Dear Dr. Rogozhina,

thanks you for your efforts in editing the manuscript. We have now prepared a detailed response to the remaining issues that were raised and edited the manuscript as well as the response to the reviewers as requested. The edited response to the reviewers is attached at the end of this document as we could only upload a single response file.

Please see a detailed response of to your comments below.

On behalf of all co-authors,

Dirk N. Karger

Dear authors,
With this letter, I would like to inform you that I have made a decision to publish your manuscript after minor revisions. The review process related to this manuscript has been extremely rocky and lengthy due to differences in opinions among reviewers, authors, and the editor (I). Even though it is tempting to opt for a quick completion of such a long process, we (reviewer 2 and I) see the need for some final adjustments in both the manuscript and communications with reviewer 1. Our recommendations are outlined below.

In his final report, reviewer 2 admits that the earlier points raised by his review have been mostly addressed, and he only has minor suggestions for the remaining changes in the manuscript included in their recommendations visible to the authors. Following these suggestions, please, do a thorough proof-read of the manuscript in its entirety, including the text, all figure captions, and legends. Please, ensure that the suggested changes are included in the manuscript's title. Furthermore, I have received a more detailed report from reviewer 2 discussing to which extent the criticism in review 1 has been addressed and whether the lack of responses to some major points is justified. I am combining this assessment by reviewer 2 with my own assessment in this final decision. First, we do not feel that the authors have mastered the art of diplomacy required by the official peer-review process. Some of the responses to review 1 are at least indelicate if not arrogant or even borderline rude. I would like to eliminate such communication style from the official review process, even if you disagree with the reviewer's line of argument. Not only does it obstruct the timely review process, since everyone gets irritated and confrontational, but it also undermines the fact that reviewers invest significant time into reading manuscripts and writing their reports. While doing so, they act as allies of the journal (if the review process is fair), with this suggesting that rudeness towards reviewers is also rudeness towards the editor whose choice of reviewers is far from random (you can be absolutely sure in this case). Taken together, these actions can be treated as disrespectful towards the journal as a whole. Although my final decision is generally in favor of the publication, the above issues will have to be corrected as part of the required minor revisions.

We have concluded that the authors have put significant additional work into improvements of their manuscript, following many suggestions of both reviewers. However, we would also like to stress that only due to reviewers' time investment and ideas, this study is now close to publication. At the initial stage, it rather resembled work-in-progress and not a very thorough one, albeit with some potential that justified its initial publication in discussions. I believe that this energy-saving effort and the lack of diplomacy were the main reasons for tensions, which could be avoided altogether if the manuscript were stronger from the start.

Response: We are sorry that our response has given a wrong impression of the effort the reviewers have devoted to the article. We edited the response to the reviewers for the second major revision and changed the tone of the responses that might have been irritating. We indicated the changes in a track changed "response to reviewers" for clarity.

Regarding some of the criticism from review 1 that has not been fully addressed, I encourage the authors to look at the first round of reviews to find the comment 2) mentioned by reviewer 1 in the second round (i.e., the interpolation procedure using temperature as a proxy for ice sheet retreat). It is not enough to simply state that you could not find it and thus could not explain why it was not fully addressed.

We have looked back onto the reviews and change the response:

"With respect to the coupling to air-temperatures, we would like to first reiterate our response, that the amount of ice is of course a balance of precipitation fluxes and temperature changes, which can be physically modeled with a numerical model. As this is not feasible at 1km resolution due to computational limitations we use mean annual temperature as a proxy for an interpolation approach.

With respect to the validation of the results we would like to refer you to section 4.3 of our manuscript and our more detailed response to your comment on l220. In section 4.3 the results of our glacier interpolations are compared to one of the datasets you suggest (https://doi.org/10.1016/S1571-0866(04)80209-4) using several test statistics. It shows that our interpolations work well, but also have inaccuracies attached to them and specific points in time. "

We also added something in the conclusions with respect to this:

"The resulting ice cover from this interpolation can, in some areas, only be a few meters thick, not representing real glaciers, but rather spatial autocorrelation artefact of the interpolation approach used (e.g. see Supplementary Figure S1, 13ka BP). Another source of error is that changes in bedrock due to the release of pressure from the melting ice sheets are not yet included in the algorithm. This can potentially result in several hundred meters of bias in affected areas that have not been taken into account in the current version of the algorithm."

Regarding your response to reviewer 1's comment related to l12, as explained by the reviewer, ICE6G is not derived from an ice sheet model, and you have corrected this misunderstanding but only after the reviewer 1 has mentioned it twice (in the two rounds of reviews!). However, it is still too vague to write "ice sheet data" – please, look it up in the original article for a more precise definition.

