

Clim. Past Discuss., author comment AC2
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Reply on RC2

Frank Arthur et al.

Author comment on "Simulations of the Holocene climate in Europe using an interactive downscaling within the iLOVECLIM model (version 1.1)" by Frank Arthur et al., Clim. Past Discuss., <https://doi.org/10.5194/cp-2022-21-AC2>, 2022

Reply to reviewer #2

We appreciate the reviewer's thorough and constructive feedback on our article. We have responded to all his/her comments below (in italic font).

Summary

The authors present a Holocene climate model simulation for Europe at a high-spatial resolution using dynamical (as opposed to statistical) downscaling. This regional model simulation uses inputs from the global iLOVECLIM EMIC. The authors compare this simulation with climate reconstructions and conclude that their higher-resolution model better matches the data than the lower-resolution EMIC. The main innovation is applying this method to a transient simulation that encompasses the whole Holocene and not just time-slices. I am with the authors here, in that I think that scaling down model results to the spatial scale of the proxy data is a potentially good idea, and one well worth investigating.

Reply: We thank the reviewer for summarising the motive behind our studies in a concise and clear understanding to readers.

General comments

The premise of the paper is interesting and is clearly appropriate for publication in Climate of the Past. The main problem is the nature of the evaluation which is qualitative and anecdotal rather than being rigorously quantitative. The authors really need to include a better designed evaluation process where proxy reconstructions are compared with the model results on a site/record basis, or for a region, in the case of gridded reconstructions. Improvements in the evaluation should also be extend to comparisons between the EMIC and downscaled modeling, for instance plotting both in the time-series plots. It would also be useful to compare the iLOVECLIM EMIC that is used with other models (eg PMIP3 GCM's) to see what particular biases this particular model has over the study region.

The authors also appear to be a bit loose with their commentary. When referring to

temperatures and temperature changes it is essential to state what temperature variable it is that they are referring to, for instance whether it is mean annual, winter, summer or some other aspect of temperature. This appears to be a source of confusion throughout, and particularly when citing other studies as supporting evidence. Similarly, in the discussion of proxy data, the authors need to distinguish between studies that provide evidence from single sites, and those that provide evidence from many hundreds of sites, since they are not equivalent. Also, it is important to understand the different studies cited; In Southern Europe, Bartlein et al is essentially a synthesis of the data of Wu et al 2007 and Davis et al 2003, Brewer et al 2007 takes data from Davis et al 2003, and Mauri et al 2014/15 is an improved version of Davis et al 2003 and Brewer et al 2007, both of which it supersedes. All of these latter studies use gridded data, not site data, although the gridding itself is based on site data.

I will also just add here the importance of isostatic uplift over Scandinavia during the Holocene, which in some areas has been substantial (100m+). Data-model comparisons over Scandinavia should be treated with caution where the model uses modern topography but is compared with proxy reconstructions that were much lower in the early Holocene

Reply: We thank the reviewer for this comment. We would like to note that the main intention of our paper is to evaluate our dynamical downscaling result against the standard version of the model which has a much lower spatial resolution. It was not our intention to make a full model-data comparison here, but we agree that it is very valuable to compare our high-resolution results with proxy-based reconstructions for specific mountainous regions where the potential of our downscaling method is especially clear. Still, in the revised version, we will try and do a quantitative evaluation as suggested by the reviewer and look for site-based proxy data in some specific regions and compare to our model results. In the discussion part we compared some GCMs model with our model results, However, we agree with the reviewer here that more GCMs model are needed to make a rigorous evaluation with our work. In the revised version, we will give a more detailed explanation regarding how we use the terms (such as annual and seasonal temperature results to avoid confusing the readers. We think the reviewer's comment regarding isostatic uplift is very important and we will put that into consideration when preparing our revised version of the manuscript.

Detailed comments

67-69 (and 160-164) This is a critical point.. the regional downscaling cannot correct major errors and biases in the global model simulation, including atmospheric dynamics which have been suggested as the source of much of the data-model discrepancy over Europe during the Holocene (Mauri et al 2014). The authors should note how the iLOVECLIM model generally compares with other global model simulations (eg PMIP3), for instance if it is generally cooler/warmer or wetter/drier than average, or comparable.

Reply: This is well noted, and we will discuss this in the revised version.

77-78 The main 'sensor' area of a proxy-based climate reconstruction is rarely greater than ~20 km radius for pollen and can be as small as a couple of hectares for lake-based proxies such as chironomids. It therefore makes sense to undertake data-model comparisons at comparable spatial scales (see my opening comments about improving the data-model comparison.

