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: Seemed ok with manuscript and 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 4.1. but ignored by the reviewer. We include a detailed response to the critique below. Were the reviewer had a valid point, we incorporated the 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. 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. We already replied that the model does not include bedrock changes but only sea level changes in our previous response. There is very little we can do about this at this point. It might be included in a newer version of the algorithm if we choose to rerun and improve the model or if it gets picked up by the community. We are aware that this model has limitations and cannot be perfect in every regard.

Unfortunately we cannot find your concerns about using temperature as an additional variable in the glacier ice sheet retreat interpolations in your specific comments, so we cannot comment on that matter. Additionally, it seems like the validation of the glacier section 4.3 seems to be largely ignored unfortunately too. As

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

Specific comments
---

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.

The model setup is nothing we did here. We did not run the TraCE-21k simulations, so its not appropriate to repeat the methods here, which would be out 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? Why is this important here? We 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 is based on a misunderstanding from the reviewers side. We do not change the bedrock topography, and that it is 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."

Why the correction of topography with sea levels does not make sense to the reviewer eludes us, since we did have major changes in sea levels over the last 21k years. That the model applied does not contain bedrock changes we already explained, and there is unfortunately nothing we can do about it at this point, except for mentioning that this effect is not included. We do not see how this is a problem. As an example: Most global climate models do not resolve convective precipitation. But that does not mean that these model are 'flawed', just because it does not include all possible processes.

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: We are not sure what you are missing here. The whole purpose of this section is to describe how we derived the paleo-orography. There are two additional figures even (Fig. 1, Fig. 2) that highlight the process. The equations are all given ('the way it is done'). What exactly is missing here?

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*). We don not see why it would be necessary (or even correct) to remove the time dependence here.

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: Why is section 4.3 ignored here? "Validation of glacier extent between 18ka BP and 1ka BP" ? Has this entire section been ignored?

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°. Why is this a problem?

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 already included an validation of the glacier reconstructions. Why is yet another one needed? We already show deglaciation on the north American ice sheet in Figure 6. that is the basis of our validation. It is not clear to us why this section consistently ignored in this review?

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:

$$\Delta tasmax_m = tasmax^{obs}_{cur_m} - tasmax^{mod}_{cur_m} \quad (22)$$

$$\Delta tasmin_m = tasmin^{obs}_{cur_m} - tasmin^{mod}_{cur_m} \quad (23)$$

and:

$$tasmax^{cor}_{m_t} = tasmax^{obs}_{cur_m} - \Delta tasmax_m \quad (24)$$

$$tasmin^{cor}_{m_t} = tasmin^{obs}_{cur_m} - \Delta tasmin_m \quad (25)$$

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: Why is this strange? 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.

With respect to good practice: We already show deglaciation on the north American ice sheet in Figure 6. that is the basis of our validation. It is not clear to us why this section consistently ignored in this review. Additionally the entire data is freely available, so anyone who want to inspect it more thorough is free to do so.

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 show deglaciation on the north American ice sheet in Figure 6. that is the basis of our validation. It is not clear to us why this section consistently ignored in this review?

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