Climate of the Past Discussions

Clim. Past Discuss., referee comment RC1
https://doi.org/10.5194/cp-2022-32-RC1, 2022

Comment on cp-2022-32

Anonymous Referee #1

Referee comment on "A cosmogenic nuclide-derived chronology of pre-Last Glacial Cycle glaciations during MIS 8 and MIS 6 in northern Patagonia" by Tancrède P. M. Leger et al., Clim. Past Discuss., https://doi.org/10.5194/cp-2022-32-RC1, 2022

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.

- 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.

- 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.

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?

- Bedrock samples? Or sample?

- suggest not using the simple word “stage” and use Marine Isotope Stage in all cases.

- “enable to explore” reads awkward

- comma not semi colon

- 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?

- replace ‘further’ with ‘farther’

- 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.

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

- 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?

- 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)?

- 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.

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?

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

And why no age reported on panel F?

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

- 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.

- "kettle kame" implies glaciogenic, no need to be wordy, remove “glaciogenic”

- “sparse vegetation” see earlier comment, it is sparse today, but…

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

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.

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

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…

- 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.

- 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."

- “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…

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.

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?

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.

- 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…

- 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.

- "within analytical uncertainty" of what?

- There is some word streamlining here, replace "the MIS 6 cold interval" with simply "MIS 6"

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.

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

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

- remove extra space

- “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?

- 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.

- 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.

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.

- 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.

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

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

- 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.

- remove word “penultimate” MIS6 glaciation suffices

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

- 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…

1031, remove word “abstract”

- 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.

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.

- 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.

- 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.

- If write "the Ice Sheet" should be lower case