Climate of the Past Discussions

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Reply on RC1

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-AC1, 2022

Reply to Reviewer #1

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 apply a "dynamical" downscaling technique to an orbital-only Holocene-long climate simulation and obtain a resolution of 0.25 degree over Europe. The downscaling is performed for temperature and precipitation. The authors were able to show that the downscaled precipitation matches precipitation and temperatures in mountain regions more realistically. In particular, the simulated trends of the downscaled data resemble reconstructions from different proxy archives.

Reply: We thank the reviewer for a very good summary of what our aim and objective of this paper is about.

General

The study touches a highly important topic in paleo climate, namely the mismatch between local climate proxy information and the rather coarsely resolved paleo climate modelling. The authors present an approach to bridge the gap of scales by a downscaling approach. Clearly the topic desires publication in the Climate of the past, but the current study shows a number of short comings. Besides some structural problems (see below) there is a lack in presenting the state of knowledge in the introduction as a large number of recent studies on dynamical downscaling for paleo climatic studies is missing. Then the results lack a clear discussion of seasonality differences in the proxy data. Also the method itself is to my opinion named wrongly: the authors called it dynamical downscaling and explain that the basic idea is to reproduce the model physics and NOT the dynamics. Due to my rather long list of comments (not sorted into major or minor) I recommend at least major revision of the manuscript.

Reply: We thank the reviewer for pointing to us the gaps that are missing in our study. The reviewer gave some suggestions on recent studies related to dynamical downscaling.

In our revised version, we provide a more thorough literature review on climate downscaling for paleoclimate studies. This literature review gives us the opportunity to explain better the originality of our approach and why we initially refer to our downscaling as «dynamical». We used "dynamical downscaling" in our case mostly because the downscaling is performed at each model time step, and it is consistent between the two grids (the precipitation at the coarse resolution interacts with the sub-grid). However, we understand that "dynamical downscaling" can be confusing for our approach since the regional climate model (RCM) community generally uses this terminology to refer to their approach. For this reason, we will change the terminology in the revised version. Following your suggestion, we have also added a discussion on the seasonality of the data.

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Comments

Title: The authors use only orbital and GHG forcing so I would not call it a Holocene Climate simulation, rather an orbital-GHG-only simulation for the Holocene period.

Reply. We agree that short-term forcings (volcanic and solar) were not taken into consideration. We focus on the long-term multicentennial to millennial trends, so we consider it appropriate to have used forcings that are important at this time scale. Solar and volcanic forcings are known to be of particular importance for annual to decadal timescales. *Simulations in the iLOVECLIM model show little impact of volcanic forcing in the Holocene transient simulations, but the impact is shown in the late Holocene (1.5kyr BP) (*Bügelmayer et al. 2016*). We prefer to keep "Holocene Climate simulation" to inform the reader we cover the entire Holocene.*

L 60 and the following paragraphs: There is growing literature on real dynamical downscaling using RCMs on the Paleo perspective so please make a reasonable review on the existing knowledge. Here is a collection of possible publications:

Reply: Reply: We thank the reviewer for providing us with more suggestions about previous studies related to dynamical downscaling using RCMs and we also agree with him/her that our studies need more examples to motivate our work. In particular to highlight the pros and cons of the different approaches. In our revised version, we will include them in the introduction.

L61: There are several approaches to statistical downscaling simulation. An example is Latombe et al. 2018, but there are several more publications so I encourage the authors to make a lit. search to add it to the introduction

Reply: We agree that, similarly to RCM downscaling approaches, statistical downscaling can also provide useful information for paleoclimate studies. We now include such studies in the introduction.

L65: The publication Feser et al. is not dealing with paleo research questions so why is it cited?

Reply: The publication of Feser et al. was about how Regional climate models add value to Global model data. We cited the paper because we were trying to make a point on how RCMs are useful for dynamical downscaling because their output data may be processed to produce higher resolution atmospheric fields, allowing for the modelling of small-scale processes and a more complete description of features (such as mountain ranges, coastal zones, etc).

