recomputed figure 6 taking into consideration, this time, the uncertainties in both the datasets (for more specifications please refer to the first referee response). Here the new plots are similar to Fig. 11 of the discussion paper, showing this time both winter and summer trends. Only the area where the trends are significant, according to a F-test at a significance level of 0.1, are shown. Additionally, such trends are calculated by mean of a weighted least squares method, allowing to take into consideration, as said, the uncertainties of the two datasets. Since the changes in both the datasets are not homogeneous over the region, we think that these maps should be more appropriate than the previous ones based on regional means. We want to highlight, relatively to the referee's comment, that the new maps do not show values of changes in temperature. Rather they show the slope of the trend associated to every grid box.

The right column of Fig. 5 shows the uncertainties in the pollen-based temperature reconstruction. How were these maps constructed? According to Fig. 6, these uncer- tainties are not constant through time, so simply averaging the errors for the different time slices is not informative here either. Please clarify.

Please refer to the previous point.

For the summer in Southern Europe, the model and the reconstructions show opposite trends: cooling in the model and warming in the reconstructions. The authors provide an explanation for this model-data mismatch that is based on the warm bias of the model in S Europe due to the underestimation of evaporation in summer. However, the mismatch may also be explained by uncertainty in the pollen-based reconstructions in S Europe. Paleoclimate reconstructions based on pollen rely on the assumption that changes in the vegetation were driven by the parameter to be reconstructed (i.e.summer temperature). In the Mediterranean region, vegetation distribution is mainly limited by effective precipitation, rather than by summer temperature (e.g. Osborne et al. 2000). It would therefore be good to discuss the associated uncertainties in the methodology of the pollen-based reconstructions and to mention Holocene temperature reconstructions that are based on other proxies. For instance, summer

temperature reconstructions from the S Europe domain based on Chironomids, show a clear Holocene cooling (Heiri et al. 2015; Toth et al. 2015) that actually support the presented modelling results. In addition, Holocene SST reconstructions from the Mediterranean Sea show a similar cooling trend (e.g. Marchal et al. 2002). The discussion section should be extended accordingly.

The choice of the dataset of Mauri et al. 2015 has been done for several reasons. First of all, it allows to perform a comparison against model results over most of the simulations domain, considering different variables (even if we only focus on temperature in our discussion). Then, it covers exactly the same time-slices of our model simulations. No other dataset has this temporal and spatial coverage at such high spatial resolution. Additionally, the robustness of the data has been thoroughly tested, in Mauri et al. 2015, against other proxies (including chironomids, $\delta 18$ O from speleothems and lake ostracods, bog-oaks, glacio-lacustrine sediments, wood anatomy and other pollen reconstructions based on different reconstruction methods) leading to satisfactory results. Nonetheless, similar pollen-based climatic reconstructions have been extensively employed in other data-model comparisons, and, most recently, for the evaluation of the PMIP3/CMIP5 climate models included in the last IPCC report (Stocker et al. 2013, Harrison et al. 2015).

As the referee mentioned, different studies already criticized the use of pollen-based data for reconstruction of temperature over the Mediterranean region, claiming that the vegetation distribution is mainly limited by effective precipitation, rather than by summer temperature (e.g. Osborn et al. (2000); Renssen et al. (2009)). In response to such critiques we want to refer to a detailed comment provided by Basil Davis, and attached to this discussion.

According to the aforementioned reasons, and additionally supported by the explanations given by B. Davis in his comments, we think that the employed pollen-based reconstructions can be considered a very reliable source for the main goals of our paper.

Nevertheless, in accordance to the referee's comments, in the new version of the manuscript we will provide further discussion on the uncertainties in the methodology of the pollen-based reconstructions and specify more details on the reliability tests conducted by Mauri et al. 2015.

Since the comparison against independent and different proxies has already been performed by Mauri et al. 2015, we feel that such analysis could be omitted from our manuscript.

