Replies to reviewer 2, Nicolás Young

This manuscript presents a series of in situ 14C measurements in bedrock surfaces with the goal of assessing their reliability in 1) constraining the rate/timing of landscape emergence, and 2) constraining the timing of local deglaciation. The manuscript is set up by pointing out that the author's field area already has a pretty well constrained emergence curve and an independent constraint on the timing of local deglaciation (some mixture of traditional 14C and 10Be from Stroeven et al., 2016). Thus, the author's hope that their in situ 14C measurements "match" the independent controls as a test of their usefulness in this type of glacial setting. The approach here is to present a new tool to the community and show how it might work/behave in an ideal setting. By and large the in situ 14C measurements here match the independent record….things appear to have "worked."

I really enjoyed reading this manuscript. I think the authors set up the study nicely, gave the appropriate amount of background information, kept it shirt and focused, and didn't try to turn this dataset/manuscript into something it's not. Well done. Dataset and measurements look solid. This manuscript will certainly be of interest to the cosmogenic nuclide community, in particular users of in situ 14C. I think this manuscript is perfect for Ghron.

I do not have many suggestions here; again, this manuscript is already a nice little package that presents a clean story. I have a few minor comments, and then one more major comment. The latter is (likely) more directed at Nat and is the product of a timely independent conversation I have been having with Nat over the last week. The authors should pay particular attention to this and make sure it gets fixed not only for this manuscript but in relevant databases. Even though this is a more major comment, it should not affect the overall conclusions of this manuscript here.

Thanks for your positive and helpful feedback on our manuscript.

Major comment:

Authors did a great job at explaining how in situ 14C ages were calculated and also provide a great figure (Fig. 2) that shows the 14C calibration datasets that go into calculating the spallation 14C production rate of "13.35+-1.13" atoms/g/yr. Glancing at panel A in Figure 2 I thought that the New Zealand and Greenland calibration numbers looked a little high relative to the other calibration datasets. These calibration 14C concentrations, in addition to all the others, were recalculated using the methods of Hippe and Lifton, 2014. For the most part, this results in a very small adjustment to the 14C concentrations. However, for the New Zealand measurements the change is significant larger. The recalculated concentrations are ~6-16% higher. This was brought to my attention last week and we have had an email chain going on trying to figure out what was happening. It appears that this issue has been identified – the primary culprit was that in their original publications only the Fraction Modern was reported *instead* of the 13C-corrected Fraction Modern, so when the un-corrected Fraction Modern values were used in recalculation using the Hippe and Lifton, 2014 methods, its resulting in significantly higher sample 14C concentrations.

I looked through the code that the authors use here for calculating in situ 14C ages, and I couldn't find the individual calibration numbers, I did see the "13.35+-1.13" atoms/g/yr" in the constants file. But I searched around on the calculator website that the authors reference and found this file, which I believe is what the authors are using to derive the global in situ 14C production rate: https://expage.github.io/data/prodrate/P-202306-input.txt I scrolled down to the 14C concentrations and, yes, what is here are the significantly higher New Zealand and Greenland concentrations. I also think these are the concentrations posted in the ICE-D database.

I raise this issue because those two datasets comprise ~15% of all the calibration measurements. With the pending concentration changes, the global production rate used here is going to get lower, and the uncertainty will likely improve. This will change the absolute in situ 14C ages in this manuscript a bit, but I believe with the error bars, all the in situ 14C ages will still overlap with the independent constraints (e.g., Fig. 3). The calibration concentrations need to be updated here, including in the relevant databases, and then the absolute ages need to be updated.

Thanks for identifying this important issue, which we have now remedied. Co-author Lifton corresponded with Drs. Young and Schimmelpfennig (first authors of the manuscripts detailing the Greenland and New Zealand calibration datasets, respectively) and identified the main issues responsible for the discrepancy – 1) the published Fm values were not corrected for stable C isotopic composition, and 2) the more significant correction for the mass-dependent graphitization blank was not included in the published Fm values, nor was the formula used to make such corrections published. We have now calculated the corrected concentrations for those two calibration datasets, and have recalculated the global production rate to 12.81 at g^{-1} yr^{-1} – a ca. 4% decrease from 13.35 at g^{-1} yr^{-1}. Updated concentrations for those two calibration sites are now also updated in the ICE-D Calibration database, and a corrigendum is in preparation for the Koester and Lifton (2023) citation from which the original recalculated data were taken.

Minor comments:

Lines 80-81 – what about marine terraces? I think that's a common term/setting for developing emergence curves.

There are no marine terraces here. Terrace formation is inhibited because the landscape is formed on tough, glacially scoured, crystalline bedrock (with only a patchy till cover), it's a highly fetch-limited environment, which limits wave energy necessary to terrace formation, and rapid isostatic rebound may have led to insufficient time for a terrace to form at a particular relative sea level.

Lines 142-144 – wondering how applicable this 200m marine limit located 100 km away is to your site? Are there any closer marine limit benchmarks? Doesn't really change anything, just curious.

We have located sites above the highest postglacial shoreline that are as close as possible to our Forsmark sites that were all located below sea level following deglaciation of the last ice sheet. Because the landscape is as low lying and low relief as it is, the nearest sites are about 100 km west. Once we found locations that were above the highest postglacial shoreline, we then had to locate outcrops in bedrock suitable for *in situ* ^{14}C analyses, which has some additional minor influence on distances from Forsmark.

Lines 225-235/Figure 2 – see main comment above

Noted!

Lines 398-403 – Agree with everything here. Not saying it's required, but if the authors wanted, it would be pretty simple to model a few endmember scenarios of MIS 3 exposure/MIS 2 cover +erosion histories to give the reader a sense on what it would take to arrive at the measured concentrations. For example, what would the evolution of 14C concentrations look like if you dosed the surface for 10 kyr during MIS, cover it during MIS 2 with no erosion, then re-expose at ~10 ka? Maybe just a few of these using *reasonable* exposure/burial/erosion rate constraints to give the reader how in situ14C concentrations may have evolved through the latter half of the glacial cycle?

Thanks for this good suggestion. We have added a new figure, "Figure 5", that shows the hypothetical development of ^{14}C concentration in the five samples above the highest shoreline in two different ice cover scenarios. We assume no glacial or interglacial erosion, continuous exposure during ice-free periods, and full shielding from cosmic rays during periods with ice cover. The simulations are done over the last 80 ka with ice cover during the periods 70-57 and 35-10.7 ka BP for the longer ice cover scenario and 66-60 and 28-10.7 ka BP for the short ice cover scenario. The simulations clearly show that if the MIS-2 ice advanced over the samples at 28 ka or earlier, the pre-MIS-2 ice history does not matter much for the present-day ^{14}C concentration.

Replying to my own review here, just want to be clear. The main issue is that the 13C **+ Blank** corrected Fraction Modern value needed to be used for the 14C calibration samples I mentioned. Sorry for the confusion.

Thank you again for clarifying this, we have adjusted the data and figures accordingly – see response above.