We changed it in the abstract as well as in the main text. In Peltier 2014, ICE5 G is referred to as a "global model of glacial isostasy", with ICE6G_C is the GPS-refined version of it. We hope that this terminology is correctly mentioned in the manuscript now:

In the Abstract:

Paleo orography at high spatial resolution and for each timestep is created by combining high resolution information on glacial cover from current and Last Glacial Maximum (LGM) glacier databases and interpolations using data from a global model of glacial isostasy (ICE-6G_C)

In the main text:

2.2 Global model of glacial isostasy: ICE-6G_C (VM5a)

We used the output data of the ICE-6G_C (VM5a) (hereafter ICE6G) model as a basis for the extent of the major ice sheets at 1° resolution. ICE6G is a refinement of the ICE-5G (VM2) (hereafter ICE5G) global model of glacial isostasy model (Peltier, 2004) which has been widely used to model the distribution of *major ice sheets through time. ICE6G improves ICE5G by applying all available Global Positioning System (GPS) measurements of vertical motion of the crust that constrain the thickness of local ice cover as well as the timing of its removal. ICE6G explicitly outputs changes in ice thickness of major ice sheets (e.g. the Laurentide ice sheet) from the LGM till today (Argus et al., 2014; Peltier et al., 2015) at 500 year time steps.*

Although I agree with reviewer 1 that GIA-related changes in the topographic forcing, in addition to changes due to sea level fluctuations, would improve the quality of your final dataset (~few hundreds of meters), I understand that this might not be possible at this stage. Even though it should not be that difficult based on my experience, I am ready to pass on this issue.

Response: We absolutely agree it would indeed improve the dataset to include bedrock changes in the next version of the downscaling model, and it will certainly be the next step in improvement we are planning. We also changed the response to the reviewer here. It now says:

"Response: We already replied to his point and pointed out that this might be partly based on a misunderstanding from the reviewers side. We do not change the bedrock topography, and that it is unfortunately not possible with the current algorithm applied. We simply make sure that the orography (terrain elevation above sea level) we use for the downscaling algorithm is actually adjusted to the sea level. If we would not do that, the land surface would not have changed in the last 21 thousand years due to increasing sea levels.

The reviewer mentions: "The idea to correct present-day topography/bathymetry with global sea-level does not make sense to me. Surface elevation over this time scale does not only change due to changing sea-level, also due to isostatic changes of the bedrock."

At this point we can unfortunately only mention that we do not have isostatic changes in the bedrock included in the model yet. It would indeed be nice to include them in an updated version, but it would require to develop a transfer function to get from e.g. coarse grid information of changes in bedrock, to 1km high resolution topography. Since we do not have such a function developed yet to integrate into the model we can only mention that this effect is unfortunately not included. We also do not see how this is a major problem but rather a factor that increases the bias of the model and gives room for future improvements. As an example: Most global climate models do not resolve convective precipitation. But that does not mean that these model are fundamentally 'flawed', just because they do not include all possible processes. It rather introduces a bias from the model that, as mentioned correctly by you, should be raised to the reader's attention."

We also added something in the conclusions with respect to this:

"The resulting ice cover from this interpolation can, in some areas, only be a few meters thick, not representing real glaciers, but rather spatial autocorrelation artefact of the interpolation approach used (e.g. see Supplementary Figure S1, 13ka BP). Another source of error is that changes in bedrock due to the release of pressure from the melting ice sheets are not yet included in the algorithm. This can potentially result in several hundred meters of bias in affected areas that have not been taken into account in the current version of the algorithm."

Finally, your statement that reviewer 1 has ignored section 4.3 when talking about the validation against geological evidence is only partly valid. Why on Earth did you pick the ancient dataset of Dyke et al. (2003) to validate your model and why did you do it over North America only? What a strange choice considering the focus of your other analyses on Europe. There are extremely well resolved geochronological datasets for the British Isles, Scandinavia and even the European Alps. I understand the comments of the authors regarding their Section 4.3 being ignored, but I also emphasize that the way it is handled now is suboptimal from the point of view of the fields of geochronology and geomorphology. You need to at least discuss your choice of a region (and dataset) for validation. Otherwise, it leaves a heavy feeling.

Response: The choice of Dyke 2004 was mainly to highlight the global extent of the dataset to the reader. Otherwise we felt that, as you mention correctly, the validation would be very euro centric. We are sorry that this led to more confusion than clarity at this point.

Additionally, we added now a supplement that shows the glacial extent from our model compared to Stroeven et. al 2016 (https://doi.org/10.1016/j.quascirev.2015.09.016) and ICE6G. This dataset allows for an additional visual validation of the glacial component in our model. The results from the comparison confirm the results from North America, where over long periods the accuracy of the glacial reconstructions is above 0.8%, but clearly between 9 ka BP and 6 ka BP our model seems to create a larger mismatch towards these two datasets. We added a few new lines about these results (please see the track changed version of the manuscript).