Additionally, the previous analyses of mid-to-late Holocene temperature evolution were misleading. In fact, simply considering regional means, they did not allow to have a proper overview of the trends at different locations, possibly resulting in a mismatch in the comparison against other proxies. The new maps presented in Fig. 4 show now a more heterogeneous behaviour, and are in better agreement with other independent reconstructions such as the one of Heiri et al. (2015), mentioned by the referee, for which summer temperatures over the Alpine region were characterized by a decreasing trend during mid-to-late Holocene.

In the discussion, the results should also be compared to other modelling studies that focus on the mid-to-late Holocene climate. Do the new results presented here confirm earlier findings? How do the seasonal trends and 6k-0k anomalies compare to that of other models (e.g., PMIP3)? What do other Holocene modelling studies say about changes in atmospheric circulation over Europe and the North Atlantic basin?

We agree. We propose to present, in the revised version of the manuscript, a section in which our results are compared against other studies. In particular, we will focus on the anomalies between 6000BP and the pre-industrial period, performing a direct comparison against the outcomes of 12 models from the PMIP3 experiment. We will compute the regional means for two regions over Northern and Southern Europe for al the datasets. We will include such values in two tables, attached to this discussion, that we aim to provide as supplementary material in the revised manuscript. The main features arising from such analysis are, a common positive bias over Southern Europe in summer, and the failure to properly represent winter anomalies in both the regions. We aim to implement and develop our discussion accordingly.

Conclusions: The conclusions should be made less descriptive / more quantitative. The paragraph starting on line 296 does not contain conclusions and can be removed. Please explain on Line 310 what atmospheric circulation configuration is meant here.

We agree. We propose to make our conclusion more quantitative. According to the new analysis presented here and as a response to the 1st referee, we aim at extending our discussion and develop our conclusions in a more concise and robust way.

2. **Minor Comments:**

Line 26: I suggest providing a more accurate definition of climate models

We agree. We will develop a more detailed description of the climate models. Nevertheless, we will try to be as exhaustive as possible, referring to their thecnical manuals for further details that would not be inherent in the discussion.

Line 34: "orbital parameters". I propose to use astronomical parameters instead, since obliquity is not a parameter of the Earth's orbit.

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We agree and will change the term "Orbital" in "Astronomical".

Line 37: Please rephrase this sentence, as it is not easy to read

We propose to rephrase the highlighted sentence accordingly to the referee's comment

Line 43: "solar forcing". Usually, "solar forcing" is used to describe changes in solar activity as opposed to astronomical forcing that reflects changes in insolation due to changes astronomical parameters. To avoid confusion, I suggest using astronomical forcing here.

Aware of the mistake, we will correct the term "solar forcing" with "astronomical forcing".

Line 46: In my view, this sentence does not introduce the reader to the paragraph, so I propose using a different topic sentence.

We agree. We propose to modify this part in order to better connect it with the following text.

Line 57: It is not clear to me what is meant by "hampered climate anomalies"

We reformulated this sentence. With "hampered anomalies" we wanted to indicate that, the improvement in the reproduction of soil water storage and heat fluxes by climate models, as suggested byStarz et al. (2013), could lead to a reduction of the biases arising from the comparison with observations. We agree with the referee that the former expression was somehow misleading and we will reformulate it in a clearer way.

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Line 60: typo, atmopshere

Corrected in atmosphere.

Line 60: "not being able to reproduce correctly the reconstructed data over the entire region". Please clarify. Was the model too cold or too warm? What was the bias?

We will extend the previous period with further details, referring to the results of Fischer & Jungclaus (2011). In particular, their results presented only a weak shift to a positive phase of the NAO at mid-Holocene in Winter, resulting in colder conditions over Northern Europe and warmer over Southern Europe with respect to the values of reconstructions. In summer, again, the signal seemed to be mainly driven by changes in insolation, resulting in homogenously warmer conditions at 6000 BP.

Line 63: Please rephrase the sentence starting at this line.

We reformulated the sentence accordingly to the referee's comment.

Line 72: " In many cases" What cases, please elaborate. The objectives of the paper should be explained more clearly. On page 3, two objec- tives are provided. The first objective is to "obtain a better interpretation of the new pollen database..." Why better? What problems have been encountered in the inter- pretation?

