Climate of the Past Discussions

Clim. Past Discuss., author comment AC1
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Reply on RC1

Tancrède P. M. Leger et al.

Author 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-AC1, 2022

Comment

"*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*."

Authors' reply:

We understand the reviewers comment and agree that the evidence is thin for the former occurrence of an MIS 7 glacial event. This is why, in the orginigal manuscript, we have discussed the possibility of such event at our study site to be hypothetical and to require more future investigation. However, we agree that writing an entire discussion paragraph on this event brings unjustified complexity to the manuscript. We have thus decided we will remove section 5.2.2 from the manuscript and remove that bit of interpretation from the conclusions. We however still think it is useful to very quickly 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.

Comment

"*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. "

Authors' 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 conclusiosn, 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 low confidence for our MIS 6 and MIS 8 records. We have made sure to also stress that our discussion around the role of local seasonality and seasonal duration implies the assumption that such extensive PIS glaciations must have occurred during periods of maximum hemisphere-wide cooling, and thus also when antarctic atmospheric temperatures reached their lowest values. 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 TCN dating uncertainty ranges, to discuss the palaeoclimate hypotheses. Although these assumptions yield uncertainties, we still believe that this discussion is important and contributes some new thoughts to the debate around the drivers of southern hemisphere and global glacial/interglacial cycles.

Comment

"*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*."

Authors' 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 hypotheses 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.

Important: Line by line comments will be addressed and replied to once a second reviewer

report has been received, in order to make informed decisions based on 2 distinct feedbacks, prior to re-submitting a new revised version of the manuscript.

Kind regards,

Tancrède Leger (on behalf of all authors).