"Additionally, we used the data from the extent of the ice sheets over Fennoscandia from 22ka BP to 10 ka BP (Stroeven et al., 2016) for all timesteps for which ICE6G data and data from Stroeven et al. (2016) was available. The results (Appendix 1) show similar to the North American ice sheets that the accuracy is relatively high until 10.5ka BP, with a drop in accuracy at 10ka BP. Therefore we assume that the temperature coupling does introduce errors in the time between 10ka BP and 6ka BP as evident from the comparison with the ice sheets of North America and over Fennoscandia."

We also reformulated our response to the reviewer here:

"Reviewer 1: There are large data collections available to constrain the LGM-present glacial extent based on geomorphological constraints (e.g. https://doi.org/10.1016/j.quascirev.2015.09.016; https://doi.org/10.1016/S1571-0866(04)80209-4). How does your approximation compare to those reconstructions? Please include a validation for your method.

Response: We included an validation of the glacier reconstructions in section 4.3. The https://doi.org/10.1016/S1571-0866(04)80209-4 you suggest here is Dyke et al. 2004, which we are already using as a basis for our validation in section 4.3. We initially opted for Dyke to also highlight to the reader that the data from the CHELSA-TraCE21k model is global in extents, as otherwise our validation or plausibility test would be very euro-centric. We however added a comparison between our model, Stroeven et. al 2016 (https://doi.org/10.1016/j.quascirev.2015.09.016) and ICE6G and moved it to the Appendix S1"

Given the above, I would like the authors to walk through the remaining issues mentioned about and find elegant ways to address them at a limited labor cost. Furthermore, I am asking the authors to go back to their responses to review 1 and remove all the irritation-filled comments and overall apply good practices of diplomacy throughout their updated responses to reviewer 1. This effort will pay off handsomely with a manuscript publication.

Kind regards,
Irina

Response to reviewer 2

I appreciate your effort to adequately address most of my comments in the latest version of this manuscript. I would only insist on the technically correct title, with hyphenated compound adjectives, addition of the word "downscaled" and use of capital letters for LGM: "CHELSA-TraCE21k — High-resolution (1-km) downscaled transient temperature and precipitation data since the Last Glacial Maximum".

Response: Changed

There are also some tiny details to be corrected such is, for example, in the sentence in the lines 49-52. A would say there is a word "desired" missing in the part : "...25-fold lower than desired computationally efficient simulations of 1 SYPD...", otherwise, it does not make too much sense.

Response: Changed as suggested

I have also noticed some technical errors (repeating words in abstract, chapter 5 missing and similar), therefore I would suggest a thorough inspection of all the text, figures and legends and technical corrections before the final submission.

Response: We went through the manuscript again and checked grammar, spelling, and technical aspects again. There are a few minor changes related to this.

EDITOR

Dear authors,

as you have seen, reports of the two referees have deviated in their attitudes and decisions with regards to your article. In response to the criticism in review 1, you have expressed doubts that reviewer 1 has read your manuscript carefully enough and that their criticism is well grounded. I have therefore asked you to prepare a detailed rebuttal letter explaining your grounds for such statements and a revised manuscript highlighting all changes that you have made in response to reviews 1 and 2. Once you have submitted both the letter and the manuscript, I will evaluate your arguments together with an independent reviewer and will make a final decision about the publication or otherwise.
Good luck and let us hope for a positive outcome.

Kind regards,
Irina

Dear Dr. Rogozhina,

Thank you for your work on this manuscript. We have now prepared a point to point response to all concerns and comments by the two reviewers.

Reviewer 2: Suggested mainly changes of the text and a more consistent terminology throughout the manuscript. We incorporated all of the comments this reviewer had.

Reviewer 1: Is mainly concerned about how we created the orography that we used for the downscaling algorithm. The concern is that we do not include changes in bedrock elevation (which we cannot at this point). Additional the reviewer asks for a validation of the glaciers, which is however, already included as section 4.3 using a dataset the reviewer actually suggests. We include a detailed response to the critique below. Were we thought that the reviewer had a valid point, we incorporated the suggested changes.

We hope that you can follow our reasoning in this respect and deem the manuscript suitable for publication.

Best regards,

On behalf of all co-authors

Dirk Karger

REVIEWER 1

General comments

I have reviewed an earlier version of this manuscript as REVIEWER 1. While I acknowledge important improvements in the current version of the manuscript, it requires major revisions to make it suitable for publishing. A few of my earlier comments have not been addressed, so I am reiterating them once more.

With the given documentation, it is still not possible to judge if the applied methodology is flawed. This concerns 1) the way only sea level (and not bedrock changes) are used to calculate elevation changes and 2) the interpolation procedure using temperature as a proxy for ice sheet retreat. I have suggested possibilities to validate the methods with available reconstructions, which I consider fundamental to support the choices presented in this paper.

