Reviewer 1 comments:

The authors describe a new 10Be chronology (supplemented with some 26Al data) for three old (pre-LGM) packages of glacial deposits stemming from past glaciation of the northern Patagonia Ice Sheet. These authors published ages previously from the innermost five ice margin positions, which is the LGM and deglacial sequence. Hence, this paper is a companion paper to the earlier one (Leger et al., 2021) published last year.

The strength of the paper lies in solid glacial-geologic mapping, and exposure dating chronology applied on moraine boulders and outwash surface cobbles. In one of the three pre-LGM drift units that are the subject of this paper, both moraine boulders and outwash cobbles are dated. In this case, the surface cobble age groups are tighter than moraine boulder age groups. The cobble ages are interpreted as being impacted by processes that can make them younger than true age (via surface deflation and/or cryoturbation), but not by processes (e.g., inheritance) that would result in them being too old. The logic behind this rests on an argument that they are well rounded (ie, eroded) and well down valley from their potential source, and on a prior study from elsewhere with sediment profile data. Based on these assumptions, the authors then lean heavily on the oldest cobble age as their best estimate for the timing of outwash deposition for the three drift units that are the focus of the study. The boulder ages (from the youngest of the three drift units) supplement its cobble ages after removing some outliers. And in the middle-aged drift unit, there is a 10Be age from an ice-sculpted bedrock surface that constrains deglaciation following that drift unit's deposition.

The best estimates for the age of the three drift units are MIS 8, MIS 8 and MIS 6. There is some arm waving about possibly a glacial advance during MIS 7 that I suggest below be removed. This study contains probably the best constraints showing MIS 6 glaciation of Patagonian ice. Combined with prior work, it reveals a lack of MIS 4 moraine preservation.

This is overall a great manuscript. It is written well and presented well with good figure and table support. I have some bigger picture comments followed by some detailed comments that are meant to simply improve the manuscript further. In several places I think the authors take too much liberty to discuss topics that deviate from the realm of their dataset, or arm wave too much given uncertainties in the dataset.

Comments

- Evidence for MIS 7 glaciation. This rests on a single bedrock 10Be age, and maybe one old outlier age on the RC II (MIS 6) moraine. Neither age is trustworthy by itself; there are many possibilities to get those apparent ages without there having been an MIS 7 glacial event. The evidence is sufficiently thin that I suggest removing mention of this possibility. One could argue that this is already a fairly long paper, and sections like this just dilute, maybe even take away from, some of the stronger results. This is a subjective comment and may be personal style, but I would advocate for a stronger paper that stays more true to what its results can robustly support.

Author's reply:

We understand the reviewers comment and agree that the evidence is thin for the former occurrence of an MIS 7 ice-sheet expansion event. This is why, in the orginigal manuscript, we have discussed the possibility of such event at our study site as hypothetical and requiring more investigation. However, we agree that writing an entire discussion paragraph on this event brings unjustified complexity to the manuscript. We have thus decided to remove section 5.2.2 from the manuscript and its associated interpretation from the conclusions. We however still think it is useful to briefly mention the possibility, using hypothetical language, that innermost RCI advances may have occurred during MIS 7 in discussion section 5.1.3, when talking about the bedrock sample.

- Pinning the chronology on the oldest outwash cobble age. Dating old glaciations is difficult. Dating outwash surfaces in this particular climatic environment is splendid, and prior sediment depth profile data along with other arguments (transport distance, channel preservation, cobble roundness) present a solid case for a reliable chronology. Yet the ages are spread out, some more than others. Nevertheless, to imply in the discussion that there is few-kyr uncertainty is too simplistic. This work is expensive and prohibits us from dating dozens and dozens of cobbles. But let's say for argument sake one did date dozens of cobbles from a single terrace surface, do you really think your population includes the oldest possible one out there? And, while arguments for ruling out inheritance are largely valid, can you really rule out that a cobble could now and again have been recycled? My point of making these comments is that a more realistic uncertainty of drift unit age should be considered in the discussion and conclusion sections. I understand it is difficult to quantify terrace age uncertainty with the "oldest cobble" method, but I suggest keeping a more realistic uncertainty in mind during the discussion.