We agree. The objectives of the paper should be better explained. We try to do so also based on the referee comments and the additional analyses provided in this revision. Mauri et al. (2015) presented a possible interpretation of the anomalies evinced from their reconstructions between 6000BP and the pre-industrial period, mainly based on changes in atmospheric circulations. Supported by previous findings, we use our results and the entire mid-to-late Holocene time slices reconstructions of ?, in order to arise plausible interpretations. In particular, while for winter we agree with their interpretation of a more pronounced positive phase of the NAO at mid-Holocene, our findings support different interpretations for summer temperature behaviour. We will try to improve our discussion accordingly.

Line 105. This first sentence of Section 2 does not provide information on the applied methods. I suggest moving this sentence to Section 1 and to replace it with a topic sentence that introduces the methodology used.

We agree. We propose to move this sentence to section 1 and to modify it in order to better introduce the reader to the employed methodology.

Line 128: Berger and Loutre (2002) do not calculate astronomical parameters and is not the appropriate reference here. In their figure they show the values of such parameters, but these are based on Berger (1978), so I suggest to use this reference here.

We will change the reference accordingly to the referee's comment.

Line 133: "only the latest ones". I am not sure what is referred to here. The latter effects?

In the previous sentence we referred to the changes in insolation due to astronomical forcings. We will try to express the period in a clearer way.

Line 175: "while coloured are the anomalies". Please rephrase and clarify.

We agree. We wanted to indicate that biases between the two datasets are represented by a chromoghraphic gradient, from blue (when negative), to red (when positive). We reformulate the sentence accordingly.

Line 194: I propose to use "anomalously warm conditions" here.

We agree. We propose to correct the sentence accordingly to the referee's suggestion.

Line 195: " as a consequence of a wrong conversion of energy towards latent heat." This suggests to me that there is an error in the model code that described this con- version. Is that the case, or is the conversion in principle correct and does the model have a bias in S Europe?

Being our results consistent with the ones of previous studies investigating present days conditions (Kotlarski et al. 2014; Jacob et al. 2014; Hollweg et al. 2008), we suggest that the model code describing soil-atmosphere interactions should be reliable. Some biases are present, particularly over Southern Europe, most presumably due to difficulties in properly reproducing soil water storage capacity for this complex orographic area.

Line 205: typo "teperature"

corrected in Temperature

Line 213: I suggest replacing "Pollen" by "pollen-based temperatures"

We agree. We will replace "Pollen" with "pollen-based temperatures" accordingly to the referee's comment.

Line 214: Please rephrase, as this sentence is confusing. The sentence suggests that Section 3.2 will discuss the results after the validation against Mauri et al's data has taken place, while in fact the next paragraph deals with this validation. Besides, I would prefer using evaluation instead

of validation here.

We agree. We propose to use "Comparison" as a better suitable word in this case.

Line 216: I suggest referring to Figure 1, as this figure shows the boundaries of the two domains.

We agree. Nevertheless we propose to modify Figure 1 accordingly to the new analysis we presented.

Line 220: I assume that the model results are up-scaled and regridded on a 1x1 degree grid before the anomalies are calculated. Please clarify this here

The model results are up-scaled to the observations'grid as hypothesized by the referee. We already provided further details within these comment and will do the same within the revised manuscript when necessary.

Line 231: I propose replacing "Paleo-Results" by Paleoclimate results.

We agree. We will modify the sentence accordingly to the referee's suggestion.

Line 237: Figure 7 shows the insolation changes over the mid-to-late Holocene. This is the main radiative forcing for the model experiments, so I suggest to show it already in Section 2 where the experimental design is discussed.

We agree. We will move the mentioned picture to the second chapter accordingly to the editor's suggestion.

Line 250: what other cases?

Realizing that the previous sentence was misleading, we propose to replace it with "other regions".

Caption Figures 8 and 9: The captions are not consistent with the figures. Are summer results plotted at the upper or the lower row?

As the referee noticed, in Figures 8 and 9 the upper row represents winter while the lower summer. The captions, instead, were previously inverted. We propose to change the caption accordingly.