Response: Thank you for your comments and efforts in reviewing our manuscript a second time. It is unfortunate that our previous explanation has not been clear enough regarding the interpolation procedure of the glaciers despite the new and additional figures to explain it step by step. Our model does not include bedrock changes but only sea level changes incorporated. We might include this effect in a newer version of the model to improve it further. We are aware that this model has limitations and cannot be perfect in every regard.

With respect to the coupling to air-temperatures, we would like to first reiterate our response, that the amount of ice is of course a balance of precipitation fluxes and temperature changes, which can be physically modeled with a numerical model. As this is not feasible at 1km resolution due to computational limitations we use mean annual temperature as a proxy for an interpolation approach.

With respect to the validation of the results we would like to refer you to section 4.3 of our manuscript and our more detailed response to your comment on l220. In section 4.3 the results of our glacier interpolations are compared to one of the datasets you suggest (https://doi.org/10.1016/S1571-0866(04)80209-4) using several test statistics. It shows that our interpolations work well, but also have inaccuracies attached to them and specific points in time.

Please find a detailed response to the specific concerns raised below.

Specific comments
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l12.
Repeating my earlier comment: ICE6G is not a dynamic ice sheet model. I have looked up the description for you: "the ICE-6G_C (VM5a) model that is under discussion in this paper is based upon the 'GIA only' methodology. In this methodology the ice thickness history as a function of position is simply adjusted iteratively in order to satisfy all of the available constraints". Please reformulate.

Response: Changed to: "…and interpolations using ice sheet data (ICE6G)…"

l26.
Here you state "climatic conditions at spatial resolutions < 1 km", but later (e.g. 48) it is "~1km". Should be made consistent.

Response: Changed

l27.
"run at much coarser grains" --> "run at much coarser resolution"

Response: Changed

l34.
"has be bridged" --> "has been bridged"

Response: Changed

l44.
"on earth" --> "on Earth"

Response: Changed

l48.
It is somewhat trivial to state that 0.043 SYPD is 25-fold lower than 1 SYPD. What would be an example of a typical 1 SYPD simulation?

Response: It is unclear what is asked here for. SYPD is a measurement of computational efficiency.

l60.
Repeating comments for "time steps of 100 years from 21k-BP to 1990"
Here and elsewhere, this should be written as "21 kyr BP"
BP typically refers to years before 1950. If you go in steps of 100 from 21 kyr BP forward, you never end up at 1990. Either you used a different time step than 100 years, or a different starting point. That is why you have to modify this statement.

Response: Changed to ka BP

l60.
(TraCE-21k) should be defined the first time you use it (outside of the abstract).

Response: Changed

l64.
It would be good to add some information about what topography and ice sheet boundary condition (topography, mask, albedo) was used to produce this simulation. Is the land-sea mask constant? Is the topography changing? If yes, is the topography change consistent with the ICE6G_C reconstruction?

Response: This is in detail given in:

He, F.: Simulating Transient Climate Evolution of the Last Deglaciation with CCSM3, PhD - Thesis, University of Wisconsin Madison, Madison, WC, USA, 171 pp., 2011.

Liu, Z., Otto-Bliesner, B. L., He, F., Brady, E. C., Tomas, R., Clark, P. U., Carlson, A. E., Lynch-Stieglitz, J., Curry, W., Brook, E., Erickson, D., Jacob, R., Kutzbach, J., and Cheng, J.: Transient Simulation of Last Deglaciation with a New Mechanism for Bølling-Allerød Warming, Science, 325, 310–314, https://doi.org/10.1126/science.1171041, 2009.

We did not run the CCSM3 TraCE-21k simulations by ourselves, so we think it would not be appropriate to repeat the methods from He et al. and Liu et al. here, which would be outside of the scope of the paper.

l69.
Is "DGVM" the name of the specific model or the shorthand for all dynamic global vegetation models (as line 71-72 may suggest)? Clarify!

Response: as stated: "…dynamic global vegetation model (DGVM)…"

l70.
Is the "land [...] model" the same as the vegetation model? If not, introduce the land model here.

Response: It's the land component of CCSM3. We tried to clarify:

"…The TraCE-21k simulation was calculated at a T31_gx3v5 resolution (Otto-Bliesner et al., 2006) using a coarse resolution dynamic global vegetation model (DGVM). The coupled atmosphere-ocean model in CCSM3 is based on the Community Atmospheric Model 3 (CAM3), on 26 vertical hybrid coordinate levels. The land and atmosphere components in CCSM3 in the TraCEF-21k simulations uses the same resolution. The parameterizations of the DGVM are largely based on the Lund-Potsdam-Jena (LPJ)-DGVM. The ocean model in CCSM3 uses the NCAR (National Center for Atmospheric Research) version of the Parallel Ocean Program (POP) with 25 vertical levels and the sea ice model is the NCAR Community Sea Ice Model (CSIM)…"

l76.
What is "mean monthly daily 2m mean"? Maybe "monthly mean, minimum and maximum temperature and precipitation fields"?