Author's reply:

We understand and agree with the reviewer's comment. Given the uncertainties associated with TCN exposure dating of such old deposits, a few kyr uncertainty is unrealistic. The main findings of the paper lie more in the establishment of the timing of local glaciations that we can attribute to a certain MIS interval, and with time-window precisions that lie more in the 2 sigma standard deviation ranges (10-20 ka), which are large enough to take into account production rate uncertainties. However, while we think our oldest cobbles are the closest "estimates" of the timing of these glaciations, they may indeed still underestimate the true deposition age, and only collecting many more samples would help determine whether this intepretation is correct. With this in mind, we have decided to modify the text in the discussion and the conclusion, to make sure that we talk about the timing of these glaciations using more conservative time ranges, rather than the exposure age figures from the oldest cobble only. When talking about the timing of these glaciations in comparison with southern hemisphere insolation parameters and other palaeoclimate proxy records, we have added to the text that this entire discussion is based our own interpretation of the available chronological evidence, which while yielding high confidence for MIS 2 chronologies, yields rather intermediate and low levels of confidence for our MIS 6 and MIS 8 record, respectively. We have made sure to further stress that our discussion around the role of local seasonality and seasonal duration implies the assumption that such extensive PIS glaciations likely occurred during periods of maximum hemisphere-wide cooling, and thus when antarctic atmospheric temperatures reached their lowest values as well. We thus use the precision of the Antarctic ice core chronologies which display minima in local atmospheric temperatures that are included in our much larger dating uncertainty ranges, to discuss the palaeoclimate and link to insolation hypotheses. Although these assumptions yield uncertainties, we still believe that this discussion is important and contributes some new thoughts on the debate around the drivers of southern hemisphere and global glacial/interglacial cycles.

- Finally, last comment, there is a bit of discussion on drivers of SH glaciation. It is a good review of some recent ideas, but the topic is not heavily informed by the results from this paper, per se. Especially in light of our ability to date features this old. I guess I'm a bit neutral about having the text in the paper; it is a good learning experience for the author, but I did not find that the discussion adds a lot to this dataset.

Author's reply:

Regarding section 5.3 of the discussion. We agree that this section of the discussion could be shortened and made more concise. As explained in the previous comment reply, we have added some text to remind the reader that this discussion relates to observations that are based on several assumptions made in the paper. However, the hypotehses arguing for a southern hemisphere view on the possible drivers of southern hemisphere glaciations and of interhemispheric synchronicity in major glacial events are still fairly new ideas. We believe these ideas deserve to be mentioned and explained in order for them to have a legitimate place in the debate around Mercer's paradox. More work will be needed in the future to determine whether our interpretations were correct: but we still strongly believe these are worth talking about and will be of interest to the Quaternay glaciology and palaeoclimate community.

Line by line comments

40: When spanning… I agree with this statement, but only somewhat. It depends on chronological ability. The chronology as presented has a skewed sense of accuracy (see above), it hinges on the oldest cobble age and its individual age error. This likely vastly underestimates true landform age uncertainty. It assumes no possibility of inheritance. It assumes that if another dozen cobbles were dated, none would be older than the oldest already produced. Point being, it is darn difficult dating old glacial deposits, even in arid, stable areas. Thus.. do chronologies spanning multiple glacial cycles really have "the capacity to resolve conundrums on interhemispheric phasing of glaciations" when they can only be dated with uncertainties that are probably, realistically, in the 10s of kyr?

Author's reply:

We agree multiple sources of evidence are required over decades of research, and the term "resolve" is likely much too strong here. The sentence was changed to: "the capacity to contribute to knowledge on the topic of interhemispheric phasing of glaciations".

1. Bedrock samples? Or sample?

Author's reply:

The answer is "samples", as the term describes all samples mentioned in the sentence, including moraine boulders, outwash cobbles and the bedrock sample.

1. suggest not using the simple word "stage" and use Marine Isotope Stage in all cases.

Author's reply:

Changes were made accordingly

1. "enable to explore" reads awkward

Author's reply:

Change: "explore" was replaced by "investigate"

1. comma not semi colon

Author's reply:

Changes were made accordingly

1. suggest avoiding all acronyms. Why bother with them? Rarely are there word/page limits these days. It just makes the work more impenetrable (some have argued acronyms make our work less equitable). Why make readers unnecessarily remember stuff?

To reduce the amount of acronyms, we have removed the acronym for Southern Westerly winds (SWW). However, the field of Quaternary glaciology is well used to highly common acronyms such as LGM and LGC, and the PIS for Patagonia, which is used by all publications investigation Quaternary Patagonian glaciers. In the field of cosmogenic nuclide surface exposure dating, the acronym TCN is also very commonly used across the large majority of publications. Getting rid of these 4 common acronyms would increase the size of the paper by 1133 words. This number would increase to 1331 words if we were to remove the MIS acronym. Given the manuscript is already quite long, we feel removing these common acronyms wouldn't be appropriate.