Reply: Yes, a very important point here, data-model comparison is hindered by the different characteristics of each dataset at small spatial scales, model output is less reliable. To avoid unbiased comparison between our model results and the proxy data, it is important we consider the spatial scales. We will incorporate this in our revised version of

the manuscript.

101-106 This is misleading. While Brewer et al does suggest that climate models can simulate cooler temperatures over Southern Europe and the Mediterranean during the mid-Holocene, this is only in WINTER and the signal is very weak. In contrast, reconstructions of SUMMER temperatures are much cooler than the models, which all show warmer summer temperatures (ie not even the same sign). This is not discussed by Brewer et al, but is clearly shown in the more recent reconstruction by Mauri et al 2014. Both Brewer et al (who uses the data from Davis et al 2003) and Mauri et al use pollen data, but the problem with cooler summer temperatures is also shown in SST reconstructions for the Mediterranean, as shown in Hessler et al 2014 (doi:10.5194/cp-10-2237-2014) figure 4. In fact, the data-model discrepancy shown in Mauri et al 2014 is a very good justification for the authors to have undertaken their study

Reply: Thanks for the comment, we will rephrase this paragraph and be more specific in the revised version of the manuscript.

119-121 I don't really understand why the authors have chosen specific areas (and variables, eg precipitation) where they then say they don't actually have proxy records, if their stated aim is to make comparisons with proxy records. It seems that they did the model analysis first, and then looked for proxy records afterwards.

Reply: With this study we provide the first transient simulations of the last 12k with interactive downscaling at 0.25° resolution over Europe. For this reason, we first wanted to assess the major changes induced by the downscaling with respect to the standard non-downscaled version of the model. The downscaling has a major impact over mountainous Region, so we focus our analysis there.

194-195 The authors use a pre-industrial climate baseline to calculate anomalies to compare with climate reconstructions. I hope that the authors are aware that anomalies shown in almost all proxy-based reconstructions are based on a modern baseline (apart from for instance Davis et al 2003 used in Brewer et al 2007, and Mauri et al 2014/5 that use a pre-industrial baseline of ~1850)

Reply: Yes, we are aware of this, but we thank the reviewer for bringing our attention to this. We will discuss this in our revised manuscript.

234+ Please be very careful, do not use the unspecified term 'temperature/s'. Please always state if this is annual, seasonal (JJA, DJF) etc. The authors appear to be conflating winter (Brewer et al 2007) and annual temperatures (Wu et al 2007) in the data, while the temperatures you are referring to in the model results are unspecified.

Reply: This will be resolved in the revised version.

246 Again this is misleading. Brewer et al only considered winter temperatures. Better to refer to Mauri et al 2014/15 which is a more recent and more comprehensive study that includes summer winter and annual temperatures (and precipitation).

Reply: We thank the reviewer for this comment. This will be done in the revised version,

249 Wu et al uses an inverse modelling method, so represents a very different pollen-climate reconstruction to the MAT method used by Davis et al (in Brewer et al) and Mauri et al, although both show the similar results (see Davis 2017 <https://doi.org/10.22498/pages.25.3.16>). Note also that Wu et al is for individual

sites, while Brewer et al use a gridded reconstruction where the site data has been interpolated onto a 1 degree spatial grid. There are also considerably more sites in Brewer et al than in the Wu et al reconstruction, while the sites used in Wu et al are poorly dated, use truncated taxa assemblages (a lot of data is from Huntley and Birks 1983), and have large uncertainties

Reply: Thank you for this informative comment. As we have replied earlier on, we will look for more proxy data which uses a lot of sites and good spatial scale for better representation.

258 Fig.3 What aspect of temperature is the figure showing? Mean annual, summer, winter etc ? please specify

Reply: This represents annual mean temperature anomaly. It will be corrected in the revised version.

264+ Again, as with temperature, please specify what aspect of precipitation you are talking about.. I presume its mean annual (units are in mm/yr), but please state this clearly at the start

Reply: Thanks for the comment, this is annual mean precipitation, so this will also be clarified in the revised version.

280-288 This is interesting, if not surprising. A question arising from this would be if the downscaling simply spatially redistributes the average precipitation of the EMIC grid box, or does it potentially increase/decrease the average precipitation that would occur in the EMIC grid box?

Reply: The downscaling does not simply redistribute the precipitation, as can be seen by a comparison for three mountainous regions in Figure 6. This clearly shows that with downscaling the precipitation is higher.

323 Fig. 5. & 371 Fig. 6, Section 4.1.1 Why have the authors chosen not to do a data-model comparison for the precipitation time-series for the Alps, Scandes etc? This would be very straight forward using the data from Mauri et al 2015 which is freely available. Also, why not show the EMIC result for the same spatial areas? This would illustrate the difference between the EMIC and the downscaling (and data)?