Response: Changed

l78.
You call it "mechanistic climate downscaling". I am familiar with terms statistical and dynamical downscaling. Which category does yours belong to?

Response: None of the above. It is a hybrid model, that has mechanistic components, but also statistical ones. Another term we commonly use is topographic downscaling.

Changed to: topographic downscaling

l84.
Remove "and dynamics". ICE6G is not a dynamic ice sheet model.

Response: Changed

l85.
"from the Last Glacial Maximum"
What is the first available year in the time series?

Response: 26kyr BP? We however only start our downscaling at the LGM as stated.

l87.
LGM is already defined. Remove "(LGM)"

Response: We removed 'Last glacial maximum' and keep the abbreviation

l88.
LGM is already defined. Modify description.

Response: We removed 'Last glacial maximum' and keep the abbreviation

l89.

How "up-to-date" is this dataset from more than 10 years ago?
Suggest to reformulate.

Response: deleted 'up to date'

l95.
What time does "the 'current' extent of the glaciers" refer to? I understand that this may not be clearly defined by the dataset providers (late 90s, early 2000 maybe), but you are assigning it eventually to a certain time in your modelling (maybe 1990?). This should be mentioned here or elsewhere as in "We are assigning the dataset to the year xxx in our modelling". The same applies to all the other data sources (GMTED, GEBCO), which may be best done by a summary statement in the end.

Response: Unfortunately the GLIMS database does not give a reference year. We therefore use as referring to the reference period (1950-1990).

L105.
"we keep as land altimetric data that of the CHELSA V1.2 procedure"
I didn't find a description what topography is used in CHELSA. Should be added in 2.2.

Response: Page 3, Line 13 in Karger et al. 2017, Sci. Dat. "…the Global Multi-resolution Terrain Elevation Data 2010 (GMTED2010)…". or Figure 1.

We included it now:

Although GEBCO also includes land surface altitude, we only use it for the oceans, and we keep as land altimetric data that of the CHELSA V1.2 algorithm (that being GMTED2010) to maintain comparable topography at the land surface.

Section 2 in general
Since this has become a long list of rather short subsections, it could be an idea to display the information about the different datasets in a table (name, description, time coverage, reference, ...) complemented with a summary paragraph.

Response: This is a matter of personal preferences and we actually prefer to keep it as it is.

l117.
"As the orography at different time steps between 21k BP and current times is not available"
I think that ICE6G_C would give you that information. Maybe add "... at the high resolution required for our downscaling method" to make sense of this statement.

Response: Changed to: "…As the orography at different time steps between 21ka BP and current times is not available at the high resolution required for the CHELSA algorithm…"

Figure 1, l124.
Terminology. I am not an expert on this, but it seems that the distinction made here between topography (as relative to present day sea-level) and orography (relative to current global sea-level) is not supported by common definitions of the two terms. Also, I could not find a description of

bathymetry in the ocean and surface elevation over land in one generalised term. I would suggest to using your own symbols and describing what they mean, rather than using established terms that mean something else.

Response: There seems to be a misunderstanding: Orography generally refers to terrain above water (including glacier surfaces), topography can also include terrain under water, bathymetry contains terrain under water. This is the same terminology as used in ICE6G_C (e.g. orog and topo) for example.

I doubt that the combination of GMTED2010 and GEBCO really gives you bedrock topography (upper right in figure 1). I think GMTED2010 provides surface elevation over glaciated areas (Greenland/Antarctica), which is the upper ice surface. The bedrock topography is a few thousand meters below that. The same applies to 'bedrock' in the downstream box 'bedrock orography' to the left.

Response: That is correct, it gives the bedrock for non-glaciated areas. For the approach used here, this is however does not constitute a problem, since we are interpolating between past and current glacier extent. We clarified this by adding:

"…To create a bedrock orography e_t^{bed} (i.e. topography adjusted for sea level without glaciers except for currently glaciated areas)…"

The idea to correct present-day topography/bathymetry with global sea-level does not make sense to me. Surface elevation over this time scale does not only change due to changing sea-level, also due to isostatic changes of the bedrock. In the periphery of the ice sheets where it may be most important for your biological application, the bedrock change may well be the dominant signal. I see in l169 that you acknowledge that bedrock changes are not taken into account. But why not?. ICE6G_C will give you a consistent set of data for sea level and bedrock elevation, in addition to ice thickness. In fact, the bedrock change is likely the most reliable output of ICE6G_C, because ice thickness is prescribed to get the right loading history. Why are you not using it? If you think your sea-level correction method gives a better representation of surface elevation, you should at least compare your results (with appropriate figures) against ICE6G_C. This will show if your method is an appropriate approximation for the full solution. My intuition is that surface elevation will be off by a few hundred meters in proximity of an ice sheet. If I am overlooking something obvious here, please explain better in the manuscript why you chose to not use the full information provided by ICE6G_C.