1. replace 'further' with 'farther'

   Changes were made accordingly

1. it is important to note that even in cases with Al and Be in production equilibrium, it does not rule out entirely that it therefore is a "simple" exposure history

   Yes we fully agree with this comment and added the terms (within uncertainty) in brackets.

1. does this imply all boulders exhibited glacial polish? That would be something if 100 kyr and 200 kyr boulders retained primary glacial polish.

   The boulders sampled were deposited during the MIS 6 glaciation, around 140 ka. Boulders from older limits were subtantially more eroded and thus not sampled. On these boulders and in this eastern Patagonian semi-arid setting, rock surface erosion rates has been shown to be rather low: typically around 0.2 mm ka^{-1}. The samples we collected targeted smoothed surfaces sticking out from more weathered surrounding surfaces. These were present on all boulders sampled, chosen for this characteristic. We agree that homogenous granular disintegration must have taken place, even on the surfaces that resisted most to erosion. Thus the term "glacial polish" is perhaps not the most appropriate here. In order to be more specific: we replaced this sentence by: "*Where found, the top 2-5 cm of boulder surfaces exhibiting smooth rock fragments protruding from more eroded surrounding surfaces were sampled using hammer, chisel and angle grinder.*"

1. back to the polished boulders, if there are ventifacts around, why isn't polish by wind, and/or how could glacial polish survive in a landscape with such ventifaction?

   On the MIS 6 boulders sampled: the ventifacts systematically only occur at the base of the boulders: on their west-facing sides. This pattern, we believe, is due to sand particle entrainment by wind: which is denser below ~50 cm. Above a certain height, much less sand particles are entrained. The rounded nature of the boulders allow the top surface to be rather sheltered from wind compared to their sides, moreover. The ventifacted surfaces on the sides of the boulders do not present polishing, but always show distinctive parallel grooves where found. We do not find those grooves on the sampled boulder surfaces. We thus believe the top rounded surface of the boulders have retained some of their original ice-moulding, despite a certain amount of granular disintegration and surface weathering. Furthermore: this question is in fact the purpose of our experiment that aims at producing exposure ages from both boulders and surface outwash cobbles. Some boulder ages match the more reliable cobble ages: showing that minimal surface erosion has occurred on these boulders, which thus showed to be "good" samples. However, some boulders are much younger. Significant moraine erosion or boulder surface erosion has occurred in these cases, which is why we consider them as outliers and remove them from the dataset when interpreting the timing of the glaciation. We consider both moraine and boulder erosion scenarios in the manuscript discussion: and conclude that cobbles are indeed more reliable than boulders in this environment, if one wants to date such old deposits. This experiment by itself is the answer to this comment.

1. how much surface erosion is there if these things are ventifacts? I guess they are still rounded and don't appear "asymmetric" in their rounding (as if the top were eroded down)?

   Our answer to the previous comment covers this question. The sampled boulders are still rounded in the overall shape. In some cases, not all though, the west-facing side of the boulder shows higher signs of erosion than the other sides and than the top surface: due to the effect of wind and sand particle entrainment creating ventifacts.

1. wouldn't one option, maybe a better option, be to scale Al production rate to the Patagonia production Be rate using a known production ratio? If not, what is the ratio of doing it your way? That is, taking a 10Be rate from one study, and a 26Al rate from a different study. That seems like it might violate the production ratio thing, especially if authors use the production rate ratio (which they do) to argue for "simple" exposure history… Hmmm worth more thinking here.

   The reason for our approach is that there is no local production rate (and thus no known production ratio) established for in situ 26Al in Patagonia. The local production rate of Kaplan et al. (2011) only enables calibration of 10Be. Therefore, our chosen option, when analysing 26Al concentrations in Patagonian samples, is to calculate the ages using the global average production rate (Borchers *et al*., 2016). This, however, only applies to the final 26Al exposure ages reported in the table. The 26Al/10Be ratio analysis used to infer the presence of absence of complex exposure/burial histories does not take into account this difference in production rate, because these ratios are derived directly from the radionuclide concentrations in Quartz samples. The ratios are thus computed without calculation of the exposure ages, and thus without the use of a production rate.

Figure 2. I can't help but to be skeptical that these cobbles are exposed at the surface since deposition. no soil bio/cryo-turbation, no past sediment cover; presumably the current vegetation is not reflective of the 200 kyr exposure period given westerlies shifts and other climate changes? Were there ever trees here, are there paleoclimate or pollen records spanning

a long time? What's the evidence for the present climate/vegetation being representative of the last few glacial/interglacial cycles?

