Clim. Past Discuss.,
https://doi.org/10.5194/cp-2020-111-AC1, 2020

Interactive comment on "Cosmogenic isotope measurements from recently deglaciated bedrock as a new tool to decipher changes in Greenland Ice Sheet size" *by* Nicolás E. Young et al.

Nicolás E. Young et al.

nicolasy@ldeo.columbia.edu

Received and published: 17 December 2020

Thanks for taking the time to read this manuscript and provide feedback. Here, we provide the main reviewer comments in quotation marks and our response follows.

"While reading the manuscript I missed having some of the most relevant tables included into the main text. In general, there are a lot of figures in the text, so I suggest to either move some of those figures to the supplementary material or possible merge some of the figures together, to make room for tables in the main manuscript. You have several figures with pictures of samples, I suggest to move/merge some of these or perhaps make more figures like figure 12."

We thought a lot about the balance of figures, tables, and the overall level of detail in the manuscript's text during our initial submission. We understand the reviewer's comment, but prefer to keep the figures and tables as is in the main manuscript text and supplement for a number of reasons. As it stands, this is a lengthy manuscript due to the different types of data that we present (e.g. number of isotopes, ice-margin chronology, erosion constraints, ice-sheet modeling), so we are hesitant to move the supplemental tables, and there are 9 of them, into the main text. As the reviewer points out, one of the key contributions in this manuscript is our triple isotope work from recently deglaciated bedrock surfaces. In this regard, we think it is important to showcase the types of bedrock surfaces and their geologic context we are targeting for this study.

As a consequence, we prefer to keep these sample-site figures in the main text. We also chose COP for this submission because it is a long-format journal (and open-access) and we wanted to use this format to highlight the unique field-based Quaternary expertise of our team and how these field observations are critical for this study. We think this is particularly true for this manuscript as it relies on subtle differences in morphostratigraphy, perhaps more so than typical exposure dating-based studies that we are accustomed to. Thus, we think keeping the sample-based and morphostratigraphic setting-based figures in the main text is critical to the interpretation of our results and overall manuscript presentation.

As the cosmogenic nuclide method is applied more widely, it has become commonplace to include the relevant tables in supplemental material, which are mainly used to re-calculate exposure ages. Indeed, we were inspired by current PhD student and co-author Alexandra Balter-Kennedy's recent publication in COP's sister journal Cryosphere where that author team merged a robust cosmogenic nuclide-based dataset, numerous field/sample photos, and a thorough description and discussion, into a well-balanced manuscript, while at the same time leaving many of the geochemical details in the supplemental tables that can be accessed as needed.

"In section 5.3 you focus on the inheritance in the 10Be samples, and conclude that the easiest explanation for this is exposure during MIS 5e. While you do comment and elaborate on the possibility of MIS 3 exposure, I miss more firm evidence for excluding this possibility. I acknowledge that the in-situ 14C ages do not seem to be affected by inheritance, which therefore limits the possibilities of MIS 3 exposure, but could it be so, that the sample areas experienced exposure during MIS 3, possibly in the earlier part, were then buried for >20 ka which together with a certain amount of erosion could make the samples reach undetectable limits more quickly (as you state in the text: previously accumulated in situ 14C to decay to undetectable levels after âĹij30 ka of simple burial of a surface by ice; with the aid of subglacial erosion, in situ 14C can reach undetectable levels more quickly). Could the authors elaborate on why this is not the case? Would it be possible to include some simple model runs, to further exclude MIS 3 exposure?"

At the core of our discussion we state "we cannot rule out small amounts of inherited 10Be. . ...is a result of exposure during MIS3" (lines 565-566).

We are aware that exposure during MIS3 is technically possible, as is exposure during the last Interglacial, and we do not "exclude" MIS3 exposure as a possibility. At this point, the discussion is simply which is more likely, exposure during MIS3 or MIS5e as both are mathematically possible. In this section we point out that the consistency in all of our cosmogenic isotope measurements, coupled with the somewhat widely accepted hypothesis that the GrIS was likely smaller than today during the last Interglacial and that the region was likely warmer during MIS 5e than MIS 3, points to MIS 5e exposure as being the more likely culprit for the inherited 10Be in our samples. In contrast, we are a bit hesitant to suggest that the consistency in our cosmogenic isotope measurements coupled with scattered and somewhat ambiguous evidence of a reduced GrIS during MIS 3 (i.e. based on very low carbon 14C ages that could suffer from contamination) is just as likely responsible for the inherited 10Be. We certainly can do a simple model where we accumulate 14C early in MIS 3 and then just let that

