Response in Red

Report #1:

The authors present a modelling reconstruction of the northern branch of the Patagonian Ice Sheet (PIS) during the last deglaciation, precisely from the last glacial maximum (LGM) to 10 ka ago. The ice-flow model ISSM is used for this purpose. The exercise makes use of a transient climatology for the last deglaciation (TraCE-21ka) to reconstruct both the glacial state of the ice sheet and to simulate the early stage of the deglaciation. The results are then compared with reconstructions available for that region (PATICE - Davies et al., 2020). Ice-climate interactions, sensitivity on the employed climate model, and goodness of the model results are then discussed.

I would like to extremely thank the authors for the exhaustive response to the criticisms that I and Reviewer#2 raised in the first round of reviews. I see there has been a huge effort in terms of clarification of the methodology and corroboration of the conclusions, improved by additional model experiments to investigate the thermal state of the PIS and the response of the ice sheet to precipitation changes throughout the last deglaciation. Also, I would like to thank the authors for the clarification regarding the GIA forcing prescribed in their simulations, which was one of my major concerns. I think that the paragraphs added in the “Limitations” section and that in the supplementary material clarify well this aspect.

The results are sound and the manuscript is well written. I therefore suggest the publication of their manuscript in TC after minor corrections, that I note down here:

We would like to thank the reviewer again for their thorough assessment of our work and especially thank the reviewer for agreeing to reassess the changes made to the original draft. We feel that the reviewer’s comments and edits made following their recommendations have greatly improved our manuscript.

Prescribed GIA
The authors claim that “the prescribed GIA is in good agreement (Figure S2) with a reconstruction of relative sea level change from an insolation basin in central Patagonia”. I believe the authors but I don’t see any comparison in Figure S2. Could you please show this agreement by comparing the two RSL curves?

Certainly. We have updated our supplemental Figure S2. There we added notation denoting the total uplift reconstructed by Troch et al., 2022 as well as the rates of uplift reconstructed by Troch et al., 2022 between 16.5 ka and 9 ka and 9 ka and 0ka, and compared this to the prescribed rates of uplift in our study.

LGM climate description
I don’t see the point of describing the LGM temperature and precipitation anomalies from TraCE21-ka in section 3.1 of the results, since these are not an outcome of the ice-flow modelling simulation, rather an output from CCSM3. I would rather shift Figure 3 and its description in the main text to section 2.4, where the climatic forcings are described. Also, it would be interesting to see the anomalies for other time snapshots during the deglaciation (e.g. 17 ka, 15 ka, 12 ka). By doing that, the description of the climate at the LGM would gain more interest as compared to other climates throughout the last 20 ka and it would add a spatial information to Figure 7 for the sensitivity tests.

Thanks. We have updated Figure 3 by moving it up to section 2.4 and have added anomaly maps for 17 ka, 15 ka, and 12 ka. We updated some text to reflect these changes in section 2.4 and the figure caption, as well as text in the section 3.2.2 and 4.1.

Sensitivity tests
I think it would be easier for the reader that the name of experiments 1, 2 and 3 directly be called the precipitation change that is applied in the sensitivity test. I would suggest to name these experiments as “prec_PI” (instead of experiment 1), “prec_12ka” and “prec_LGM”, or something alike.

We agree. We have changed the experiment names to: Precip. PI, Precip. 12 ka, and Precip. LGM.

Mathematical expressions

This is a technical comment, but please, be careful on how you write the equations in the Methods section. Many vectors miss the proper notation (e.g. basal shear stress and basal velocity in eq. 1; frontal velocity, horizontal velocity and normal vector pointing out from the calving front in eq. 5; horizontal velocity in eq. 6).

Done

Specific comments:

P7 L206-209. I would also cite Figure 4 to give a spatial overview of this agreement

Done.

P7 L235. I believe the reference for the level set method should be Choi et al., 2021, equation 3 (Equation 3 from Bondzio et al., 2016 is the ice thickness equation)

Thanks for double checking. Consulting with other ISSM team members, the levelset method is indeed introduced in Bondzio et al., 2016, so we prefer to stick with that citation.