SWW have remained the dominant winds throughout the Quaternary in this part of the world. They represent the main source of precipitation at the latitudes of our study site. As we currently are in a warm interglacial period, more precipitation makes it to eastern Patagonia than during glacial periods: when the thick ice sheet was acting as an additional orographic barrier to moisture delivery from the west. According to proxy and modelled palaeoclimate data, seasonal precipitation was 40-50% lower than present at the LGM east of the Patagonian Andes (Berman et al., 2016). Moreover, the strong and persistent westerly winds (annual mean speed of~5.3 m s^{-1} at RC moraines location; WorldClim 2 data; Fick and Hijmans., 2017) are locally responsible for minimal annual snow and vegetation cover on protruding landforms, such as moraine crests (Hein et al., 2010; 2009; Mendelova et al., 2020a, 2020b). Indeed, climate model simulations have estimates that, despite northern migrated westerlies during colder, full glacial climate, the Eastern Patagonian foreland is thought to have been drier than today then, causing the local vegetation zone to be classed as "temperate desert", while today's vegetation zone is less arid: and considered a Steppe. This has been moreover suggested systematically by the PMIP experiments simulating LGM climate in the southern hemisphere: such as the latest PMIP 4 model output which suggests strong negative precipitation anomalies in eastern Patagonia at the global LGM (*e.g.* a relevant figure of the simulation is shown in Petherick *et al*., 2022[1]). It is therefore safe to assume that local vegetation cover was never likely to be significantly denser than today for extended period of time during the late Pleistocene, and this is supported by the limited soil thickness covering the glaciofluvial deposits we studied.

Regarding soil bio/cryo-turbation and outwash surface deflation: we definitely believe that these processes may have had an impact on our exposure ages, and we explicitly take those processes into account and make detail descriptions of their impact on our cobble ages in the discussion paragraphs regarding exposure age interpretations. This is the reason why we consider the oldest cobble age as a better minimum-age estimate, and why we also tested the impact of cobble exhumation on modifying the mean exposure age from each population by modelling the impact of cobble exhumation through soil on exposure ages. We feel like this component of the discussion is already well-developed in the paper."

E and F should be labelled with their RC unit designation, as the first panels are.

Changes were made accordingly

And why no age reported on panel F?

[1] An extended last glacial maximum in the Southern Hemisphere: A contribution to the SHeMax project - ScienceDirect

Because panel F is another photograph of the RC20-01 sample, as indicated by the labels, but taken from a different angle to better visualise the ice-moulding curvature of the rock surface. That surface is also shown on Panel E, with its exposure age.

suggest reporting Al ages too on these figures if they exist.

26Al ages were added to the picture according to this suggestion

1. these distances aren't true everywhere, maybe along a particular cross section. You can save trouble by not writing this and just referring readers to the figure.

   We agree: the sentence: "Along our sampling transect" was added the start of the sentence to make it more specific and clear.

1. "kettle kame" implies glaciogenic, no need to be wordy, remove "glaciogenic"

   Change were made accordingly

1. "sparse vegetation" see earlier comment, it is sparse today, but…

   See my reply from earlier comment. The evidence we have so far shows that it was overall likely to have been drier or equivalent to today.

1. maybe. maybe not. First, what is the expected ratio of Kaplan Be and Borchers Al?

   See previous reply on this. 26Al/10Be ratio does not take into account productions rates as they can be derived directly from radionuclide concentration. This differential production rate is only relevant to reported exposure ages in the manuscript tables.

Second, how long of burial does it take to have a statistically recognizable disequilibrium from the above ratio? Given error bars, probably well more than 100 kyr of burial is not detectable. Therefore, using this to confirm "continuous" history is too simplistic.

For such old ages, the minimum detectable burial duration from the 26Al/10Be ratio is approximately 100 ka. We have added the terms "within uncertainty" and a "prolonged and >100 ka period" to the relevant sentence and direct the reader to the relevant figure (SM fig 1) in the supplementary materials which displays the burial duration isochrone with labels.

1. It is important to add here, not only inboard of RC1, but also "and outboard of the RCII moraine"

   Agree: changes were made accordingly

Table 2. this is a little bit of a number soup. I think commas would help. Eg, 276,461. I've always thought there should be a convention in TCN like in 14C where things are rounded to nearest decade or century. Weird to see reported to single year…

Because this paper is accompanied by 2 other published articles in which we have followed the same CRONUS exposure age calculator age-report table formats as followed by most authors in the field, we feel it is important, for the sake of consistency, to report the ages in the same format here. This comment is however making a very good point, and this is something we need to take it into account in future publications, and at the scale of the entire COSMO community. A discussion at relevant conferences on this should be initiated.