inventory of 14C decay away, but the concern is that we would give readers the impression that we think the MIS3-exposure scenario is just as likely as the MIS5e-exposure scenario. All things being equal and in the absence of any complimentary evidence, then surface exposure during MIS3 or MIS5e seems equally likely. However, after considering sedimentological evidence from offshore southern Greenland that points to a reduced GrIS during MIS 5e (Colville et al., 2011, Science), the idea that closing the global MIS 5e eustatic sea-level budget likely requires at least some contribution from Greenland (i.e. a reduced MIS 5e GrIS; Dutton et al., 2015, Science), and that the region was likely warmer during MIS 5e vs. MIS 3 (NGRIP, 2004, Nature; NEEM, 2013, Nature) we simply prefer what appears to be the more straightforward explanation that our inherited 10Be is a product of MIS 5e exposure. This is how we currently have it presented in the text while readily acknowledging that MIS 3 exposure cannot be ruled out.

To some degree this issue highlights the inherent weakness in multiple isotope systems where there are technically infinite solutions. For example, brief exposure during interstadial MIS 5a, instead of 5e or 3, is also mathematically possible. Nonetheless, we can include this simple model following the reviewer's suggestion, but we prefer to make it clear in the text that while MIS 3 exposure is possible, we think MIS 5e exposure is a simpler explanation after considering additional lines of geologic evidence.

"A personal comment on the title of the manuscript: I struggle with calling the combination of the three isotopes a "new tool" to track GrIS changes – it is a rather new approach to combine these three isotopes, but all of them are commonly used to track ice sheet changes. This is optional, but consider changing the title to something less promising like "Combining 10Be-26Al-14C cosmogenic isotope measurements from recently deglaciated bedrock reveal changes in Greenland Ice Sheet size""

We disagree with this assessment. Widespread application of 10Be and 26Al is common, application of 14C is not. Combining all three isotopes in the same sample is rare, in fact, the only other example that comes to mind is Miller et al (2006; QSR). What is unique here is the widespread combination of all three isotopes within surfaces that have only become ice-free in the last few decades to a century. We are unaware of any set of measurements that focuses on recently deglaciated bedrock in this manner. And, a key aspect of the work presented here is that detailed knowledge of the initial (Holocene) surface dosing history makes this triple isotope tool much more useful. We prefer to leave the title unchanged.

"Text Lines 17-18: What about the size of the GrIS during the Neoglacial? I believe it was larger than its current configuration in some places in Greenland? Possibly define late Holocene differently or make a comment regarding the Neoglacial/southwestern Greenland."

We can update the wording. However, we are providing the broad strokes about the GrIS here in the abstract. In general, and what this manuscript focuses on, is that the current configuration and modern/Little Ice Age limit are quite often one in the same or extremely similar. This was really meant to highlight that in most places the GrIS margin was inland during the middle Holocene.

"Lines 188-189: Could you elaborate a bit on the chosen scaling scheme? Why choose that, when, as you mention, changes in the geomagnetic field over time are minimal at high latitudes? Could you make a small comment on how much ages deviate using the other scaling schemes?"

Even though changes in the geomagnetic field is extremely minimal over time at this high latitude, we should, as good practice, probably use a scaling scheme that at least attempts to account for these changes (e.g. Lm). Using St scaling, which does not account for geomag changes results in a nearly identical age (often within a year) because the sites are at such high latitude and all the production rate calibration datasets are located at high latitude.

"Lines 289-291: Consider moving the lines “Silt. . .. diverted elsewhere” to the methods section"

We think the introduction of what a proglacial-threshold lake actually is should remain where we have it instead of in the methods sections. Any lake in a glacial environment can be a proglacial-threshold lake, but you do not know if it is or not until you core the lake and see the sediment stratigraphy. For example, if we cored these lakes and cored nothing but organic sediments, then we would be hesitant to call it a threshold lake. In the methods section, we prefer to leave the wording as is where we simply state that we cored two lakes. It is not until results that we really know these are threshold lakes, and then we explain how alternating silt and minerogenic sequences are achieved.

"Lines 305-325: As I read it here you have a maximum limiting age outboard the moraines of 10.23 ka, date the moraines to 10.24 ka and have minimum limiting ages inside the moraines of >10.25 ka – I know the ages overlap within uncertainty, but could the authors comment on this age distribution? Does it show a very rapid deglaciation and how does it fit with moraine formation?"

All of these ages overlap meaning that deglaciation and moraine deposition all occurred within the resolution of our chronometer.

"Lines 337-339: As this might be true, I feel it is a rather big conclusion made from two samples/ages – could the authors elaborate a bit, possibly include other data to underly the statement?"