P7 L245 and P8 L251. Please change tensile "strength" to tensile "stress".

Done

P7 L246-252. I would rephrase that as "… is the maximum stress threshold which has separate values for tidewater and floating ice, namely 1 MPa and 200 kPa respectively. The ice front will retreat if the von Mises tensile stress exceeds the user defined stress threshold.". Also, is it really needed to specify the threshold for floating ice? I don't think that ice shelves are modelled in your domain.
Thanks for the suggestion. We have updated the text accordingly, but kept the floating ice stress threshold in there for completeness since it was set in the model runs despite no to limited floating extensions forming.

Figure 3. Please add "The bilinearly interpolated LGM summer (DJF)…" to the caption.

Done.

P8 L267-274. As I mentioned in the general comments, I would shift the LGM climate description to the section about Climate Forcings.

Done.

P9 L274-278. If I understand correctly the position of SWW simulated in TracE-21ka at the LGM is in contrast to what is found in Kohfeld et al., 2013 and Moreno et al., 2015, who simulate a glacial equatorial shift of the SWW. I think you describe well this discrepancy in P17

Thank you. Yes, you are correct and we address this in the discussion.

L 508-527, but could you add a sentence about this also in the introduction when you comment about latitudinal changes in SWW? You could also remove P9 L 274-278, since you tackle this point well in the discussion.

We are not 100% certain if the reviewer is asking us to note in the introduction the relatively good agreement between TraCE-21ka and paleoproxies across the CLD. If so, we would like to leave the introduction text as is, as to our knowledge, our manuscript is the first to directly compare TraCE-21ka qualitatively against paleoproxies of precipitation in the CLD. Regarding L274-278. That is a good point and we have removed this text.

P9 L283. Please add "(Figure 4)" at the end of the sentence.

Done

P9 L288. Please add "(Figure 10)" at the end of the sentence.

Done

Figure 7. Could you add a shadow area to highlight the time intervals at 12-12.5 ka and between 22 and 20 ka?

Done. And we added text in figure caption: "Intervals of time used in the sensitivity test are highlighted by gray shading."

P12 L379. I believe for experiment 2 you mean experiment 1, but I would rather suggest to name this experiment as "prec_PI" or something alike.

Yes, thank you. We have corrected this and used your recommended naming.

Figure 8. Experiments 2, 3 and 4 should be 1, 2 and 3, or "prec_PI", "prec_12ka" and "prec_LGM".

Done.

P13 L390-398. I would restructure this paragraph so that you first describe experiment 2 (or "prec_12ka") and then experiment 3 (or "prec_LGM").

We have restructured following your recommendation. The text now reads: "For experiment *Precip. 12 ka*, winter precipitation is reduced by up to 7% (Figure 8B) relative to the preindustrial across the model domain (Figure 3 and 7). In this experiment ice retreats faster across most of the CLD, from the ice margins and through the interior. Deglaciation along the margins occurs >1 kyr faster in many locations, and between 200 yrs to 1 kyr faster across portions of the ice interior. For experiment *Precip LGM*, winter precipitation is increased by up to 10% (Figure 8C; *Precip LGM*:) across the northern portion of the model domain (north of 40°S) relative to preindustrial, but is similar to preindustrial values across the southern portion of our model domain (south of 40°S). In this experiment, with the imposed higher precipitation across the northern portion of the model domain, ice retreats slower during the last deglaciation relative to our standard simulation by >1 kyr, and in some locations up to 2 kyr."

Figure 9. Could you choose another colour for the simulated ice extent at 15.1 ka (e.g. orange, yellow)?

We prefer to keep the current colormap. We tried many combinations and this seemed to be the best visually.

Figure 10. Could you please add a line at 15.7 ka or a symbol to facilitate the comparison with the PATICE reconstruction at 15 ka?

We have added a green rectangle to highlight the simulated ice area at 15.7 ka.