1. Coming back to a comment I already made… I recommend adding an element to this section that transcends time. This climate data is relevant for the present, but really a discussion like this would be more relevant if it considered the oscillatory nature local climate on glacial-interglacial cycles.

   See my previous replies on this comment.

1. I suggest expanding this important section a little bit. I believe that a lot of people will react to seeing just surface cobbles being dated, so it would be worthwhile to spend more text justifying that approach. Suggest adding something like "Depth profile data reported by Heim et al (2009) revealed no inheritance in x age outwash gravels in x place. The distance of the terrace dating site to the bedrock valleys in the core of the range is x km, comparable to our study area. For these reasons, our age interpretations are based on similarly negligible inheritance in our study area."

   In agreement with this comment; the relevant text was expanded and modified to:

   "*For all outwash surface cobbles sampled, total rock-surface erosion is considered negligible due to same reasons as described for moraine boulder samples, but also due to the fluvially-rounded and polished nature of target cobbles. Such interpretation is further supported by the analysis of 10Be concentrations in a proglacial outwash depth-profile of MIS 8 - old sediments deposited more than 65 km east of the closest bedrock source region, in an eastern Patagonian setting similar to our study site (Hein et al., 2009). Results from this analysis indicate that nuclide inheritance is negligible in outwash deposits of the Río Blanco and Hatcher units, in the Lago Pueyrredón valley (47.5°S).*"

1. "minimal" or "non existent"? If "minimal" then some text lower down where oldest age is taken more face value would need to be re-considered…

   We have replaced the term "minimal" by "unlikely". As we are dealing with reconstructions of past events, which are inherently uncertain in nature, we ought to use probabilistic, or "conditional" terms to describe such processes. "non-existent" would be too certain. However, our exposure age distributions allow us to test these processes.

If inheritance had a significant impact: we would most likely see more scattered exposure age populations: especially in our surface cobble ages.

Figure 4B is awesome, probably the most important figure in this paper. It is refreshingly transparent about the chronology and provides full details. Nice.

It would be GREAT to have this figure along with some kind of global curve, LR04, for example. This, by the way, does not appear anywhere in this paper, but it should, after all it defines MIS boundaries used heavily in this work. I realize the "climate curves" figure comes later, but it is nice to have LR04 and to have it right next to these data PDFs.

We have added the LR04 climate curve next to the data in figure 4B. Many thanks for pointing this out.

A couple things seem a little weird in terms of data visualization, like how the bedrock age has a blue dot and an error bar, yet the blue dots representing the mean of the cobble ages does not have an error bar, and instead errors are given as vertical gray dashed lines. I think the blue dot with error bar is simpler. And why change it up all in the same figure?

We have modified the figure to make sure to consistently use blue dots and error bars in agreement with this comment.

Also why does the stand along dashed gray PDF curve of the oldest cobble have its own mean and error range? Can't imagine that is important.

We have removed that information from Panel B. However, we feel it is important to keep it on Panel A: as that way one can see immediately that the 2 sigma uncertainty of the outlier does not overlap with the 2 sigma range associated with the mean of the tecka outwash cobble population: which increases our confidence in interpreting that old age as a statistical outlier.

1. In the moraine dating world, boulder ages don't really date an "advance" but rather a "glacial culmination" or the initiation of deglaciation (which starts moraine stabilization). Do you think outwash terraces are the same? Hmmm. Just the use of the word "advance" here made me think…

   See reply further down to other comment of this nature.

1. not sure why the bedrock is described as RCI-II. It is RC I and only RC I, no? It is beyond the reach of RC II. Its surface age has nothing to do with RC II, right? To me this labeling confuses things.

   We modified the text throughout the manuscript so that this labelling was not used anymore, according to the reviewer's comment.

1. “within analytical uncertainty” of what?

   We modified the sentence to add: “which is within the 1σ analytical uncertainty of the exposure age.”

1. There is some word streamlining here, replace “the MIS 6 cold interval” with simply “MIS 6”

   Change were made accordingly

734-739. this gets a little circular. Recommend applying what you think is a reasonable erosion rate correction given x, y and z evidence, then see where that age falls in the global climate history and discuss. Best not to back out what erosion rate is required to fit the age to a certain climate event. This weakens any argument you later make for any support whatsoever for evidence of glacial activity during MIS 7 in your field area. To be honest I think it is weak anyway, even too weak to mention. This is just one age from one bedrock surface after all.

We agree with this comment: and we have removed the last few sentences of the paragraph that presents the hypothetical age of the landform fitting a MIS 7 cooling if we were to apply a certain erosion rate.

double check that fig 7 is referred to prior to fig 8

Yes, we doubled checked and it is (section 5.1.3, paragraph 3, line 2).