Response: We already replied to his point and pointed out that this might be partly based on a misunderstanding from the reviewers side. We do not change the bedrock topography, and that it is unfortunately not possible with the current algorithm applied. We simply make sure that the orography (terrain elevation above sea level) we use for the downscaling algorithm is actually adjusted to the sea level. If we would not do that, the land surface would not have changed in the last 21 thousand years due to increasing sea levels.

The reviewer mentions: "The idea to correct present-day topography/bathymetry with global sea-level does not make sense to me. Surface elevation over this time scale does not only change due to changing sea-level, also due to isostatic changes of the bedrock."

At this point we can unfortunately only mention that we do not have isostatic changes in the bedrock included in the model yet. It would indeed be nice to include them in an updated version, but it would require to develop a transfer function to get from e.g. coarse grid information of changes in bedrock, to 1km high resolution topography. Since we do not have such a function developed yet to integrate into the model we can only mention that this effect is unfortunately not included. We also do not see how this is a major problem but rather a factor that increases the bias of the model and gives room for future improvements. As an example: Most global climate models do not resolve convective precipitation. But that does not mean that these model are fundamentally 'flawed', just because they do not include all possible processes. It rather introduces a bias from the model that, as mentioned correctly by you, should be raised to the reader's attention.

In the conclusions we added:
"…The resulting ice cover from this interpolation can, in some areas, only be a few meters thick, not representing real glaciers, but rather spatial autocorrelation artefact of the interpolation approach used (e.g. see Supplementary Figure S1, 13ka BP). Another source of error is that changes in bedrock due to the release of pressure from the melting ice sheets are not yet included in the algorithm. This can potentially result in several hundred meters of bias in affected areas that have not been taken into account in the current version of the algorithm…"

l128.
The following description still misses clear motivations for why things are done the way they are done. You should start by making clear what information you have (e.g. present day seabed and land topography), what you are trying derive, and how you are making approximations/interpolations to get there.

Response: From the comment as given it is not entirely sure to us what is missing here. We tried to do exactly what has been suggested here. First we state the information we have:

"The first step in estimating the paleo-orography was carried out for the LGM (21k BP). For this time point, both estimates of glacial extents from Ehlers et al., 2011 and estimates of glacier thickness from ICE6G exist. "

, after that we state what we want to derive:

"We first combined the topographic information from GMTED2010 on land, and that of GEBCO into a bedrock topography that provides the current bedrock topography e_c^topo (including current day glaciers, see ff.)"

In figure 1 and figure 2 we show the process in a graphically. Additionally the equations are all given to allow to better assess the way it is done.

l130.
I think from "We first combined ..." you are no longer at the LGM. That would be good to make clear. E.g. with "that provides the surface elevation e at the present day"

Response: Changed: "…We first combined the topographic information from GMTED2010 on land, and that of GEBCO into a bedrock topography that provides the current bedrock topography e_c^{topo} (including current day glaciers, see ff.)…"

l136.
Back to LGM? Confusing!
I think from l136-l147 (maybe even l149) you are describing how to produce a smooth ice surface elevation for the LGM on a high-resolution grid. Could be good to say that upfront.
The then following transition into a time-dependent estimate is confusing to me, because we don't know at this point how the interpolation between LGM and present will work. Maybe it would be better to leave the time dependence out of this for now?

Response: We describing first how we create the initial high resolution ice surface at the LGM (t=0) and then how we use this as a basis for the next timestep. (t0 … t*n*). Since this is a iterative process over time removing the time dependence here would not be correct.

l170.
Without any validation/comparison against reconstructed glacial extents from the literature, it is not possible to judge whether this interpolation approach is an innovative method or a bad idea. See also comment l220.

Response: At this point we would like to refer to section 4.3 "Validation of glacier extent between 18ka BP and 1ka BP" and our response to the comment in l220.

l171.
"As high-resolution estimates of glacial surface elevation are not available for timesteps t other than the LGM"
For the LGM you had Ehlers2011 to delineate glacier extents, but the resolution of the surface elevation data is the same for all time steps in ICE6G_C, isn't it?.

Response: Yes, it is all the same: 1°.

l173.
"at each time step $t \neq 0$"
But also not at present day, right?

Response: Correct. Changed to:

"…at each time step $t \neq 0$ and t = 221…"

l181.
"resampled to a 0.5° grid resolution"
Can you motivate the need for this intermediate grid? Why not interpolate one of the products to the grid of the other?

Response: This is the same resolution as in the CHELSA V1.2 algorithm. We kept it constant with the algorithm.

We added: "…The resolution of 0.5° follows the same procedure as used in CHELSA 1.2 (Karger et al. 2017)…"

l220.
There are large data collections available to constrain the LGM-present glacial extent based on geomorphological constraints (e.g. https://doi.org/10.1016/j.quascirev.2015.09.016; https://doi.org/10.1016/S1571-0866(04)80209-4). How does your approximation compare to those reconstructions? Please include a validation for your method.

