Reply to Report # 1: Anonymous Referee # 2

Summary: In this revised version of the manuscript, the authors have made some improvements according to my recommendations but have also neglected to correct other problems. The manuscript includes lots of small errors with the English and careless typos, seemingly worse than the first draft.

Reply: We have done all the recommendations suggested by the reviewer and corrected the small errors and English in the revised manuscript.

English. There are many small but nevertheless significant typographic and grammatical errors in the English. Perhaps your co-authors could help you here. I highlight only a selection but there are many more.

Reply: We thank the reviewer for pointing to us the typos and grammar errors in the manuscript, we have made corrections to these errors in the revised manuscript.

15. 'Reconstructions' not 'reconstruction'

Reply: We have corrected this.

151. 'and FOR almost all climate models'

Reply: This has been corrected.

152. 'warming IN SUMMER in the Mediterranean' Models response is tightly connected to insolation change at the mid-Holocene; summer insolation was higher and winter insolation was lower and temperature reflect this, including over the Mediterranean region.

Reply. We thank the reviewer for these suggestions, we have now amended this sentence in the new version.

153-156. See my original comments about making false equivalence's between continental scale reconstructions based on 100's of sites and averaged over 1000's of km2, and individual sites that reflect only very local conditions of only a few km2. Even though temperatures may show cooling on average across the Mediterranean, it does not mean that warming did not occur in some areas, that's how 'averages' work. The paper by Samartin et al is disingenuous in its approach. The glaciers and chironomid records cited by Samartin et al are in central Italy, where pollen records also show a mid-Holocene warming entirely in agreement with the chironomid and glacier records. For instance, see warmest month temperatures from the closest pollen record (originally by Marscicek et al) in the Climate12k compilation https://lipdverse.org/Temp12k/current_version/Ospitale.Watson.1996.html, and compare it with the Samartin et al. July chironomid record just a few km's away https://lipdverse.org/Temp12k/current_version/LagoVerdarolo.Samartin.2011.html). The Alkenone SST records that Samartin et al. mention (I presume these are the 'marine cores' that Arthur et al mention in line 155) represent annual temperatures, not summer temperature records comparable with the chironomid records. Alkenone records have been shown to be subject to bias in the Mediterranean caused by seasonal changes in productivity (eg Grauel et al 2013 doi: 10.1016/j.quascirev.2013.05.007). Actual summer SST records based on forams show an average cooling across sites in the Mediterranean (see Hessler et al 2014 doi:10.5194/cp-10-2237-2014, Fig. 4).

Reply: We thank the reviewer for the broad explanation of the data comparison here. We have now amended this section to make it clear to the reader.

Fig.2. 0 BP is AD 1950, but GHG does not show appropriate levels for 1950. If GHG are pre-industrial then the age scale needs to be adjusted accordingly.

Reply: We have corrected the figure and included a revised version.

300 (and in the rest of the manuscript). Almost every time you use the word 'anomaly', you should in fact be using the word 'anomalies'.

Reply: We have made the corrections in the entire manuscript.

303. 'SPATIAL pattern'.

Reply: This is done

477-478. Please remove 'likely' and replace with 'could be' or something similar. Bader et al. 2020 provide at best only circumstantial evidence that proxies could be biased towards the growing season. They rely almost entirely on correlation to demonstrate causation, and only for selected sites/records. Their argument would quickly collapse if they had used a broader selection of records that would have shown a wider variety of temperature trends for the same latitude, some positive, some negative (eg see the wide variety of Holocene temperature trends in https://lipdverse.org/Temp12k/current_version). The more convincing way to investigate this problem would have been to use a process-based model of the proxy, then change the seasonal insolation to see what the effect would be. In fact, this has already been done quite extensively for pollen using vegetation models run in inverse mode (see inverse modelling in Chevalier et al 2020 doi:10.1016/j.earscirev.2020.103384), and no evidence has emerged that seasonal bias is a factor in pollen reconstructions. For instance, comparisons of reconstructions based on inverse modelling (using a vegetation model) and modern analogue methods (using modern surface sample calibration datasets) show no discernable difference. For example, for the mid-Holocene see Davis 2017 Doi:10.22498/pages.25.3.161, Fig.1, and for the LGM see Davis 2022 Doi:10.5194/cp-2022-59, Fig.7.

Reply: We thank the reviewer for an extensive explanation of seasonal bias, we have amended this paragraph as suggested by the reviewer.

480-485: I mentioned this in my first review, and the authors don't seem to have understood the problem. It makes no sense to compare an area-averaged record for the whole Alps with a single site in the Italian Alps. Precipitation varies considerably across the Alps at a regional and local scale. The authors need to compare like with like using either the closest grid point, the closest grid box or (best of all) an interpolation to the exact x, y, z location of the site. Presumably this is where the advantages of the higher resolution regional model simulation should be clear. At the very least the regional model should show a higher precipitation value because the larger grid box of the GCM will have a lower average altitude. This would actually help demonstrate the usefulness of the higher resolution model. If the authors do include this comparison, then I would ask for them to show the reconstruction by Furlanetto et al and the model simulations as a time-series figure, even in the supplementary.

Reply: We thank the reviewer for this suggestion, we have now plotted our high-resolution results to the exact location of the site used by Furlanetto et al paper in the Alps and our high-resolution model show higher precipitation values as the reviewer pointed out above.