1. Why not make simpler titles? “RC II exposure ages” for example

   Agree: the title was made simpler

1. remove extra space

   Change was made accordingly

1. “fluvioglacial polish” throws me off a little bit. If you use this term, you might need to clarify somewhere you interpretation of how these moraines formed. And, if you can’t tell if it is fluvial or glacial polish, then why not wind polished?

   Fluvioglacial relates to erosion processes involving glacier meltwater on the surface of, within, or below the glacier ice. “Fluvioglacial polish” thus relates to the polishing of a clast surface when this clast was at the bed or when being transported by the glacier. The polishing here isn’t fluvial in the proglacial or post-glacial sense of the term: in which case it would be described as “glaciofluvial”.

   Because this can indeed be confusing, we removed that term and instead write, more simply: “presenting polished surfaces”

   We have observed surface polishing on boulder samples that produce exposure ages that agree with the surface cobble ages and indicate a late-MIS 6 glaciation. If the boulder surfaces had experienced wind polishing: and thus surface erosion, this would have caused younger apparent ages. Wind erosion on the western sides of these boulders generates ventifacts and distinct grooves, and not smooth, plane polishing like we see on the protruding surfaces sampled. See more detailed previous comment on this.

1. This is inheritance. Boulder recycling is a way to get inheritance. Cobbles can get recycled, too, in fact maybe more likely where glaciers are flowing over previously glaciated valleys stuffed with outwash. What is the lithology of the cobbles? You write quartz bearing. You describe lithology of moraine boulders, but not cobbles I don’t think. I must admit it is a little strange that inheritance of boulders is considered, but not in cobbles. I’d think it more likely in cobbles than in boulders – ie, more likely to recycle cobbles in a re-glaciated area than moraine boulders. Anyway, both is possible.

   For RC II and RC 0 ages, we do not see a spread in cobble ages similar to that can be seen in the RC II boulders, instead the ages are relatively clustered considering their ages. Our experiment in itself is an answer to this comment. This has also been found in other studies by Hein et al. in Patagonia. Moreover, we do in fact consider inheritance as a likely scenario when there is a spread in our exposure ages: *i.e*. the RC II moraine boulders and the RC I cobbles. Our discussion concerning the interpretation of the RC I cobble ages features a hypothesis on cobble recycling and inheritance, which we consider a potential factor (see section 5.1.2 paragraph 2). The above comment is thus not fully justified, we feel.

1. this takes the age too much at face value. If the boulder was recycled, it would not have landed face up exactly as it had before, any number of minor rotation adjustments could perhaps lead to this age from a MIS 8 boulder, for example.

   This is indeed a possibility, but combined with the MIS 6 outwash cobbles ages located outboard of the RC II moraine, there is ample evidence suggesting this expansion event dates to MIS 6, along with the moraine geomorphology which displays significantly more erosion on the MIS 8 moraine than on the MIS 6 moraine.

Figure 6A legend is hard to follow, suggest adding RC labels to it like in the earlier map figure. Also, it is really informative seeing the individual ages on a map figure. Suggest finding a way to do this for the RC0 site.

We have added the RC labels to the figure.

1. I agree with this, and therefore am a little uncertain as to why you are having a discussion on topics that lie beyond the ability of your data to inform.

See reply to main reviewer argument on this.

1. I think “accurate” should be “precise” in this use.

The change was made accordingly

Fig 7 doesn’t add much, Fig 6B tends to cover it.

We beg to differ here. We have in fact received contrary feedback on Figure 7. The maps provide a visual representation and inform the scale of the ice extent and geographical cover for each reconstructed scenarios within the study site. They also inform the former dynamics of meltwater drainage and proglacial lake formations and how these changed between the various advances. This is knowledge we reconstruct from both our detailed geomorphological mapping (published separately) and the geochronology presented here. This valuable data does not feature in figure 6B. Moreover, a similar summary figure was produced in our companion paper in QSR, looking at the LGM advances. Figure 7 makes a good link to this companion paper, therefore. These figures are often considered the most useful to compare with ice-sheet model outputs, moreover.

1. this section is a stretch. I believe that these bigger arm wavy components of your manuscript dilute the stronger parts. It is a long paper as it is, why go into this territory? Evidence for glacial activity during MIS 7 is extremely thin.

   We have removed that entire MIS 7 section from the discussion.

1. remove word “penultimate” MIS6 glaciation suffices

Change was made accordingly

1. same, just write “from the RC II deposits suggest” Can an “interpretation” “suggest” something?

