

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.

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Received and published: 3 November 2020

The paper by Young et al. provides new cosmogenic exposure ages (26Al, 10Be and 14C) from recently deglaciated bedrock in the KNS region, southwest Greenland. Based on their data, the authors find an early Holocene ice retreat behind its modern margin, where after the ice stabilized for several thousand years. The minimum extent of the GrIS likely occurred between c. 5 and 2 ka. Including previous studies and modelling the authors look into the inland retreat of the southwest GrIS. The study is interesting, especially the three isotope combination, which is intriguing and hopefully can help to further implement the use of especially in-situ 14C for chronological con-

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straints in Greenland. The study is well explained and adds to our knowledge on the glacial history in the KNS region and combined with previous studies conclude on the broader southwest Greenland glacial history. The implementation of modelling of the GrIS margin highlight not only differences within southwest Greenland and the KNS region but also emphasize data model misfits and the overall importance of implementing ocean-forcing into ice sheet models.

I have a few main comments/suggestions listed here below. The rest of my comments are divided into specific and technical comments all regarding both the text, figures, and tables. I hope the authors will address these prior to publication. Thank you for an interesting read and I look forward to see the final version in print!

MAIN COMMENTS

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.

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 ~30 ka of simple burial of a surface by ice; with the aid of subglacial erosion, in situ 14C can reach undetectable

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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?

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”.

SPECIFIC COMMENTS

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.

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?

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

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?

Line 324: Consider ending the sentence at “ka BP” – and then move the rest of the sentence “all available supporting” to line 330-332 where you comment on the same.

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Or you could simply delete it.

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?

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?

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 » 14C age are from the same sampling site - what is special about it?

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

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.

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?

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.

Lines 620-624: I suggest moving these lines “Lastly, we note. . . . advance of the GrIS”,

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to section 6.2, as you here discuss the retreat of the GrIS behind the historical maximum/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?

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?

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?

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?

Figures and Tables

I find that much of the text in the figures (place names, ages, lat/long) is difficult to read

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and could benefit from a larger font size.

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?

TECHNICAL CORRECTIONS

Text

Line 60: Consider to use only surface or surfaces in this sentence. “..when those surfaces are exposed. . .

Line 93: “reworked” is spelled “re-worked” everywhere else in the text. Consider changing this.

Line 105: Check spelling of Kangârsarsuup throughout the text – some places you spell it with “â” and some places with “a”. Further, judging from Figure 1, it seems that Kangârsarsuup Sermia is located about 20 km more than 35-40 km south of KNS?

Line 109: “Trim-lines” is spelled without “-“ a few places, consider changing for consistency.

Line 115: You have “cosmogenic-nuclide” (as here) and “cosmogenic nuclide” (eg. line 800) in the text. I assume you want one consistent way of spelling?

Line 120: Consider using the abbreviation “GOOF” for Goose Feather Lake in the text. You use it in the caption to Figure 6, but nowhere else. Same comment for Marshall Lake.

Line 128: Space missing between “61” and “10Be”.

Lines 162+165: I assume you mean “in situ 14C” here?

Line 323: I believe you mean production “rate” uncertainty?

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Line 369: Delete the “space” between “therein” and “.” at the end of the line.

Lines 538: Is there a “to” missing between “relative” and “10Be”?

Line 551: I think you miss a “with” before “recent modelled”.

Line 588: I think there is a “the” missing before “so-called”?

Figures and tables

Tables: Generally, consider consistency – you have Latitude (N)/Longitude (W) (Table S2), Latitude/Longitude (Table S3), Latitude (°N)/Longitude (°W) (Table S7).

Figure 1: Suggesting to either find another symbol or make the orange diamond larger – it is difficult to distinguishing from the orange circles.

Figure 2: I would remove “(ka BP +/- 1 S.D.)” to right after “in situ 14C ages” instead of having it at the end of the line.

Figure 6: You have (panel A) and panel (a). Consider writing it in the same way.

Figure 16: A “.” is missing after “(section 5.2)”.

Table S8: I think you have a double spacing at the beginning of the last line.

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