Figure 8: How is Figure 8 constructed? On what timeslice is it based, or is it based on results from several time slices?

Figure 8 represent the first two EOFs of winter and summer seasonal mean of mean sea level pressure, standardized to the pre-industrial period. We propose to add more details in the caption of this figure, being the previous one not very precise.

Line 268: "scarce ability" Replace by poor ability?

We modified "scarce ability" with "poor ability" following the referee's suggestion.

Line 276: "showing instead low correlation over the South". This is a confusing state- ment. Figure 10 shows that over most of the Mediterranean, the correlation in winter is strongly negative for the 1st EOF and strongly positive for summer.

We realized that the previous period was not really clear. In fact, with the term SNAO we wanted to refer here to the Summer NAO. The conclusions we were proposing, were definitely the same as the ones suggested by the referee. For this reason we propose to better express this period in order make it more easily readable.

Line 284: "the model simulates a lower weight of the NAO ($\sim 40\%$) for mid-to-late Holocene in comparison to present-days conditions ($\sim 55\%$)". How can we reconcile this with the notion of a "more pronounced positive phase of the NAO during the mid- Holocene" as stated on line 277?

We agree. Nevertheless, we want to highlight the fact that, according to different comments of both the authors, we deeply modified the previous analysis of atmospheric circulation. Based on the new analyses, we suggest that the previous sentence on line 284 needs corrections.

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

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Please also note the supplement to this comment:
http://www.clim-past-discuss.net/cp-2016-10/cp-2016-10-AC2-supplement.pdf

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Table 1. *Winter Temperature Cost Function Estimates for the CCLM and the ECHAM5 models compared to the Proxy reconstructions for each time slice of mid-to-late Holocene. Values closer to 0 indicate a better agreement with proxy reconstructions.*

Time Slice	CCLM	ECHAM5
6000BP	0.87	0.92
5000BP	0.88	0.92
4000BP	0.77	0.84
3000BP	0.78	0.82
2000BP	0.77	0.79
1000BP	0.61	0.61

Table 2. *As Table 1 but for Summer Temperature*

Time Slice	CCLM	ECHAM5
6000BP	0.93	0.96
5000BP	0.72	0.72
4000BP	0.65	0.67
3000BP	0.63	0.71
2000BP	0.48	0.54
1000BP	0.43	0.48

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Table 3. *Comparison of Winter temperature anomalies between 6000BP and Pre-Industrial times, for different simulations of the PMIP3 experiment and as represented in our simulations. Also the data calculated from the pollen reconstructions are provided at the bottom of the table. The values represent mean values over the regions between 35:50N and -10:40E(South), and 55:72N and -10:40E (North).*

Model	North	South
BCC-CSM1-1	1.08	-0.18
CCSM4	-0.65	-0.50
CCSM4	-0.62	-0.32
CNRM-CM5	1.45	0.27
CSIRO-Mk3-6-0	0.69	0.19
FGOALS-g2	0.13	-0.99
FGOALS-g2	-1.13	-0.26
GISS-E2-R	0.39	-0.01
IPSL-CM5A-LR	0.91	0.03
MIROC-ESM	0.14	-0.47
MPI-ESM	-0.48	-0.35
MRI-CGCM3	0.23	-0.16
CCLM	0.83	-0.29
ECHAM5	1.1	-0.33
ECHO-G	0.21	-0.11
Pollen	2.51	-0.66

Table 4. *As Table 3, but for Summer Temperature*

Model	North	South
BCC-CSM1-1	1.52	1.21
CCSM4	0.81	1.15
CCSM4	1.06	1.36
CNRM-CM5	1.29	1.21
CSIRO-Mk3-6-0	1.25	1.69
FGOALS-g2	0.53	0.76
FGOALS-g2	0.89	1.29
GISS-E2-R	1.26	0.41
IPSL-CM5A-LR	1.21	1.30
MIROC-ESM	0.81	1.20
MPI-ESM	1.19	1.09
MRI-CGCM3	1.01	1.22
CCLM	0.83	0.85
ECHAM5	1.16	0.67
ECHO-G	1.24	0.49
Pollen	0.64	-1.17