We do not really think this is a big conclusion. . ..I suppose we can cite a paper or two that list basal radiocarbon ages from southwestern Greenland that are not too different (i.e. older; Bennike and Bjork, 2002) than what is known about the regional deglaciation chronology. We can perhaps word this so we don’t give the impression that we endorse dating bulk sediments (we certainly do not), but broad strokes, a bulk basal age is unlikely to give you a seriously erroneous age in southwestern Greenland. For example, in Young et al., 2015 (QSR) two sets of paired macrofossil-bulk radiocarbon ages from the same horizon yielded statistically identical ages.

"Lines 354-355: How “well” do you believe this age to constrain the timing of local deglaciation? The age seem relatively young compared to the KNS site, but fits relatively well with previous findings from this area. You discuss this in greater detail later, but could you use a sentence here to give the readers a sense of how much value you put into this age?"

It is a little unclear what the reviewer is asking. All this section does is list the best, and sometimes only (as in this case here) available deglaciation constrain beyond the historical maximum position. We state here that this constraint is only from a single 10Be age. We certainly wish we had more 10Be ages, but this a bit of “it is what it is” situation. In addition, this single age constraint isn’t really discussed later in the text as it is not really important to the overall deglaciation chronology. On line 635-636 we again mention that this constraint is from a single age, but beyond that there is not really anything we can do with this. We thought that by highlighting twice that this deglaciation age is based on a single 10Be age it would be implied that it is not the most robust age constraint in the region.

"Lines 435-436: This is interesting, do you have any idea why that is so? Geomorphology, samples, erosional features? It seems the three pairs of ages where the 10Be age Âż 14C age are from the same sampling site - what is special about it?"

We can mention in a revised version that there we didn’t notice anything different about any of the bedrock sites; they look the same. In fact, considering our team’s extensive experience sampling in southwestern Greenland, we were surprised to find any inheritance whatsoever.

"Lines 444-446: Could the authors elaborate on these combinations (less Holocene exposure and/or more subglacial erosion)? What do you consider more likely?"

This comment is a bit confusing. Lines 444-446 is the last sentence of section 5.1 and is meant to act as a segue to the next section. All of section 5.2 elaborates on the very question the reviewer asks here.

"Line 501: Suggesting to delete “The inferred. . . Jakobshavn Isbræ” and instead start the sentence “However, there are key differences” – as it is now you repeat yourself."

Ok

"Lines 508-512: I read here that you favour a scenario in which the GrIS deposited the moraines at c. 10 ka, and then stayed within very close proximity over the next 2-3 ka? How does this compare to your conclusions in section 4.2 (lines 330-332 – here you state that the ice retreated within the historical limit shortly after deposition at c. 10 ka? Could you elaborate a bit more on the spatial extent of this retreat in section 4.2?"

The reviewer is absolutely correct - we favor a scenario where the GrIS deposits moraines and crosses behind the historical maximum at 10 ka and then likely stays within close proximity for another 2-3 ka. I think we are all on the same page at this point. However, this manuscript is structured so that first we use 10Be and/or traditional 14C from beyond the historical maximum to simply constrain the timing of landscape deglaciation. This is what we do in section 4.2, and this section’s only purpose is to develop classic deglaciation constraints and set the stage for the rest of the manuscript. Another way to put it is that section 4’s only conclusions are stating the local to regional deglaciation constraints, nothing more. The next part of this manuscript is then addressing how do we gain any insight into what happens after initial deglaciation. To do this we have to introduce all these new tools, including the triple isotope measurements from recently exposed bedrock. This is what we have done in section 5. Therefore, we cannot take what we have learned about the ice margin in section 5 and then insert it into section 4. We prefer to leave the manuscript’s structure as is.

"Line 604: Could you briefly include a definition of “Baffin Bay” here? It is a rather large area and I don’t believe widespread moraine deposition at this time interval is known from northern Baffin Bay/Northwest Greenland? As I read it you mention southwest and west Greenland as well as Baffin Island."

I suppose we can add “southern” in front of Baffin Bay. The broader point here seems to be we have worked across a significant part of the Baffin Bay region (e.g. west and southwest Greenland, Baffin Island), so use of "Baffin Bay" in the general sense doesn't seem unwarranted.

"Lines 620-624: I suggest moving these lines “Lastly, we note. . .. advance of the GrIS”, to section 6.2, as you here discuss the retreat of the GrIS behind the historical maximum/modern margin."