Reply: This will be done in the revised version.

399-413 The Furlanetto et al paper consists of only one site from the Alps, the Mauri et al analysis consists of hundreds of sites from the Alps (this has been gridded, but there is also the underlying site data that could be used). Why did you pick only this particular study? In any case, it would be useful to plot the Furlanetto et al precipitation reconstruction against the model result (both high and low resolution) so that the reader can see for themselves. It is also notable that the authors identify the strong spatial variance of the precipitation signal, but compare this single proxy site with the average precipitation of the entire Alps. Surely it makes more sense to compare the proxy record with the nearest point in the model grid?

Reply. Our downscaled results provide precipitation results at a relatively high spatial resolution. To evaluate this kind of model result, especially when evaluating against a low resolution model version, it is of crucial importance to use proxy-based records that capture this same high spatial resolution. This is the reason why we use the Furlanetto et al. paper. It is not easy to find other proxy data with such high resolution. The Mauri et al. dataset is very valuable for model evaluation, but it presents gridded precipitation at 1° resolution, which in our view does not represent a high enough resolution to represent specific conditions in the Alps or the other mountainous regions we focus on.

415-429 Again, as with the Alps, it would be better to directly show the proxy reconstructions plotted against the model result, and even better to show this at the model grid point closest to the site (or interpolated to the site location). If the authors really want to compare using the entire Scandes region, then at least compare against the same area using the Mauri et al 2015 gridded data, since this is designed to avoid spatial sampling bias associated with simply averaging site records together. It is also important to note that (presumably) the model uses modern topography and does not take into account the substantial changes in elevation that has occurred during the Holocene due to isostatic uplift. This is important when comparing with proxy records that actually include this isostatic change (See Mauri et al 2015)

Reply: Yes, a very important comment and we will work on all the constructive suggestions to improve our paper.

448-465 The authors are conflating proxy reconstructions here across all kinds of spatial scales, some from individual sites, some based on the synthesis of large numbers of sites to represent individual regions, and some where the site records are projected onto a spatial grid. Again, it would be better if at least some of these records were compared explicitly with the model (ie one plotted over the other) rather than resorting to a rather vague 'one thing looks like another thing' statement, which is open to interpretation. This is particularly important because the ability to compare model and proxy record at the scale of the proxy site is supposed to be one of the main advantages of the model downscaling that the authors are proposing. Including the results of the EMIC in the same way would also help demonstrate this

Reply: We agree with the reviewer that the ability to compare our model results and data at the scale of the proxy site should be treated as a high importance. We will do so in the revised version.

475 Southern Europe in the mid-Holocene in the PMIP2 simulations is warmer not cooler (only winter is a little cooler in the far east) and with little change in precipitation. This is shown in detail in Mauri et al 2014.

Reply: We will clarify this paragraph in the revised manuscript.

475-478 Please do not write about climate model results as if they are some kind of reality. For instance "we can infer from their work that southern Europe was wetter and cooler." Should read something like "eg we can infer from their work that southern Europe was wetter and cooler in PMIP2 model simulations."

Reply: Thanks for the suggestion, this will be corrected in the next version.

475-482 The PMIP2 results encompass a large number of different models, each sometimes showing quite different results. Are you talking about individual PMIP2 models, the ensemble mean or something else? Please be more specific. Also, Braconnot et al 2007 does not show any detail for Southern Europe or the Mediterranean (but is shown in detail in Mauri et al 2014) so I am not sure how the authors are making their comparison unless they have been plotting the data separately (it would be great to actually show this). In any case PMIP2 has been superseded by PMIP3.

Reply: We agree with the reviewer here and this will be taken into consideration.

485 Grammar needs correcting: "in the change pattern for"

Reply: We will revise as suggested.

485-495 What about temperature lapse rates? Changes in temperature lapse rates as a result of the downscaling will also lead to change in temperatures at different altitudes, perhaps better reflecting the proxy data.

Reply: The vertical lapse rate in temperature is computed in our model and shows a representative of the free-atmosphere temperature variations. So, we agree with the reviewer here that the lapse rate had an effect with temperature changes with elevation and could better represent the proxy data.

496+ Conclusions- see my opening comments. The study needs a more rigorous approach to the data-model and model-model comparison.

Reply: We thank the reviewer for taking time to make suggestions on ways to get an improved version of our manuscript. We will work on his/her major suggestions which is more based on the evaluation of our results with proxy data and other model data.

