RESPONSE TO REVIEWS (*responses in red*)

Ref.: Ms. No. G36839
Cold-based, Laurentide ice covered New England's highest summits during the

Dear Dr. Bierman ,

I have now received three reviewers' comments on your GEOLOGY manuscript. All three were overall supportive of the work, and so am I, but one reviewer suggested 'reject, invite resubmission' while two asked for 'minor revision'. After reading all reviews and the manuscript, I am convinced that more than minor revision is needed. In fact, what reviewer 3 asks, seems very reasonable to me but could not be accommodated with 'minor revision'. I also agree with reviewer 1 that the data may not uniquely support the conclusions.

In addition, I would like to see abstract revised and a short conclusion section inserted in order to more clearly address a broad audience, and make the wider implications of the research more clear to such an audience of non-specialists.
We have revised the abstract and added an implications section with the goal of addressing a wider audience and making the research results for accessible to non–specialist readers.

I also would like to see a somewhat more informative Figure 1 (e.g., with altitudes shown).
We have revised figure 1 to make it more informative, including information about elevation and the location of other sites mentioned in the text.

Dr. Ellen Thomas
Yale University
Geology

Reviewers' comments:
Reviewer #1: The manuscript presents measurements of multiple cosmogenic nuclides from three summits in New England. Whether or not the Laurentide Ice Sheet covered the summits during the Last Glacial Maximum has been a topic of great interest for many decades. The authors conclude that the Laurentide Ice Sheet covered the summits during the Last Glacial Maximum, but by frozen-bedded ice, based on high abundances of long-half-life nuclides (10Be and 26Al), and low-abundances of the short-half-life nuclide 14C.

The strength of the manuscript is two fold. It lies in the fact that there are very few published datasets yet to use the powerful in-situ 14C tool, and this study nicely illustrates the advantage of being able to measure multiple nuclides, each with a different half-life, in the same samples. The second strength has to do with being a topic of great interest, at least at the regional scale.

Despite these strengths, there are a couple of fundamental issues with the manuscript in its present form, and there are number of more minor things, per usual, that could be done to help streamline this draft.
We agree and have done this streamlining, focusing on the cold-based, non erosive ice argument and minimizing our focus on the less well constrained post LGM history.

Foremost is that the data do not uniquely support the conclusions made by the authors.
We agree that our interpretation of post LGM history was non unique and have de-emphasized this part of the manuscript and added alternative hypotheses. In addition to considering ice carapaces, we also now consider till cover, post-glaciation erosion of boulders, boulder rotation, and other periglacial processes.

An alternative interpretation compatible with the data is that the summits were covered by local glaciers/ice caps during the Last Glacial Maximum. Cover by local ice for 29 kyr could also produce the decay needed to re-set the 14C clock.
Extant geologic data, specifically the presence of Canadian Shield erratics and the lack of moraines in the cirques argues against this interpretation, which we do not consider to be viable. We have added additional information in the paper about the evidence supporting glaciation of the summits by Laurentide ice, along with three references related to the possibility of permafrost on the summit of Mt Washington even today.

In fact, as it stands, the authors already invoke a period of shielding by local glaciers both before and after the Last Glacial Maximum in order to explain the 14C concentrations.
After consideration of our data and the comments of the three reviewers, we now consider shielding by thin ice carapaces as less likely. We never thought of this cover as being by local glaciers but did not clearly articulate our thoughts and thus confused the reviewer.

So it is not a stretch, and one may even argue more likely, that it was local ice that covered the summits throughout the Last Glacial Maximum and not Laurentide ice.
The extant geologic information (erratics on the summits) and simple ice profile models (requested by other reviewers) argue against summit cover by local ice. We now outline these lines of evidence more clearly in the manuscript.

Furthermore, the lack of burial recorded in the Al/Be system also lends support to the fact that the peaks may have not been buried by ice sheet ice. Although the Al/Be system cannot be used to definitively support lack of burial during the Last Glacial Maximum (or any duration of cumulative burial less than ~100-200 ky, depending on Be and particularly Al error), it does suggest that it is unlikely that the peaks were covered during Quaternary average glacial maxima.
Modeling (now included in the supplement) does not support the reviewer's qualitative assertion. Short (10-20 ky periods of burial) followed by longer (100 ky) periods of exposure result in 26Al/10Be ratios that cannot, with the precision of the measurements reported here, be distinguished from constant exposure. Our data are consistent with coverage of the peaks during major glacial maxima.