Report #2:

Paper Summary:
Cuzzone et al. present a numerical ice sheet model reconstruction of the Patagonian Ice Sheet in the region of the Chilean Lake District (CLD), focusing on the LGM and subsequent deglaciation. The CLD is an area that is generally lacking empirical data and this work represents a meaningful contribution to our understanding of the drivers of ice evolution following the LGM. The authors use the model ISSM to simulate Patagonian Ice Sheet evolution in the region which they combine with the TraCE-21ka climate simulation. They use this to: 1) provide constraints on the nature of CLD ice retreat and 2) assess the possible key climatic controls on ice evolution during deglaciation. They provide a detailed methodology and the results of their experiments are clear, highlighting the role of precipitation in modulating ice recession during deglaciation. The authors highlight uncertainties in the model and justify their choices in model setup and design (e.g., GIA, calving).

The authors have been very comprehensive in addressing previous comments on the manuscript. I only have minor comments on the resubmitted manuscript with reference to the methods and results, which are clear from my viewpoint. The discussion in relation to climate is also clear. The majority of weaknesses that remain relate to the discussion, where the descriptions, interpretations, and comparisons to empirical data need to be re-phrased or clarified- some points just do not make sense (e.g., discussion of ice retreat and limitations). I have provided directions for the authors in relation to those. Once these minor corrections are complete (which shouldn't take the authors long at all) I recommend this work, which represents a substantial contribution to our understanding of glacier-climate interactions in the CLD, be published.

We greatly appreciate the reviewer for their thorough remarks and comments, support for our work, and lastly for stepping in to review this 2nd submission. And thank you for clarifying some of our misunderstandings with the regional geomorphology.

Comments on the Manuscript
Introduction:
Line 51: '…across an area presently known as the Chilean Lake District'- add latitude and longitude for CLD area here. This is helpful context, particularly as you are later comparing to the SWW.

Done.

Lines 57/58: + and outwash plains, though I think a lot of constraints in this region are also from sedimentological work on exposures, whereas constraints in the east of the CLD are extremely lacking. Maybe add 'only… well constrained'.

Done.

Line 59/60" I had to read this a few times as you have just said the LGM limits are well constrained in the SW/W, maybe add: '… due to a lack of geomorphological and geochronological constraints on ice-margin change following the LGM, the reconstructed deglaciation remains highly uncertain'.

Done.

Line 63/65: Maybe best to throw a line explaining the Southern Annular mode in this paragraph here? Not everyone will be familiar with regional + SH climate(drivers).

Certainly. We have added some text here: "The wintertime climate across South America is strongly influenced by the southern annular mode (SAM; Hartmann and Lo, 1998), by which its phase and strength is regulated by changes in the difference of zonal mean sea-level pressure between mid (40°S) and high latitudes (65°S). The SAM in turn modulates the strength and position of the southern westerly winds (SWW) over decadal to multi-centennial timescales, which exert a large control on the synoptic scale hydrologic and heat budget (Garreaud et al., 2013)."

Methods/Results:
The methods and results of this work have been outlined very clearly and resolved in previous iterations, and are combined with a very detailed supplemental document. Just a few clarifications here:
Line 234: 'We simulate calving where the PIS interacts with ocean.' -but not with lakes, add brief sentence so this is clear early on?- I know this is justified later, but here I wondered why it wasn't included given substantial numbers of lakes across the 'Chilean Lake District'.

We have modified the text here: "We simulate calving where the PIS interacts with ocean, but do not include any treatment of calving in proglacial lakes (see section 4.3)."

We recognize that it would be nice to have included some treatment of calving in proglacial lakes. However, as discussed in prior response to reviewers and in section 4.3, this methodology within ice sheet models is still novel and not well established. It is the subject of some current work with ISSM.

Lines 330-333: What do you mean by deglaciation? Significant re-advances would imply a punctuated net retreat, versus small glacier fluctuations we would expect to see on an annual basis as a result of seasonal differences in mass balance? Maybe just define what you mean by deglaciation here, as interpretations do vary.