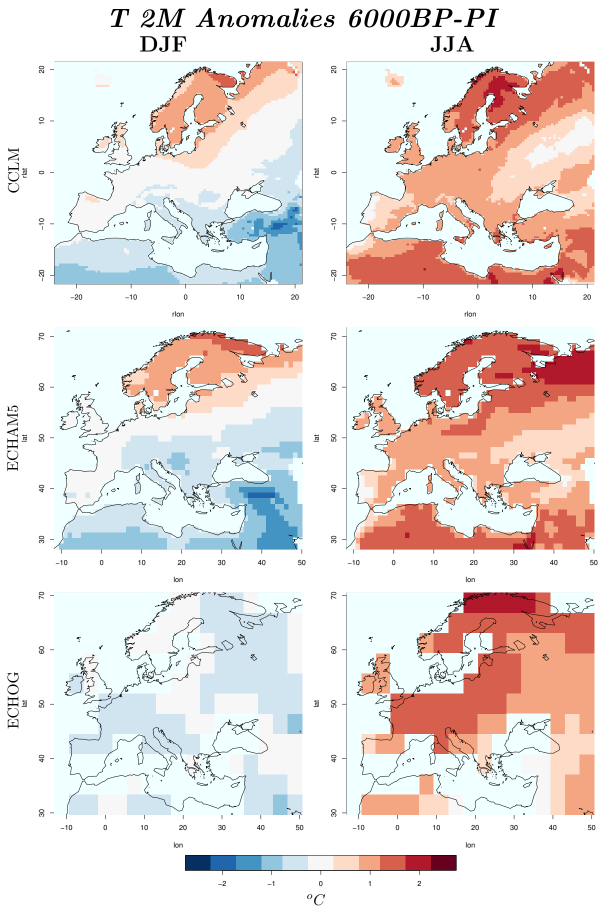

Fig. 1. Maps of Winter (left) and Summer (right) 2 meters temperature anomalies between 6000BP and the preindustrial period. The results of the different models are presented: CCLM(top),ECHAM5(center),ECHOG()