495-498. I also mention in my first review the importance of post-glacial uplift in the Scandes mountains when making comparisons between data and models. The authors still make no mention of post glacial uplift in the revised manuscript. The precipitation reconstruction in Mauri et al does not correct for uplift, only temperature is corrected.

Reply: We did not correct for post glacial uplift for precipitation in the Scandes mountains and we have now stated this in the discussion.

498. 'DOES not reflect the underlying topography'? although I don't know what the authors are trying to say here.

Reply: We have corrected this.

526. Missing bracket after 'Peyron et al 2017)'.

Reply: This is now corrected.

563-581. The English contains many simple errors. 563. This sentence is incomplete. 565. 'precipitation ANOMALIES' 567. Same. 575. 'DOES not capture. 576. 'and much wetter' 578. 'the PERSISTANT mismatch' or similar, but not incessant.

Reply: We thank the reviewer for bringing these to our attention, we have now corrected the errors in the entire manuscript.

616. '(For example..' should be '(for example..'

Reply: This is corrected.

Reply to Report # 2: Anonymous Referee # 1

Overall, some of my concerns have been treated with caution but there still a couple of issues which needs to be solved.

Reply: We thank the reviewer for taking time to review our work. We have resolved the issues in our revised version.

L22-23 The last sentence of the abstracts reads bad, please revise.

Reply: We have done this correction.

L76 missing point and I would avoid the line break here.

Reply: This has been done.

L121-125 This paragraph seems to be at the wrong place. I suggest to merge it with paragraph from L171-181.

Reply: This has been done as suggested by the reviewer.

All figures have still a bad resolution it is maybe an issue of the uploading the files and how the files are handled by climate of the past. please make sure that in the final version the resolution is sufficient.

Reply: The issue with the resolution has been resolved as well as the technical challenges due to the upload of files by Climate of the Past and the figures now have good resolution. The final version will have separate .jpg files above 600 dpi for all the figures which will be attached to the manuscript.

Fig. 1: The color scale does not fit to the shading used in the figure. There use just use the standard colorscale of ncview which is not appropriate for a publication.

Reply: We have resolved this.

L194: please change to "2 Model, Simulations and Methods"

Reply: This has been done as suggested.

Fig. 2: The aspect ratio of the figure is changed so please avoid this. There is a strange A as head of the figure why? I suggest to produce an own plot as the authors performed the simulations so they should have the forcing files.

Reply: We have done this as suggested by the reviewer.

L301: It still remains unclear how the authors had calculated the anomaly, is it the different between 9kyr and preindustrial or vice versa or the difference between 9kyr and the long term mean of the simulations? The sentence in L261-264 is highly unclear

Reply: The anomaly is calculated as the difference between 9 kyr BP and pre-industrial (that is, 9 kyr BP minus Pre-industrial mean). The sentence has been clarified in the revised version.

Table 1 a bracket is missing "(downscaling)"

Reply: This has been corrected.

L309: "warm with an annual temperature"

Reply: This is now corrected.

L311: "part, which had"

Reply: This is corrected.

Fig. 3 and 4 The colorscale is skewed (aspect ratio changed) also write “annual” in the caption

Reply: we have done this in the revised version.

Fig 5 and 6: The grey shading must be explained in the caption. Also I think the authors show some smoothed time series, I would not expect such a smooth annual time series from a model, please explain what it shown here. Is it a 1000 year running mean?

Reply: We thank the reviewer for this important comment, we have now explained every detail in all the figures. Yes, the time series is a 1000-year running mean and we have now clarified this in the revised version.

Fig7: Again the caption does not contain all explanations.

Reply: We have now explained every detail in all the figures as suggested by the reviewer.

L454 Still the sub section is only a few lines and the next covers the rest of section 4 so I suggest to avoid subsections in the discussion section.

Reply: We have merged all the subsections in the discussion as suggested.

L477-478: This is not enough. The authors need to discuss the seasonality in much more details. With their model simulations they can show results for the growing season fitting better to some of the proxy reconstructions.

Reply: We thank the reviewer for this suggestion. We have compared our model's growing season (as expressed by Growing Degree Days 0) with that of the reconstructions of Mauri et al 2015 (supplementary information) and other proxy-based data and it fits better with some proxy reconstructions.

L526 bracket missing.

Reply: *we have corrected this.*

L580: the statement "In any case, it is challenging

to determine which model would best depict the climate at the mid-Holocene" is awkward, please reformulate or remove.

Reply: We have removed this sentence.

L596 I recommend to include a discussion on the weaknesses/limitations of the method used. This is essential as at the moment the reader gets the impression that there are no weaknesses. So include this in the conclusion section maybe at the end and also in the discussion section.

Reply: we have added this to the last paragraph of the discussion as suggested by the reviewer.

L615 The sentence "reconstructions, we agree that that our 11.5K_Down simulates in some" makes no sense, please revise.

Reply: We have made this correction.

There are still errors in the reference list, e.g. "Davis et al., 2003 B.A.S. Davis, S. Brewer, A.C. Stevenson, J. Guiot.:" I will never understand why author still hard code references and not use endnote or other tools.

Reply: We have made all the corrections in the reference list