Currently in the text, we define deglaciation as the point in time when the grid cell becomes ice free. However, from that definition and due to possibility of readvance, a particular grid point could experience deglaciation multiple times (readvance/retreat). Therefore, we take the youngest age of when a grid cell deglaciates (or ice retreats from). We have modified the text as: "From the resulting transient simulation, we calculate the timing of deglaciation across our model domain (Figure 5) as the youngest age at which grid points become ice free."

Discussion:
Lines 440-441: '…as limitations in paleo-proxy data and disagreement between climate models prohibit certainty'- '…due to disagreement between paleo-proxy data and climate models'? Or something like this, I struggled to follow.

We have clarified this as: "Determining the influence of the SWW on the heat and hydrologic budget across South America during the LGM and last deglaciation remains difficult, as paleo-proxy data is limited and climate models tend to disagree on the evolution of the SWW (Kohfeld, 2013; Berman et al., 2018)."

Lines 443-444: '…linking the paleoclimate change in SWW position and strength from regional paleoclimate proxies remains problematic (Kohfeld et al., 2013).'- again, this was hard to read. What do you mean, re-phrase?

We have changed this sentence to: "And while paleo-proxy evidence does suggest wetter conditions across the CLD during the late glacial (Moreno and Videla, 2018), linking this variability to changes in the position and strength of the SWW remains difficult (Kohfeld et al., 2013)."

The comparison to climate is detailed, but the discussion of ice retreat and the limitations section can definitely be built upon:
Line 552: Particularly in the CLD- as you have highlighted?

Yes. We have adjusted:" Because geochronological constraints on past PIS change are limited, particularly in the CLD, the PATICE reconstruction assigns qualitative confidence to its reconstructed ice margins."

Lines 559-560: Across the CLD, the LGM ice extent is well constrained by geologic proxies particularly in the west and southwest- clarify the timeframe from the panels in Figure 1.

We have adjusted the text: "Across the CLD, the LGM (25 ka, 20 ka) ice extent is well constrained by geologic proxies particularly in the west and southwest (Figure 1)."

Lines 560-563: 'The moraines that constrain the piedmont ice lobes that formed along the western boundary are now presently lakes and have reasonable age control (Denton et al., 1999; Moreno et al., 1999; Lowell et al., 1995), giving confidence to the LGM ice margin limits.'- How can moraines be lakes? I know what you are trying to say, but this needs re-writing. These north-western outlet glaciers deposited moraines that lay west of the overdeepenings that are occupied by modern lakes. Clarify.

We have clarified by adjusting the text: "The moraines that constrain the piedmont ice lobes that formed along the western boundary have reasonable age control (Denton et al., 1999; Moreno et al., 1999; Lowell et al., 1995), giving confidence to the LGM ice margin limits."

Lines 571-572: '…cosmogenic nuclide surface exposure date retrieved from the Nahel Huapi moraine yielded an age of ~31.4 ka (Zech et al., 2017).'- Where is this moraine? Is it on a figure? We need some idea where it is, even coordinates or a dot on a map somewhere.

We have updated the text here with some coordinates:

“For instance, a single cosmogenic nuclide surface exposure date retrieved from the Nahuel Huapi moraine yielded an age of ~31.4 ka (Zech et al., 2017; 41.04° S, 71.15° W).”

Lines 578-581: ‘In regards to ice area and extent, our simulated ice sheet at the LGM using TraCE-21ka climate boundary conditions agrees well with the PATICE reconstruction (Figure 10). Our simulations reveal that deglaciation began between 19 ka to 18 ka, consistent with the geologic proxies (Davies et al., 2020)’.- Can we add a line or two of comparison down the PIS as this is your discussion? This is similar to the Lago Palena/General Vintter (Soteres et al., 2022), Corcovado (Leger et al., 2021, QSR) and Cisnes (Garcia et al, 2019) glaciers southeast of the CLD (~43-44 deg S). Is it different to what is going on even further South (LGC/BA or Tierra Del Fuego)? Or on Isla Chiloe? Worth a mention at least of similarities and differences along the transect of the Andes here. (Note that this is a different Leger paper to the one you mention, same year).