Response: We included an validation of the glacier reconstructions in section 4.3. We also show the deglaciation on the north American ice sheet in Figure 6. that forms the basis of our validation. The https://doi.org/10.1016/S1571-0866(04)80209-4 you suggest here is Dyke et al. 2004, which we are already using as a basis for our validation in section 4.3. We opted for Dyke to also highlight to the reader that the data from the CHELSA-TraCE21k model is global in extents, as otherwise our validation or plausibility test would be very euro-centric.

l240.
'idiosyncratic'
I have commented this before and supposedly you had changed it. Here it is again.

Changed to: "...The CHELSA V1.2 algorithm assumes that orography is one of the main drivers of precipitation..."

l299.
Could you motivate the parameter choices for c and h? Have you tried other values? Are they taken from past experience with the model or from published values?

Response: They come from the tuning of CHELSA V1.2. See Karger et al. 2017, Sci. Dat.

l304.
Remind us what reference period the bias correction is calculated over and that you are using an intermediate grid.

Response: 1980-1990. Added

l331.
"tas being interchangeable for tasmax and tasmin in Eq. 22 and Eq. 23"
This doesn't work, because you have already defined tas = (tasmax+tasmin)/2 in line 323. Need to find another symbol.

Response: Clarified to:

"...The downscaling of monthly near surface air temperatures (*tas, tasmax, tasmin*) follows the methods described in 3.2.2., with the only difference that instead of mean annual temperature, *tasmax* and *tasmin* are used, where *tas* = (*tasmax*+*tasmin*)/2. The temperatures have again first been bias corrected using:

$$\Delta tasmax_m = tasmax_{cur_m}^{obs} - tasmax_{cur_m}^{mod} \tag{22}$$

$$\Delta tasmin_m = tasmin_{cur_m}^{obs} - tasmin_{cur_m}^{mod} \tag{23}$$

and:

$$tasmax_{m_t}^{cor} = tasmax_{cur_m}^{obs} - \Delta tasmax_m \tag{24}$$

$$tasmin_{m_t}^{cor} = tasmin_{cur_m}^{obs} - \Delta tasmin_m \tag{25}$$

with *m* being the respective month of the year, in Eq. 22 - Eq. 25...."

l335.
Figure2. It seems strange to switch region in the middle of the description from e) to f). It would be useful to continue with the first region to the end and add another figure for the second region if needed. For the validation of interpolated ice extent, it would also be good practice to show your reconstructed ice mask for a number of time slices through the deglaciation of the NH ice sheets.

Response: We use a different algorithm for the large ice sheets compared to the smaller ones (e.g. the Alps), as described in section 3.1. which is why we include a switch in region here.

l335.
Figure2. Fine to have a figure for the LGM, but it would be useful to also have a figure some time during the deglaciation. That is when interpolation to a different topography happens and eventual problems with the method could be inspected. I would suggest to document three time slices with figures similar to Fig2 for the LGM, halfway into the deglaciation and present day. Depending on how different they are, one or two could be pushed to a supplement.

Response: We already included an validation of the glacier reconstructions in section 4.3 with respective time slices for Dyke et al. 2004. We initially opted for Dyke to also highlight to the reader that the data from the CHELSA-TraCE21k model is global in extents, as otherwise our validation or plausibility test would be very euro-centric. We however added a comparison between our model, Stroeven et. al 2015 (https://doi.org/10.1016/j.quascirev.2015.09.016) and ICE6G and moved it to the Appendix S1 which shows the timesteps for which ICE6G, CHELSA-TraCE21k, and data from Stroeven et. al 2015 is available.

The results from the comparison confirm the results from North America, where over long periods the accuracy of the glacial reconstructions is above 0.8%, but clearly between 9 ka BP and 6 ka BP our model seems to create a larger mismatch towards these two datasets. We added a few new lines about these results.

REVIEWER 2
The latest version of the manuscript leaves much better impression when compared with the previous one. I appreciate authors' efforts to improve the manuscript based on both reviewers' suggestions. It obviously required a lot of work, but the manuscript now has significantly improved structure and consequently, notably improved readability. Also, a newly added validation chapter crucially contributes to the quality of the manuscript. However, I still find there is a space and need for further improvements, so my observations and suggestions in this regard are as follows:

Thank you for your comments. We have made the necessary changes you suggested.

1) The title does not correspond adequately to what is presented in the manuscript. It seems to me that the data set is developed with intention to be used in paleo-ecology (at least the introduction indicates so), therefore, there is no reason not to put that into the title. Also, I would remove "V1.0" from the title, I find it useless. I would rather put "highresolution data set" or even more specifically: "1-km data set" if that is something what will differentiate it from other potential similar datasets. A suggested title could be:

Response: The omission of v1.0 is certainly possible. I would however not put 'development, validation, and application in paleoecology' into the title. The use of this data is not restricted to paleo-ecology, but might also be useful in other fields.