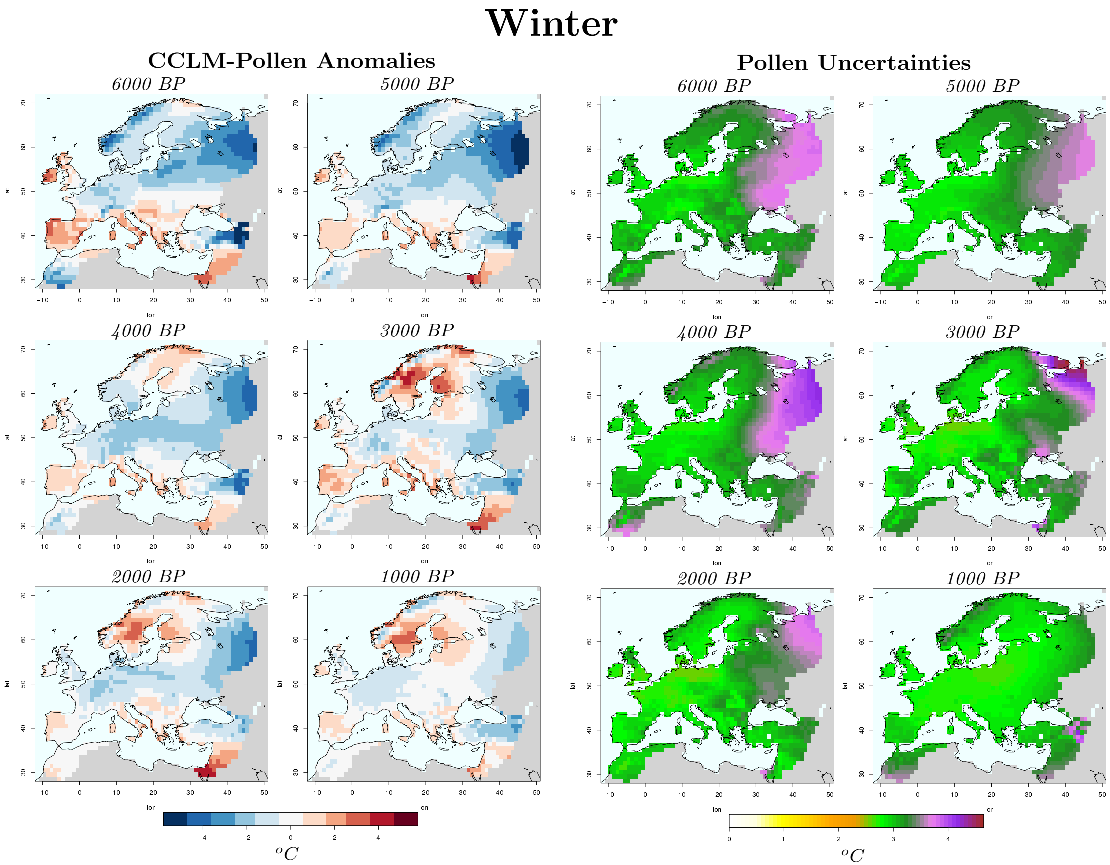

Fig. 2. Left: Maps of Winter temperature anomalies between CCLM and Pollen Reconstructions for the different time slices of mid-to-late Holocene. Right: Uncertainties in the winter seasonal mean of the pollen