In any case, unless the authors treat this scenario explicitly, the paper is not acceptable for publication in its current form. It is possible that after some re-working, they can make a convincing case for one interpretation over the other, in which case it may be suitable for Geology.
This comment by reviewer #1 (and comments by the other two reviewers) were very useful as they spurred us to do additional modeling and think of ways in which we could: test the qualitative findings of others, reorganize the manuscript so our arguments would be more clearly presented, and create simple models to test whether limited burial between long periods of exposure could be reliably detected.

In thinking about the reviewers' comments prior to revision, we continue to believe (as do all three reviewers) that the 10Be/26Al data strongly support our conclusion that cold based ice covered the highlands repeatedly. We conclude (as did the reviewers) that support for later ice, post LGM, is less robust. Rather, our data are consistent with several different scenarios (all related to a dynamic permafrost environment) after the Laurentide ice ablated including stripping of a till cover, rolling of sampled blocks, and burial by ice, snow and rime.

We therefore have revised parts of the manuscript emphasizing the data related to basal thermal conditions (more certain finding) and minimizing our focus on the history of the summits after deglaciation (less certain, more speculative finding). We have diversified our discussion of the post-glacial history to include several hypotheses about periglacial processes.

Maybe ice sheet surface profiles, extended northward from the Last Glacial Maximum terminus position, could bolster the CRN data?
We have used the Nye equation and simple models varying basal shear stress to show that both Katahdin and Mt. Washington were ice covered assuming reasonable basal shear stress values of > 0.4 bar. This is now mentioned in the text and profiles are included in the supplemental data.

A second issue is the presentation of the research, and is related to the above. I find it ambiguous whether the authors are meaning to justify the research by testing a model of ice sheet occupation of the summits that is supported by the literature (lightly weathered erratics, till patches with weak soils) with cosmogenic isotope measurements. Or whether they a priori assume based on this (qualitative) evidence that the summits were occupied by the ice sheet during the Last Glacial Maximum, and are rather doing the research to constrain the pattern of erosion and sub-ice conditions.
We agree that some of our presentation was ambiguous and have rewritten the paper to emphasize what we know already from previous work (lightly weathered erratics, till patches with weak soils) and what we have learned from the isotopic measurements (erosion and sub-ice conditions). Our paper follows the reviewer's second suggestion.

For example, on line 47, near the beginning of the paper, regarding the qualitative data presented in past literature, the authors write "Testing these observations has been stymied by the difficult of dating…" The words imply that the point of this study is to test the observation with improved approaches.
Exactly, we are using new approaches to understand better the landscape building our work on established field observations of others.

Versus line 132 in the discussion, where the authors write "When considered along with the geologic evidence that the summits were overrun by ice…" and then continue to make that assumption throughout duration of the paper.
We take at face value the repeated observation by others of exotic, erratic clasts on the summit areas of both mountains implying unambiguously that the summits were overrun by Laurentide ice. We have rewritten and reorganized the first part of the manuscript to make these observations more clear to the reader.

My feeling is that these authors can do better with this manuscript with more time spent on it, and perhaps they can re-tool it so it includes the obvious possibility of local ice cover that the manuscript ignores in its present form.
We have spent much more time on the manuscript but we do not believe that the data support local ice cover and have explained why in the revised manuscript.

I could see after a re-write that this paper might be suitable for Geology, it has a lot of potential.
We hope that our re-write of the paper has made our thinking more clear to readers.

Minor issues, hoping that authors can use these comments to improve/streamline manuscript:

Line 24, replace "considering" with "assuming" ?

We have replaced considering with "considering field evidence" which is specific and direct. The field evidence is elaborated upon in the paper.

Line 35, ice or snow persisted on the peaks for millennia after (AND PRIOR to) the last glaciation of the summits.
This entire sentence has been removed.

Line 75, sentence beginning with "Comparison.." This sentence is virtually the same as the previous one, condense.
The sentences have been combined and shortened to remove redundancy.

Line 78, "…several hundred ky…" is a bit long, but in any case, this depends on the uncertainty of Be and Al measurements, which are quite low these days, especially Al data at PRIME.
True the time of burial depends on measurement precision. The data reported here were gathered before the recent (2014) measurement advances made at PRIME. More important, as shown by modeling we have done for this revision, is that the ratio does not drop significantly if there is intervening, interglacial exposure.

Line 82. It mentions that "glacially polished" bedrock was sampled, but nowhere else in the paper is it mentioned what ages came from these types of samples, nor are sites in the Tables described as glacially polished.
Thanks for pointing out this omission. We have added additional information to the supplemental information table S1 describing each sample site. We have also corrected polished to molded.

Line 96. Data section. Would be helpful to see errors in text.
We have added uncertainties to the text for all ages.