L80-85: Here is another place to add model publication of real dynamical downscaling.

Reply: Although we do not necessarily agree with the term "real dynamical downscaling" we do agree that indeed RCM studies should be mentioned here.

L95: Units are not in italic

Reply: This will be corrected.

L100 and following: There are several more studies doing Holocene long simulation. A recent one is Bader et al. but please check again the literature. There are also approaches which first use a coarse resolved AO GCM and then a high resolved A GCM forced by the SST and sea ice distributions, please check Merz et al. and Hofer et al. publications.

Bader, J., et al. 2020: Global temperature modes shed light on the Holocene temperature conundrum. Nat. Commun., 11, https://doi.org/10.1038/s41467-020-18478-6.

Hofer, D., et al. 2012: The impact of different glacial boundary conditions on atmospheric dynamics and precipitation in the North Atlantic region, Climate of the Past, 8, 935-949

Hofer, D., et al. , 2012: Simulated winter circulation types in the North Atlantic and European region for preindustrial and glacial conditions, Geophys. Res. Lett., 39, L15805

Merz, et al. 2013: Greenland accumulation and its connection to the large-scale atmospheric circulation in ERA-Interim and paleo-climate simulations, Climate of the Past, **9**, 2433-2450

Reply: Thanks for these important publications. They will be part of the revised introduction.

Introduction in general: A discussion on the so-called 'Holocene temperature conundrum' is missing. Please check Bader et al 2020 and Liu et al. 2014.

Liu, Z., et al. 2014: The Holocene temperature conundrum. Proc. Natl. Acad. Sci., 111, 3501–3505, https://doi.org/10.1073/pnas.1407229111.

Reply: This is a very good point; We thank the reviewer for providing us very good literature suggestions. We will include the literatures proposed by the reviewer in the revised manuscript.

L125: Please change in the caption to "extent".

Reply: This will be corrected.

L 129: Why do you go from section 2 to subsubsection 2.1.1? This makes no sense. This is happening several times. There are also sections where there is only one subsection which is again awkward. Please correct the structure according to the rules of the journal

Reply: The structuring of the paper will be done by following the Journal's rule.

L140: The ECBilt model is a quasi-geostrophic model so the most important mode of variability in the climate system ENSO is not included in the model by definition. So how does this shortcoming impact your results knowing that ENSO has an influence on the Europe?

Reply: The impact of ENSO is expected to be minor in our simulations because we are investigating long (multi-millennial) trends while ENSO's impact is normally on short timescale.

Section 2.1.2: The authors do NOT apply a dynamical downscaling as they correctly say that the only try to reproduce the model physics and not the dynamics so it is awkward to call the method "dynamical downscaling. This is an important point as real dynamical downscaling implies the application of a regional climate model with incudes dynamics. So I recommend that the authors change the wording in the entire manuscript.

Reply: We used "dynamical downscaling" in our case mostly because of two reasons. First, the downscaling is performed at each model time step during run time. Second, there is a two-way coupling between the coarse grid and the sub-grid which ensures consistency (the precipitation at the coarse resolution is the sum of the sub-grid precipitation). As such, there is a strong difference with standard offline downscaling techniques. We thought that the term "dynamical" is good to convey these two ideas. However, we will change the terminology in the revised version and explain better the originality of our downscaling procedure.

L158-59: If I understand this correctly the method conserves the precipitation amount so that I would average over the same area as the coarse grid I would obtain the same precipitation also in the fine grid. If this is correct I do not understand why precipitation is different in Figure 5and e.g. in Fig 4 if we look at the grid point over Scotland. So either the figure is wrong or the description of the methods is incorrect.

Reply: We do not fully understand this comment because Figure 5 shows the precipitation trend for the whole of Europe whiles figure 4 shows simulated precipitation anomaly in spatial distribution in Europe. The figures 4a and 4d are different because we have two different experiments: (an experiment with downscaling and one without the downscaling). The experiment with downscaling modifies the amount of precipitation with respect to the standard model given that it accounts for the sub grid orography and the non-linear relationship between temperature and humidity.