We have updated the text to reflect the recommendations of the reviewer:

“In regards to ice area and extent, our simulated ice sheet at the LGM using TraCE-21ka climate boundary conditions agrees well with the PATICE reconstruction (Figure 10). Our simulations reveal that deglaciation began between 19 ka to 18 ka, consistent with the Davies et al. (2020) reconstruction. Notably, the simulated timing of deglaciation agrees with moraine records further south on the eastern side, such as in Río Corcovado (~43° S, Leger et al., 2021a), Río Cisnes (~44° S, Garcia et al., 2019), Lago Palena/General Vintter (~44° S, Soteres et al., 2022), and Río Ñirehuao (~45° S, Peltier et al., 2023). On the other hand, glaciers are thought to have withdrawn from their LGM position later between ~18 - 17 ka on the northwestern margin (~41° S, Denton et al., 1999; Moreno et al., 2015), in the southern (~46° S, Kaplan et al., 2004), and southernmost regions (~52° S, McCulloch et al., 2000; 2005; Kaplan et al., 2008; Peltier et al., 2021). The simulated ice retreat continues until 15 ka, with the largest pulses in ice mass loss occurring at 18.6 ka, 16.8 ka, and 16 ka (Figure 6).”

Lines 584-585: ‘After 15 ka, mountain glaciers remain in our simulation but there is no presence of a large ice cap as reconstructed in PATICE’- there is no data really at all for this time slice in PATICE. I think you can highlight this at various points here, more data is needed in the CLD to better evaluate this and future models. You have done what you can to evaluate this with available empirical data.

We agree, and have made attempts throughout the manuscript to inform the reader of the lack of data. The initial reviewers question why the model diverged from PATICE, but did not necessarily realize (from the text in the first submission), that PATICE is very uncertain and therefore is not a reliable constraint later in deglaciation. Here we add: “Comparison between the model simulations and PATICE becomes difficult during the 15 -13 ka period as confidence in the geologic reconstruction is low due to a lack of geochronological and geomorphological constraints on past ice history.”

Line 588: ‘…region largely retreated by 15 ka, with only mountain glaciers remaining.’ There is support for this further south. E.g., ice had receded enough for Atlantic-Pacific drainage routes to open east (and drainage reversed) by at around ~16.3 ka (e.g., Leger et al., 2021; QSR; ~44 Deg S)- which supports that the ice sheet had begun to unzip back into smaller ice caps as you show here. Would this not also support the same occurring further north (or even earlier possibly) in the CLD?

We have updated the text here: “This is supported further south, where the ice sheet disintegrated at ~16 ka with paleolake draining to the Pacific Ocean (~43° S, Leger et al., 2021a) and the ice remaining limited to higher mountain areas”

Lines 594-595: ‘This potential for a favourable and prolonged period of glacier growth is likely missing in our simulations during the ACR, which may explain some of the mismatch against the PATICE reconstruction at 15 ka – 13 ka’. I realise you have a short ACR, but there are not constrains on these time slices in PATICE. You can probably remove or re-phrase this, and highlight that the mismatch is hard to assess due to data paucity here. This is generally a Patagonia-wide issue (we need more constraints).

We agree, but added these statements to address the comments of reviewers during the first submission. Since we address the scarcity of data in PATICE above these lines, we decided to change the text to: "This potential for a favorable and prolonged period of glacier growth is likely missing in our simulations during the ACR."

In those regards we acknowledge that the simulations may miss a window of potentially favorable glacier growth (which we think readers may find interesting), without again comparing our results during this interval to PATICE which is uncertain.

Limitations section of Discussion:
There are a few points when referring to the geomorphology that this section is a little muddled- here are a few (minor) points to clarify:
Line 616: Re. evidence- are the moraines not comprised of glacio-tectonised outwash (check paper)? Be specific with the evidence. Just provide the details mentioned in the paper here. 'Moraines formed of glacio-tectonised outwash provide evidence for…'. This is helpful context (and it will keep the geomorphologists happy).

Yes, Bentley 1996 describe glacio-tectonized outwash. We have adjusted the text: "Across most of our domain, moraines formed of glacio-tectonized outwash (Bentley, 1996) provide evidence for an advance of piedmont glaciers across glacial outwash during the LGM, which formed the physical boundary for some of the existing terminal moraines around the lakes within the CLD (Bentley, 1996; Bentley, 1997)."