Line 98. Make consistent reporting of significant digits. Also 9.28 is younger than what text says is youngest age on line 101 as 9.6 ka.
The reviewer is correct that we needed to standardize significant figures, which we have now done. However, the reviewer is incorrect in comparing 9.28 with 9.6. The former is a single nuclide age; the latter is an average of the two nuclides. We have tried to more clearly emphasize the difference in the text adding an underline to the relevant phrases and using the modifiers average and single nuclide where appropriate to avoid confusion.

Line 102. Says 153 ka is oldest age, two sentences prior says 156 ka.
Same issue as above, reviewer did not notice that we specified the difference in the text between single nuclide and average (26Al and 10Be) ages.

Line 109. Sentence beginning with "At 2 SD…" should be condensed with the

beginning of the paragraph when the text explains the concordance of Al and Be ages, otherwise repetitive.
We disagree with the reviewer here and believe keeping the two different sentences here is important for clarity, especially for readers less familiar with the isotopic system than the reviewer.

Line 157. You write that it takes 29 ky of burial to zero a 14C inventory. At some point (even in sup) you should explain where this value comes from, is it from a paper (then cite), or based on a certain measurement ability to distinguish from background?
Good suggestion, we have added wording to the text to clarify this.

Line 159. The "…plus 29 ky…" should be ">29 ky"
Change made as requested.

Line 181. "around" vs. using the "~" symbol. Be consistent.
Change made as requested.

Figure 3 caption. Cite benthic d18O data.
We have done this.

Figure 2. I would find it helpful to see all the data (ages) on this figure.
We have made this modification to the figure.

Figure 3. The vertical shaded zone labeled "minimum burial (29 ky) to remove pre-LGM 14C" is only ~20 ky wide.
We have made this modification to the figure

Reviewer #2: I have given this paper the highest rating possible and have only indicated minor revisions to take care of some minor editorial changes, mostly with references. This is an excellent paper and one that gives us a major leap forward in our understanding of the overall geomorphic alteration of landscapes by ice sheets. Although often suspected, this paper finally proves that there was minimal erosion across the tops of high mountain peaks in northern New England. It takes advantage of both Be and Al cosmogenic ages but also employs the use of new insitu measurements of C14. It is clear from this paper that the overall relief of New England is increasing with repeated glaciations as high peaks are essentially not eroded due to a frozen bed and many valley areas are heavily scoured with rock surfaces below sea level, for example the Connecticut Valley.
This is an interesting comment, made by two reviewers, which has caused us to add this line of thinking to the paper.

I was especially astounded at the high inheritance of blocks in deposits of periglacial origin. The paper sets up many spin off studies, by establishing the technique, and also sparking many ideas about where to try this next. For example are peaks at slightly lower elevations, such as Mt. Monadnock and the quartzite ridges of western New Hampshire heavily scoured or not? The paper also sheds some light on the amount of snow cover that occurred during the last glacial period both before and after the arrival of continental ice. This has importance to deciding whether cirque glaciation is possible as the continental ice sheet arrived or immediately following its recession from the high peaks. The main contribution is that it shows how to use the cosmogenic technique as a tool for assessing erosion in a glaciated terrain with varying bed conditions.

Here are some minor editorial changes that should be made by line number:

Line 136 - Should Briner et al. be 2014 or is Briner et al. 2006 missing from reference list.
Briner et al., 2006 was left out of the reference list and has now been added.

Line 161 - I have read this line many times - Should this say "colder" than today instead of "warmer".
This was a typo, now corrected.

Line 271 - Goldthwait, 1970 reference should come after the Goldthwait 1940 reference.
Corrected.

In supplement references:

Shouldn't the title of the "References Cited" be "Additional References Cited" since many of the references in the main paper are not listed here.
Corrected to References Cited in Supplemental Material

The two Anderson references are not used or else I could not find them.

"COST-727" should be "COST, 2007"
Rechecked, COST-727 is correct title and we cited as suggested on the cover page of the document.

The Dorian reference can be omitted since it is cited in the main paper text.
We left this in the supplement under the belief that the supplement needs to be freestanding

Reimer et al, 2014 on Table S4 is not referenced here.
This has now been added to the reference list.

Reviewer #3: This is an exciting report that is low on sample number, rich in data and long on interpretations that are forced by the multiisotope data. I would like the authors to (1) better explain the local spatial/topographic context of where they collected samples, since that context seems central for their inferences about snow/ice cover and its persistence;
This information has been added to column B of Table S1.

and (2) reassure me and other readers that the 14C production rates are correct.

