m.white@us.nature.com
April 29, 2014 11:57 PM
To: Paul.Bierman@uvm.edu
Reply-To: m.white@us.nature.com
Decision on Nature manuscript 2014-02-01640A

29th April 2014

Dear Paul

Your manuscript entitled "Cosmogenic ^{10}Be records 10 million years of Greenland Ice Sheet history" has now been seen by the three original referees, whose comments are attached below (referee #1 had no further comments addressed to you). In the light of their advice we have decided that we cannot offer to publish your manuscript in Nature.

Specifically, we appreciate that referee #1 supports publication (largely reflecting his/her differing area of expertise) and that referee #3 is enthusiastic about your data and method; we accordingly have no doubt that an appropriately revised manuscript should be published in some forum. Overall, however, the comments from referees #2 and #3 indicate that your data support a plausible, rather than unique, solution for the history of the Greenland Ice Sheet. In order for us to publish the paper in Nature, we would normally need more compelling evidence.

I am sorry that we cannot be more positive on this occasion but hope that you will find our referees' comments helpful when preparing your paper for submission elsewhere.

Yours sincerely
Michael

Dr Michael White
Senior Editor
Nature
San Francisco

P.S. I should also let you know that I contacted Jesse Smith at Science to see whether or not their embargo policy should have prevented you from citing the Science paper. He indicated that the embargo policy does not relate to citations in papers submitted elsewhere, and that it would have been appropriate to cite the Science paper in your Nature submission (and indeed that he would have expected you to let him know about any Nature submissions, were the situation to be reversed). That said, the citation issue and possible overlap was not a main reason for our decision.

Referees' comments:

Referee #1 (No further remarks to the Author):

Referee #2 (Remarks to the Author):

I still do not agree that the overall outcome is substantially useful for the community. We already knew before that 10Be concentrations scale with glaciation; the question remains whether the pattern the authors observe is explainable with the model they suggest,
or if other parameters and/or their synopsis may reach other conclusions. I stress again that a sensitivity analysis would shed light on the driving parameters, and work against the very oversimplified model the authors present.
I would like to underline that also reviewer 3 has in his/her last major comment also indicated that the authors should not draw "grand conclusion" about the coverage of all of Greenland by ice", before elementary issues are not clarified, like sample provenance and clay composition explaining the trend rather than regolith cover. Similarly, I would argue that since complete resetting of the cosmogenic nuclide clock involves very deep erosion of bedrock (due to deeply penetrating muons that the authors have correctly incorporated into their model), ANY interpretation from thick warm-based ice sheets eroding SOME nuclides down to the "regolith hypothesis" may be possible for the 10Be excursions i.e. "full glaciation" model in Fig. 1a describing the present-day situation in Greenland. If today is totally unclear, how can be make assumptions about the past?

I am worried that people will, similar to this study, use this system as a mere "black box" in the future, with absolutely no information on how the systems really behaves, but simply turn too much attention to the "cool" result of
a relation of 10Be concentration with time, inferred to be driven by glaciation.

Referee #3 (Remarks to the Author):

The model by Bierman and Shakun is very simple. To first order, they show a nice pattern of decreasing 10Be concentration over the past 10 million years, consistent with global cooling and increasing glaciation of Greenland. The data set is novel and unique. The main issue raised by earlier review was whether the interpretations are robust and whether they yield new insight into the Greenland ice sheet history.

The authors have responded thoughtfully to the previous comments. However, it must be realized that this is still very much an observational paper that correlates various records, rather than a paper that firmly rejects one hypothesis or another. The problem is that the 10Be record is not unique. It could represent the average behavior of the ice sheet, or it could reflect some combination of local erosion and preservation. There may be 'hotspots' with high 10Be concentration that are eroded at one time or another, or protected under

slow-moving or cold-based ice at another. This sort of complexity seems impossible to unravel beyond the level of generalities.

For example, the authors make an ad hoc explanation for the variability in the middle to late Pleistocene by calling on specific events such as MIS 11. Can we say anything quantitative about MIS 11? Not really. It is not clear whether the data are testing any particular hypothesis about MIS 11, but they are consistent with earlier interpretations that Greenland was largely deglaciated at that time. The MIS 11 excursion is every bit as big as the excursion at the mid-Pleistocene revolution near 0.8 Ma. Clearly there is an interesting signal here in the 10Be record that is consistent with the offshore pollen records, but that seems to be the extent of what is said.

The lithofacies analysis adds to the paper, but there is very little that is said beyond the general correlation between 10Be and grain size. The assignment of lithofacies seems to be somewhat confusing in detail. The authors have the lithofacies of Larsen et al. But a later paper by St. John and Krissek has a substantially different lithofacies assignment. I am left with the impression that this is a data set that is incompletely explored. For example, how do the data vary with mass accumulation rate (MAR)? I see very little correspondence when I plot Bierman and Shakun's data on top of St. John's MAR, which is in itself interesting. One might expect lowest 10Be concentrations at the highest MAR, but the records are essentially uncorrelated. Does this say something about the origin of the sediment? At this point one can only speculate.

In summary, this is a fascinating paper that offers a truly unique dataset. The data are consistent with persistent glaciation of Greenland beginning very early, with notable excursions at known times of drastic climate change (e.g., 2.7 Ma, 0.8 Ma, MIS 11). The addition of grain size data and the (crude) statistical correlation help to bring the paper towards a mechanistic base, but ultimately I see the greatest strength of the paper in its novelty and in methodology, rather than in its ability to offer any particular new insights into the Greenland ice sheet. The record is consistent with long-lived glaciation of Greenland, but it is not clear whether the record leads to new or important insights.

As a minor comment, I still think the discussion of the preglacial regolith hypothesis is unsupported and detracts from the paper.

* NPG's author and referees' website (www.nature.com/authors) contains information about and links to policies and resources.

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