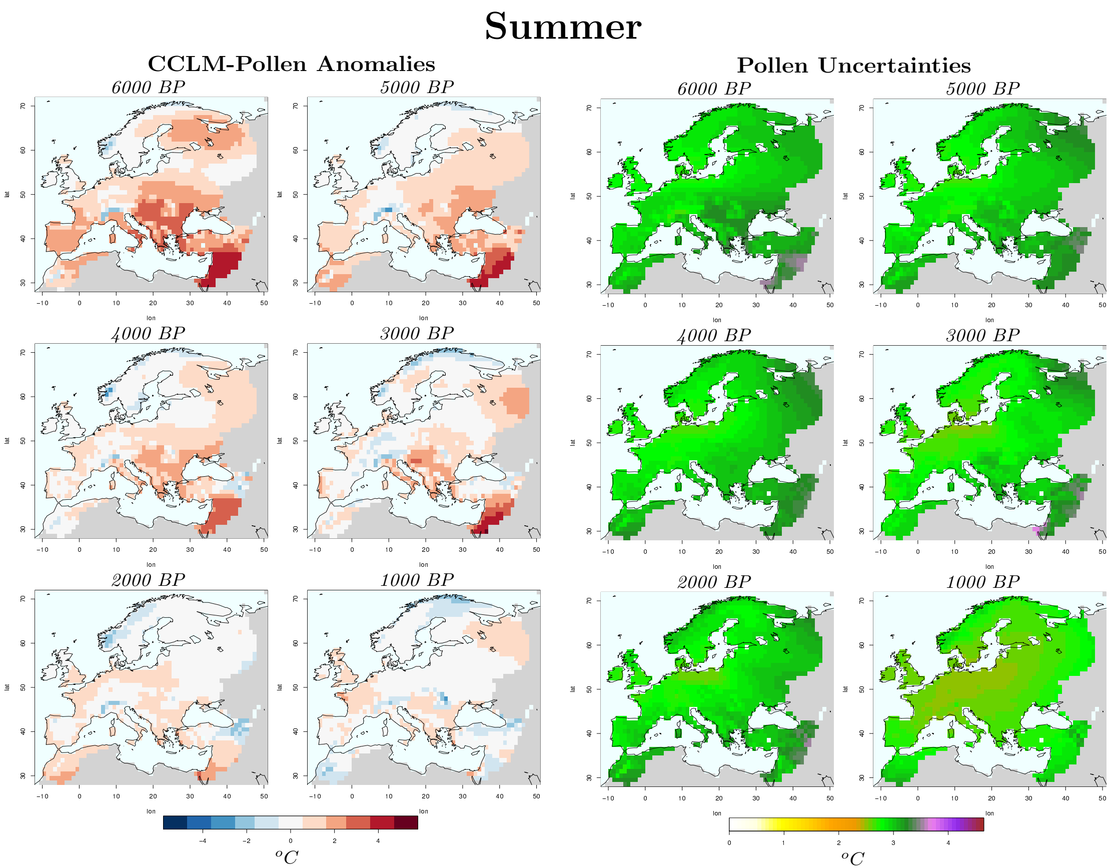

Fig. 3. As in Fig.2 but for Summer seasonal means

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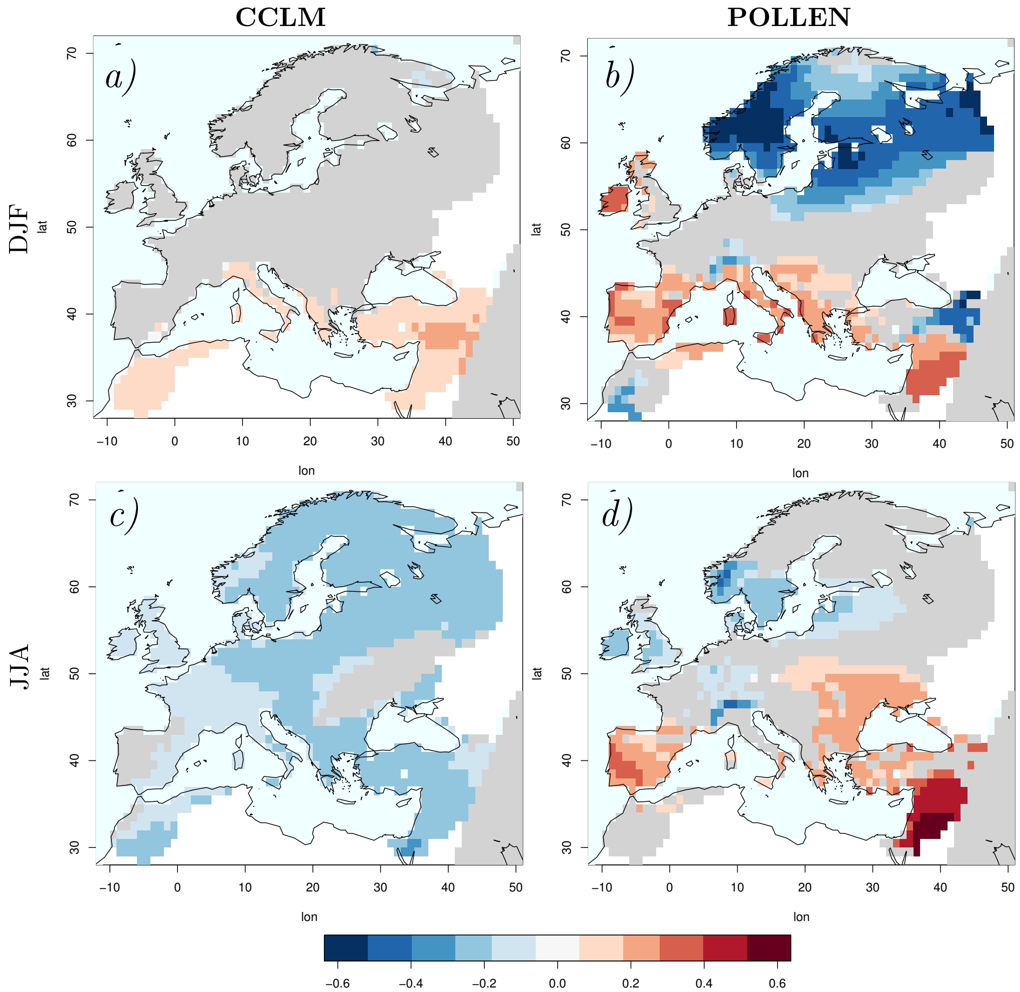

Fig. 4. Mid-to-late Holocene temporal Evolution of 2 meters temperature seasonal mean. The maps show the slopes of the linear trends calculated, for every grid box, taking into consideration the uncertaintie

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