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Climate of the Past Discussions

# *Interactive comment on* "Younger Dryas ice-margin retreat in Greenland, new evidence from Southwest Greenland" *by* Svend Funder et al.

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This manuscript by Funder et al presents a series of 10Be ages from coastal locations in southwest Greenland that are then used to infer ice-sheet history during the Younger Dryas. This is very much a chronology paper, and many of their arguments are heavily dependent on how one interprets the new 10Be ages. If this entire SW Greenland coastal chronology were based on 10Be ages from erratic boulders perched on bedrock, the author's interpretation of the dataset would be much easier to digest; however, this chronology is almost exclusively from bedrock which presents some challenges. The authors state that these sampling regions were largely devoid of suitable erratic boulders. Thats fine, you can't sample what is not there. But, what to make of standalone 10Be ages from bedrock surfaces can be tricky and I am not sure the



authors here have really given this enough thought.

The primary issue here is that the odds of encountering isotopic inheritance (producing ages that are too old) are higher in bedrock than in erratics, and the authors even acknowledge this. Often times inheritance presents itself as obvious outliers, but there are certainly cases where inheritance is much more subtle and even exists as 'uniform' amounts of inheritance. Again, I think the authors somewhat acknowledge this because they go through great lengths in section 4 to convince the reader that their preferred site by site coastal deglaciation ages are NOT influenced by inheritance even though in some cases their preferred ages of coastal deglaciation are significantly older than the ages of adjacent regions.

Why does this matter? If you take their 10Be ages at face value, then it allows the possibility of the ice margin retreating onto land prior to the start of the Younger Dryas, which in turn the authors use to argue that a major Younger Dryas related oscillation did not occur out on the shelf since everything on the shelf would have to be pre-YD in age. However, a lot of this argument is resting on their 13.3 ka and 12.3 ka sites, Fiskenæsset and Bukesefjord. These ages are noticeably older than the constraints on deglaciation from adjacent areas. The authors take these ages at face value, but I think it is more likely that these suspiciously old sites have slight amount amounts of isotopic inheritance. If the 'true' age of deglaciation is in fact younger and more similar to the remainder of the deglaciation constraints from SW Greenland, the the ice margin is still somewhere out on the shelf during the Younger Dryas. Therefore you cannot rule out a major early or mid-YD oscillation of the ice margin.

Considering all of the 10Be ages from Larsen 2014 and Winsor 2015, and all of the 14C ages from the region, the bulk of the deglaciation chronology around Nuuk (14C + 10Be) suggests deglaciation occurred 10.5 -10.7 ka, with a single older 14C age of around 11.4 ka. With that being the case, I think it is much harder to sell that the Fiskenæsset and Bukesefjord regions deglaciated several thousand years earlier. Note that the region between Fiskenæsset and Bukesefjord also deglaciated at the same

time as the Nuuk region. Considering all of this evidence, coupled with bedrock only sampling, leads me to think that these 10Be ages are influenced by a bit of inheritance.

To be sure, 10Be ages from bedrock can work, but it is typically only in select environments. The most obvious from the region that come to mind are Briner et al (2009; Nature Geoscience) from Baffin Island and Young et al (2013; Quat. Sci. Rev) from Disko Bugt. In both of these cases the 10Be chronologies rely pretty heavily on bedrock samples. Also in both cases the 10Be ages agree with the 14C constraints that exist. In particular, up in Disko Bugt the 14C and 10Be constraints are near identical. This consistency suggests that the bedrock-based 10Be ages are not influenced by inheritance. But what these sites have in common is that the bedrock is located is some of the most erosive environments on Earth where the chances of encountering inheritance should be minimal. On Baffin, samples are from bedrock knobs either right in or adjacent to the Fiord trough (Sam Ford Fiord), and in Disko, these sites are directly adjacent to arguably the fastest glacier on Earth. In this manuscript, the authors are presenting 10Be ages from bedrock in coastal locations *between* former fast-flowing outlets. I wouldn't exactly consider this a recipe for success if you want to avoid inheritance in bedrock surfaces. Even sampling in highly erosive environments does not ensure inheritance-free samples. For example, Hughes et al (2012; Geology) sampled bedrock and erratics in Sermilik Fjord (Helheim) and present pretty convincing evidence that the bedrock samples are influenced by a small (only a few ka) and relatively uniform amount of inheritance. Co-author Briner himself demonstrated the same thing in Norway (2016; Geophysical Research Letters) where on the surface his 10Be ages from erratics are fairly uniform and likely come from what should be an erosive environment. But after considering other constraints, they concluded that their samples are likely influenced by a uniform baseline amount of inheritance equating to only a few ka. As we develop more and more of these 10be-based chronologies, its becoming clear that that small amounts of uniform inheritance exist and bedrock-based ages need to be treated with extra caution.



I look at this dataset and the pre-existing chronology and highly suspect that many of the new 10Be ages in this manuscript have slight isotopic inheritance. And, I think the authors have significantly overreached in their interpretations on what the ice sheet may or may not have done during the YD based on their new 10Be ages. That is not to say these 10Be ages shouldn't be published; they are a valuable contribution that others will certainly build off. It is even possible that these ages are 'correct' but you would need to back the bedrock-only results with matching ages from erratics or any other independent constraint before you launch into sweeping interpretations about the ice margin during YD times.

A few other minor things caught my eye as I read this:

1) It says you used version 3 of the CRONUS calculator for 10Be ages, and a production rate of 3.96. Version 3 uses an updated treatment of muon production, so the production rate needs to be recast with the same treatment of muon-based production. So if you used the online calculator, you would have to input all the Baffin Bay calibration data to make your own calibration, and then calculate the ages (there is an option to do this). In this case, the calculator will do the re-casting for you and the production rate is 4.04+-0.07; 3.96 is the number for the old scheme. Your ages should not change, but to avoid confusion, you should list the right PR. Or, you actually used version 2 of the CRONUS code, in which case that 3.96 number still applies (see Young et al., 2020; QSR)

2) The authors spend a lot of time going around Greenland compiling constraints related to the YD ice margin position. Thus I was a bit surprised not to see a fairly robust constraint from near Sisimiut. Young et al (2020; QSR) present several 10Be ages from erratics directly at the coast and from moraine boulders also at the coast, that all date to $\sim$11.6 ka. Therefore the ice margin was out on the shelf during the entirety of the YD.

3) The authors also mention that they do not correct for uplift on 10Be ages because

those effects are offset to some degree by atmospheric pressure variations, and they cite Young et al (2020; QSR). While this is true, a more robust thing you can say here is that the 10Be production rate calibration dataset likely underwent a similar amount of uplift to your known sites and therefore no correction is needed, the correction is essentially 'built in" in this case.

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