L159-164: Well isn't this logical as the method does not include a dynamical part (only physics is changed) one would expect that it is not able to change the biased large scale atm. circulation.

Reply: Yes, we agree with the reviewer here on the fact that we cannot correct the biases related to atmospheric circulation. However, the downscaling changes the whole pattern of precipitation, generally producing higher precipitation over mountain ranges and as a result some drying over the continental plains. In this way it could have corrected some of the biases (or equally worsen them, depending on the bias considered).

Section 2.1.3: To my understanding I would not call this a Holocene simulation as important external forcing agents are missing, i.e., solar forcing and volcanic eruptions. It is clear that the authors cannot rerun the simulations using all forcings so I suggest to make it clear that the authors performed an orbital-GHG-only simulation for the Holocene period. So name the simulation always "an orbital-GHG-only simulation for the Holocene period." Just for curiosity why do you only use orbital and GHG forcing and not include the other two?

Reply: As replied earlier, *we focus on the long-term multicentennial to millennial trends, so we consider it appropriate to have used forcings that are important at this time scale. Solar and volcanic forcings are known to be of particular importance for annual to decadal timescales*. *Simulations in the iLOVECLIM model show little impact of volcanic forcing in the Holocene transient simulations, but the model responds to volcanic forcings in the late Holocene (1.5 kyr BP) (*Bügelmayer et al. 2016).

L245: Why do the authors compare their results to PMIP2 and not to PMIP3 or 4? There are newer studies e.g. Liu et al. 2014, Russo et al. 2022 and PMIP4 studies.

Reply: We will compare our results with new figures of precipitation and temperature to the latest PMIP papers as well in the revised version.

L253-54: please change to "Overall the native grid (T21/11.5_Standard) is still seen on the 11.5K_Down model results in many regions for all times slices. "

Reply: This will be corrected, thank you.

Fig.4: The downscaled data looks weird, e.g. in panel d we see at 50N a clear boundary with a change from +100 mm/yr to -100 mm/yr with no gradient in between. This makes no sense

Reply: The downscaling still uses the T21 grid, creating some unrealistic gradients. This is a limitation to our methodology. We will discuss this in the revised version.

3.2.1 is the only subsubsection which makes no sense.

Reply: We will correct this.

4.1.1 The authors compare their results to proxy data which is good. Still I miss a clear discussion on the seasonality of the proxy data which might play an important role in interpreting the proxy data especially the trends. E.g. tree rings and pollen data are biased to the growing season but these data are compared to yearly means of the simulation. Check out the Bader et al 2020 publication on this.

Reply: Seasonality is important, we intend to add a figure on seasonality and we will discuss this in our revised manuscript.

L442: Brayshaw et al. does not simulate the entire Holocene. He rather simulated time slices distributed during the Holocene. So it is not a transient simulation he performed. Please be more specific about this.

Reply: We thank the reviewer for this comment. yes, I think we will need to do re-wording here to explain to the reader that the work of Brayshaw et al was not transient simulations.

L475 and paragraph: Again only PMIP2 is used, why not using the updates of PMIP3 and 4 ?

Reply: We will add the updates of PMIP 3 and 4 to our revised manuscript.

4.1.2 This subsection is rather short compared to the 4.1.1 so just merge it to one section
4 Discussion

Reply: We will merge them together in the revised version.

L485-87: I think there is a caveat which makes the data not so useful as the authors think as the coarse grid sometimes remains preserved in the downscaled data leading to boundaries (see Fig. 5). I think the authors need to be more cautious about this and not overrate their results.

Reply: We thank the reviewer for this comment, there is a limitation to the downscaling methodology. Regardless of this limitation, the downscaling data is at least able to show more spatial details which is lacking in the course resolution. Yes, we agree with the reviewer to be cautions in exaggerating our results. We will revise in the next manuscript with the choice of words.

Reference list contains a lot of errors please correct them.

Reply: We will correct this.

The quality of the figures is bad please use at least 300 dpi.

Reply: We will improve the quality of the figures.