   Change made to the sentence accordingly. “ TCN exposure ages from the RC II deposit suggest that a…”

1. I’m not convinced that there is evidence for the timing of glacier expansion or duration of the maximum interval. The top of the outwash terrace is dated, which perhaps gets frozen into place once the outwash surface becomes abandoned. This happens during river incision, this probably happens during glacier recession. So perhaps there is no evidence, given what is dated, for glacier advance or “expansion” etc…

   Surfaces cobbles date the timing of outwash abandonment and stabilisation. In this semi-arid to arid (during full glacial conditions) setting that features moreover reversed bed slopes, outwash abandonment should occur as soon as glacier recession from the dated margin starts. Because we sample the outwash directly outboard of the outermost moraine: the abandonment of the outwash at these locations is related to ice retreat from the outermost advance of a moraine complex: moreover: and is considered closer to the true age of the earliest advance than eventual boulders from any inboard moraines, for instance. Given the large analytical uncertainties associated with surface exposure dating of such old deposits, the timing of ice retreat from a specific margin also encompasses, within such uncertainties, the timing of the glaciation and expansion of the ice sheet. Indeed, the Patagonian Ice Sheet being temperate and quite sensitive to climate fluctuations: it is unlikely that the ice front remained at the location of the outermost moraines for several to tens of thousands of years. With our LGM chronology from the same outlet glacier (Leger et al., 2021), we see that these processes locally occur on sub-millennial timescales.

   We completely agree that the nature of our dataset and uncertainties of TCN exposure dating does not allow to resolve the relative timing of expansion vs retreat. This is explicitely why we give a conservative 10 kyr time window (140-150 ka) for the RC II margin, for instance, which likely incorporates the timing of the PIS expansion to, and also the retreat from, that limit. This, moreover, is considered a “suggestion” from our ages. Note we use conditional and conservative language, as we know that these results, despite being some of the best dating results in Patagonia for these older glaciations, present significant uncertainties. We don’t think there is “no evidence”, as hinted upon here. We produce suggestive evidence that come with quantified uncertainties that we report in the paper.

1031, remove word “abstract”

We removed the reference altogether as the sentence “is amongst the first published datasets” covers the idea already: and because work by Peltier et al hasn’t been published yet: while we thought it might have during the production of this manuscript.

1. Text implies that there is evidence for PIS expansion events a few ka after minima in NH summer insolation intensity, etc. The fact is that knowing this would require an error bar on your glacial deposits that is much much smaller than your understanding. I would encourage you to consider what your chronology is based on (oldest single cobble age and its analytical uncertainty, see above comment, it is impossible that this error bar, and this age, is known this precisely). Statements like this should be reconsidered.

In section 5.2.1, paragraph 2, line 8: we mention to the readers that analytical uncertainties associated with the pre-LGM chronology does not enable to distinguish its correlation with minima or maxima in summer insolation intensity signals, but that this is the case for the MIS 2 chronology. We agree that this should be reminded here and that the statement needs to be re worded to make sure the uncertainty is better considered.

We modified this section of the text to:

"*They also appear to occur around the timing of minima in NH summer insolation intensity (60°N) and maxima in SH seasonality, while being out-of-phase with mid-latitude SH summer insolation intensity (Fig. 8c). However, one must note that this statement can only be advanced with confidence for the local MIS 2 expansions of the PIS. For the local MIS 8 and MIS 6 glaciations, this observation is based on current knowledge of 10Be production rates and the assumptions made in this paper, and does not take into account the full exposure-age range covered by dating analytical uncertainties.*"

Section 5.3. I'm a bit neutral about whether this section adds to the paper or not. It has very little to do with the dataset that was generated. It is a review of ideas that are not strongly informed by the results of this study, at least as written.

We think this section is relevant.

1. The final sentence of 5.3 makes an argument that these ideas need testing. Echoing some statements made in the abstract. Don't get me wrong, I am a glacial geologist who does this stuff for a living, but I'm not sure that, given our chronological toolkit at present, that we have the ability to date terrestrial glacial events with enough precision to resolve these hypotheses at present. It is a challenge.

   We strongly agree with that comment and do also think this is a major challenge given our present-day tools. However, that does not imply that trying to answer these questions shouldn't be an avenue of future research. We did decide to modify the sentence to take into account the challenging nature of such research, however.

   "Testing the above hypotheses, and determining which of seasonality versus seasonal duration played a primary role in driving SH climate and glacial variations during the middle-to-late Pleistocene, remains a major challenge and represents a key avenue for future research."