"CHELSA-TraCE21k high-resolution (or "1-km") data set - downscaled transient temperature and precipitation data since the last glacial maximum – development, validation and application in paleo-ecology", or something similar, maybe shorter. If you want to maintain the current title, then I am afraid you would have to change the bigger part of the introduction.

Response: See our comment above.

2) It was really an unfortunate decision to name equally both the data set and the algorithm/model - CHELSA V1.2. Regrettably, I don't see significant progress in clarification in that

context throughout the manuscript. Only between the lines 60 and 121, a reader can find the next phrases: “CHELSA V1.2 algorithm”, “CHELSA V1.2 climate data set”, “CHELSA V1.2 mechanistic downscaling model”, “CHELSA V1.2 procedure”, “CHELSA downscaling model”, “CHELSA V1.2 model”. It is just unacceptable and extremely confusing. I find it necessary to add a paragraph, for example, somewhere at the beginning of chapter 2, clarifying that and informing the reader there are the dataset and the algorithm/model with the same name. It has to be very clear. And try to use only 2 words in its description throughout the text, for example
“data set” and “model/algorithm”. In addition, once again, please, maintain consistency throughout the text and decide whether you want to use “TraCE21k” or “TraCE-21k”.

Response: We tried to be more consistent and called all CHELSA ‘data’ as data and when we talk about the Algorithm, we consistently now say ‘algorithm’.

TraCE-21k is the CCSM3 simulation output at a course resolution.
CHELSA-TraCE21k is the downscaled output

This is already consistent in the manuscript

3) In abstract, you say:”High resolution, downscaled climate model data are used in a wide variety of applications across environmental sciences”. Then in chapter 6, you start with:”Transient long-term climatic data have a wide range of possible applications”. Please, add 2-3 examples where exactly, for example, just continuing the sentence by:”..., such as A, B and C, for example”.

Response: We added some studies where the data has been used already.

Transient long-term climatic data have a wide range of possible applications, ranging from population genetics (Leugger et al., 2022; Yannic et al., 2020), community ecology (Staples et al., 2022), to evolutionary biology (Cerezer et al., 2022), just to name a few.

4) Are there other comparable downscaled data sets available on the market? If yes, then, what is the advantage of CHELSA-Trace21k, why is it different/better then the other ones, why it should deserve attention, why is it unique? Please, clarify in a sentence/paragraph, in conclusion, for example.

Response: That is easy to answer: None at 1km.

We added:

Validations show that CHELSA-TraCE21k V1.0 dataset reasonably represents the distribution the distribution of temperature and precipitation through time at an unpreceded 1km spatial resolution

5) Before submitting a final version of the manuscript, I suggest a thorough inspection regarding the consistency of the use of terms throughout the manuscript, as well as typing and other errors

Specific comments:

Line 8: High-resolution

Response: Changed

Lines 9-10:
Here we introduce a new, high-resolution dataset, named CHELSA-TraCE21k. It is obtained by downscaling TraCE-21k data, using CHELSA V1.2 algorithm with objective to create global monthly

climatologies for temperature and precipitation at 30-arc sec spatial resolution in 100-year time steps for the last 21,000 years.

Response: Changed

Line 18:
Validations show that CHELSA-TraCE21k V1.0 dataset reasonably represents the distribution...

Response: Changed

Line 27:
GCM states for general circulation model, not global coarser grain=>coarser resolutions

Response: Changed

Line 34:
has been bridged, or had to be bridged

Response: Changed

Line 59:
Here we present paleo-climatic data, downscaled from the CCSM3_TraCE21k model output (or TraCE21k dataset) to a 30-arc sec. resolution using the CHELSA V1.2 algorithm
Response: Changed

Line 67:
in various parts of the world from

Response: Changed

Line 68:
The TraCE-21k simulation has T31_gx3v5 resolution (.....)

Response: Changed

Line 70-71:
Which resolution??
Please, specify the resolution per model component, for example CAM has resolution 3,75°x3,75°, which is important information for this manuscript, because you are downscaling atmospheric variables

Response: Changed: The TraCE-21k simulation output has a T31_gx3v5 resolution

Line 76:
It includes mean monthly daily 2m mean, minimum, and maximum temperature ?!

Response: Changed

Line 78:
ERA stands for 'ECMWF Re-Analysis'

Response: Changed

Lines 332-335:
Figure 2, axis labels not very clear, should be improved

Response: It is clear in the high res. vector graphic so we assume it will be fine in the final publication.

Line 361, 364, 373:
RMSE, not RSME

Response: Changed

Lines 444-450:
Figure 6, a) and b) missing on the figure; a) is also missing in the legend

Response: Changed

Line 455:
...if the transient...

Response: Changed

Line 498:
Separate Figure 7 legend from the text below

Response: Changed