In a previous internal draft of this manuscript, we actually had this statement in the next section as the reviewer suggests. However, after further consideration and a round of internal comments, the consensus among authors was to mention any potential 8.2 ka event moraines along with the rest of the broader southwest Greenland moraine chronology discussed in this section. Having these two sentences in the next section proved to be distracting as this section is primarily concerned with the mid-Holocene minimum extent of the GrIS. In its current position, we think these few lines serve as a nice segue to the next section where we fully discuss retreat behind the modern margin.

"Lines 636-641: Including data from Saqqap Sermia, you argue for a temporal difference of more than 5 kyr between deglaciation outboard the historical moraines in the KNS region – is the data from Saqqap Sermia the only to represent this relatively late deglaciation in the entire region, and if so, how much do you rely on this?"

We looked at the Saqqap dataset in detail (Levy et al) and consider it exceptionally solid; we have no reason not to trust the Saqqap dataset. The broader message here is that the 5 kyr spread in deglaciation ages in the KNS region that includes the Saqqap dataset is similar to the spread in deglaciation ages when you consider all of southwestern Greenland (our Fig. 16a).

"Lines 649-652: This is interesting, could you perhaps comment on where you would expect a greater or smaller re-advance of the ice margin, and what that would mean for the interpretation of your data? Could it be so, that places with younger deglaciation

ages have experienced a smaller re-advance and areas with older deglaciation ages experienced a larger re-advance – so you possibly have a contemporary deglaciation across the region as oppose to the 5 kyr difference? Or can you completely reject this scenario?"

Most of this is addressed in lines 638-648 directly prior to what the reviewer is pointing out, and the rest of this section after line 652 is highlighting that we probably shouldn't place too much emphasis on the deglaciation age beyond the historical limit. For example, lines 653-667 highlight how these site-to-site differences in deglaciation ages might occur yet at the same time not really signify drastic differences in ice-margin behavior. Site to site differences are almost certainly dictated by local topographic conditions or slight variations in ice margin behavior but, overall, deglaciation is somewhat/largely contemporaneous.

"Lines 692-694: From what I read here you base the 5-2 ka BP “window” mostly on data from other studies – could you make a small comment on your own findings in according to this age constraint – based solely on your findings would the “window” not be a couple of thousand years longer, with initial retreat c. 7 ka BP? I assume some of the explanation lies in the discussion of different ice-margin environments, that you give in lines 722-734?"

Agreed, we can add a sentence that addresses this. It would most likely fit around current line 637. I think part of the confusion here is what one considers the KNS region. In the strictest sense the reviewer is correct in that our new dataset identifies a window between $\sim$7 ka and 1 ka, but that also ignores the Saqqap region that we consider to be in the broader KNS region, and this manuscript has a much wider scope than just the KNS region (e.g. Fig 16 and our entire modeling effort). We can add a sentence that identifies a “window” solely based on new KNS data presented here.

"Lines 748-764: It seems that model runs simulate an ice sheet minimum that to some degree fits with your data from the KNS region (as stated in the comment above) –

could you briefly outline why/why not the models and your data fit/does not fit? Why you believe in the 5-2 minimum, and not an earlier retreat behind the present day margin?"

We do not think that any of the model runs do a particularly good job in the KNS region, and much of the modelling section (section 6.3) is devoted to pointing out the difficulties with incorporating calving in the KNS region as well as the influence of the KNS region's unique topography. We next discuss how these same issues don't really exist in the Kangerlussauq region north of KNS and how a few of the model runs appear to fit geologic constraints quite well. The modelling effort applies to the entire southwestern Greenland domain, which is why we compiled geologic constraints (many developed by our group) along southwestern Greenland beyond the KNS region so we could conduct a better assessment of the model results. When considering the geological constraints across southwestern Greenland, it appears to us that there is a pretty clear window of when the GrIS achieved its minimum (Fig. 16a; $\sim$5-2 ka). Our group and Nicolaj Larsen's group has spent nearly a decade developing these geologic constraints and collectively, the GrIS minimum appears to be robustly constrained as spelled out in Section 6.2 (and Fig. 16a).

"I find that much of the text in the figures (place names, ages, lat/long) is difficult to read and could benefit from a larger font size."

We will look into making some of the text larger.

"Table S3: Just a comment on the high accuracy of the sample thicknesses. As this might be the accuracy of the caliper (or whatever instrument you have used), I find it difficult to work with high accuracy numbers like this, on what I assume are rather uneven samples. Are sample thicknesses a mean of several measurements?"

We will add a footnote mentioning that we make several thickness measurements on samples and the accuracy given in the table reflects those measurements.

"Technical Corrections."

Thank you for finding these. We will make the necessary corrections.

Interactive comment on Clim. Past Discuss., https://doi.org/10.5194/cp-2020-111, 2020.