1. Can another phrase be used in place of "inceptive evidence" this is 2nd use. Not sure what that means. Anyway, you know how I feel about the evidence for MIS 7 glacial activity. What does "another MIS 6 advance" mean? Not sure I follow this part.

In agrement with this comment: we removed that sentence from the conclusion. Indeed: the MIS 7 glaciation is discussed as an eventuality and doesn't present good enough data to be part of the main paper conclusions.

1. If write "the Ice Sheet" should be lower case

Change was made accordingly.

Reviewer 2 comments:

L70 - Could you specify the timing of the LGC for clarity?

In agreement with this comment, this information was added to the first reference to the LGC in the introduction section.

L230 - This might not be relevant, but is it worth mentioning why the work took place at 3 different labs? Interlab calibration?

The reason for this is purely logistical. The 6 samples that were treated at CEREGE required more purification, and a time allocation that collaborators at SUERC did not have.

As I, the principal investigator, was based at CEREGE at the time, these remaining samples were sent to CEREGE so that I could work more closely on them and purify them further. For practical and financial reasons, these

remaining samples were also measured at the AMS on site.

Please let us know by replying to this comment if you think this information should be added to the paper.

L627 - 'outwash terrace sampled features preserved braided...' please consider rewording this part for clarity

In agreement with this comment: this sentence was reworded to: "At the sampling sites, the outwash terrace surface displays preserved braided meltwater channels that suggest minimal outwash surface deflation post deposition"

L880 - You may consider referencing specific parts of Fig. 8 within this text for clarity (e.g., (Fig. 8D; Darvill et al., 2016), (Fig. 8C; Denton et al., 2021)

Changes were made accordingly

L894 - Is it possible that some of the temperature proxies reflect changes in or a feedback/reaction to ice volume? Shouldn't MIS 5D be the coldest part of the last glacial cycle with high seasonality and very long, colder winters?

The magnitude of cooling in Antarctic ice cores during MIS 5d is in fact quite impressive, around 10 degrees C in 20 ka according to the 5-core average temperature curve (fig. 8A). Indeed this is possibly related to this special orbital configuration causing high seasonality and very long, colder winters in the southern hemisphere.

However this extreme orbital configuration occurred right after a strong glacial termination, just after the Earth's climate system reached a treshold and warmed abruptly, most likely due to an internal mechanisms such as an extreme Heinrich events, shutdown of AMOC, modification of the thermohaline circulation causing southern ocean current southward migration which could also have triggered a CO_2 outgasing positive feedback etc. We thus need to look at both the orbital configurations but also the internal mechanisms reponsible for these abrut shifts in climate. This intense cooling started from a quite warm interglacial and thus didn't result in a maximum cooling of similar intensity than during MIS6,4,2 peak cooling.

In general, how strongly can we pin the range of ages (or just the maximum age) from outwash plains to specific insolation signals, especially when we should expect a delay of several millennia between forcing and response?

Given the uncertainties associated with TCN exposure dating of such old deposits, we agree that a few kyr uncertainty is unrealistic. The main findings of the paper lie more in the establishment of the timing of local glaciations that we can attribute to a certain MIS interval, and with time-window precisions that lie more in the 2 sigma standard deviation ranges (10-20 ka), which are large enough to take into account production rate uncertainties. However, while we think our oldest cobbles are the closest “estimates” of the timing of these glaciations, they may indeed still underestimate the true deposition age, and only collecting many more samples would help determine whether this intepretation is correct. With this in mind, we have decided to modify the text in the discussion and the conclusion, to make sure that we talk about the timing of these glaciations using more conservative time ranges, rather than the exposure age figures from the oldest cobble only. When talking about the timing of these glaciations in comparison with southern hemisphere insolation parameters and other palaeoclimate proxy records, we have added to the text that this entire discussion is based our own interpretation of the available chronological evidence, which while yielding high confidence for MIS 2 chronologies, yields rather intermediate and low levels of confidence for our MIS 6 and MIS 8 record, respectively. We have made sure to further stress that our discussion around the role of local seasonality and seasonal duration implies the assumption that such extensive PIS glaciations likely occurred during periods of maximum hemisphere-wide cooling, and thus when antarctic atmospheric temperatures reached their lowest values as well. We thus use the precision of the Antarctic ice core chronologies which display minima in local atmospheric temperatures that are included in our much larger dating uncertainty ranges, to discuss the palaeoclimate and link to insolation hypotheses. Although these assumptions yield uncertainties, we still believe that this discussion is important and contributes some new thoughts on the debate around the drivers of southern hemisphere and global glacial/interglacial cycles.