Lines 617-619: 'The formation of ice-contact proglacial lakes likely occurred as a function of deglacial warming and ice retreat (Bentley, 1996)'. Well, kind of, but really it is due to mass loss combined with the underlying bed topography (restricting proglacial drainage). Not all glaciers have lakes, it is a product of retreating into their overdeepenings where meltwater becomes trapped. Re-phrase? E.g., examples outlined in recent papers by Dave Evans (Iceland).

We have adjusted the text following your recommendation: "The formation of ice-contact proglacial lakes likely occurred as a function of deglacial warming as ice retreated into overdeependings in the bedrock topography and filled with meltwater (Bentley, 1996)."

Lines 620-624: This doesn't really make sense: 'During deglaciation, iceberg calving into the proglacial lakes may have occurred (Bentley 1996,1997; Davies et al., 2020), with evidence suggesting that local topography and calving may have controlled the spatially irregular timing of abandonment from the terminal moraines surrounding the proglacial lakes (Bentley, 1997).
1) If you have an ice-contact lake, you have calving in the ablation area… re. '…iceberg calving into the proglacial lakes may have occurred'. This is not necessary; remove or re-phrase point.
2) Surely calving cannot control the glacier abandonment of the terminal moraine positions surrounding the proglacial lakes, because the glaciers had not yet developed lake? Am I missing something? Please clarify/re-write this point so that it is clear.

Thank you for pointing out this misrepresentation. We have adjusted the text following your recommendations:

"During deglaciation, proglacial lakes formed along the ice sheet margin (Bentley 1996,1997; Davies et al., 2020), with evidence suggesting that local topography and calving may have influenced the spatially varying retreat rates along these margins (Bentley, 1997)."

Lines 627-629: 'However, across our region, Heirman et al. (2011) indicate that it is not well constrained how the proglacial lakes in the CLD may have influenced local deglaciation, as more geomorphic data is needed'. It's true that we do not have geomorphological data, but a lot is probably at the base of these lakes (if at all) and would require bathymetric surveys. There has been some work done on glaciolacustrine varve sediments around Lago Buenos Aires (see: Bendle et al., 2017/2019 (?) papers) supports a period of rapid recession following the onset of calving as glaciers retreated and formed lakes in their overdeepenings. Maybe reference some of this work? It builds on these points.

Again, thank you for boosting our geomorphology knowledge for this region. We have taken a look at the papers mentioned, and adjusted our text as follows.

“However, it is not well constrained how the proglacial lakes in the CLD may have influenced local deglaciation (Heirman et al., 2011). While more geomorphic data is needed, recent work south of our study region (46.5˚S) reconstructed early deglacial ice retreat using a glaciolacustrine varve record from Lago General Carrera-Buenos Aires (Bendle et al., 2019). The authors find that following initial retreat due to deglacial warming, the ice margin retreated into the deepening proglacial lake which accelerated ice retreat in this region due to persistent calving, therefore supporting the role proglacial lakes likely played across the margins of the retreating PIS during the last deglaciation.”

Figures:
Figure 1: Generally, I would make sure that maps have a north arrow, scale bar, and grid (latitude/longitude). Not necessary for all panels, but at least one to provide context.

We have added a scale bar and North arrow to Figure 2, which is shows our model domain across the CLD and bedrock geometry. We hope this will add better context for the reader when considering our figures.

Figure 5: I am struggling to follow this description. Is it not dark blue where ice is persisting after 10 ka? Is something wrong here either in the caption or the figure? Please clarify and fix if needed.

The dark blue denotes deglaciation between 11 ka and 10 ka. Our saturated color (<10 ka) is the gray color, for ice cover persisting <10 ka is assigned.

I hope that my comments are of some use to you and I look forward to seeing this published in the near future.

Thank you so much for your comments. Together with the other reviews, we are grateful for the constructive feedback as it has surely made this a stronger paper.