We believe that the production rates used for 14C are reasonable estimates. The PR that is used is based on 4 studies with multiple samples analyzed per study - all now recalculated using methods of Hippe and Lifton (2014). All are well-constrained in age - Bonneville is now published (Lifton et al., 2015, QG CRONUS volume - yields 12.3 ± 0.4 at/g/y St), Scotland is soon to be submitted as part of CRONUS QG volume 2 (yields 12.0 ± 1.4 at/g/y St), Schimmelpfennig et al. (2012) yielded 12.1 ± 0.6 at/g/y St originally, and Young et al. (2014) yielded 14.4 ± 0.9 at/g/y St. The original mean value derived for Lal/Stone elevation/latitude corrections is 12.7 ± 1.1 at/g/y. However, we recently to recalculated the NZ data of Schimmelpfennig et al. using the updated Hippe and Lifton (2014) procedure and come up with 13.7 ± 0.6 at/g/y St. This changes the mean somewhat to 13.1 ± 1.1 at/g/y - within 1 sigma of the earlier number, but a bit higher. Resulting 14C ages would PTK-7: 10440 ± 1930, and PTMW-3: 11940 ± 2380 (still within 1 sigma of the previous values). A new 14C paper from ETH is out now with a depth profile from Spain (Lupker et al., 2015), and their results agree with our mean value - 12.8 at/g/y spallation, and the muon results are consistent with those of Heisinger et al. (2002), on which our calculations are based. In summary, there is no evidence that there are uncertainties major or important enough associated with the 14C production rate to change our conclusions. 14C ages are much, much younger than 26Al and 10Be ages of the same samples.

This work offers a novel mechanism for producing or maintaining relief in an environment shaped by ice erosion.

We thank the reviewer for making this important observation, which had not occurred to us and is now mentioned in the manuscript.

Things that concern me.

1. Sample locations/local topographic relations. You necessarily are working

with a small number of samples and much of your interpretation rests on being able to interpret where these blocks came from, their local topographic context, and their recent history. Samples clearly are local and some reflect a long history of exposure. But could the others be "lower" blocks from the same outcrop or covered with some till until recently? The answers and interpretations are important, but of particular significance for the 14C concentrations and for the other samples that are "too young".

This is an interesting and astute observation by the reviewer that made us think and in the end suggested to us that we needed to revise our interpretation of the 14C data. The reviewer is correct that removal of surface cover (till, snow, ice, or eroded rock) would lower measured concentration of 14C and leave high 26Al and 10Be concentrations. This loss of mass is a viable alternate hypothesis that obviates the need for ice carapaces or local glaciers after the LGM and is in fact, more consilient with other data we have such as several young ages (see comments below).

Are the sample sites places that seem likely to have accumulated snow or rime both before and after the LGM? Your illustrations suggest these are narrow, windblown summits that do not accumulate much snow or long-lasting rime in the modern environment. Adjacent to such areas are places where drifts are persistent in the modern. I presume if you had photos of the outcrop areas that we'd see them in the ms?

We have added an additional photograph to Figure 2. True it is hard to imagine large amounts of snow or ice in today's climate but we don't know about the past - in colder climates.

2. Three-isotope system and 14C. Having a three-isotope system is remarkable and 10Be and 26Al are a good and well-understood check. Is 14C as well understood? How well is the production rate known? Is 14C geochemically stable after it forms under all conditions? A lower production rate or having just a little more 14C would solve several issues. And is analytical uncertainty such that you really need to bury the 14C samples for 5 half-lives, or would 3 or 4 half-lives do?

As noted above, we argue that the spallogenic PR for 14C is reasonably well-known now. In situ 14C is thought to be stable after formation - we don't have any evidence that it's not. Analytical uncertainty is typically a few (2-4) percent - comparable to 10Be or slightly higher. Interlab comparisons, though, suggest lab reproducibility for nuclides other than 10Be are above measurement uncertainties (Jull et al., 2015 - CRONUS QG, and Lifton et al., 2015, NIMB AMS13 volume in press). A calculation of saturation concentrations at the two sites, followed by burial and decay by x half-lives, suggests that 5 half-lives gets you to the level of the 1 sigma measurement uncertainty.

3. Didn't summit areas emerge even earlier? As the ice front was retreating,

the regional ice surface was lowering such that these high-elevation sites would have poked out first; the nearby and more distant low-elevation dates provide only a lower limit for deglaciation of sites 1 km above them. Is it a close limit? What happened to Laurentide ice during the time between the stable Cape Cod margin and Pineo Ridge time? Was the ice profile essentially the same until BA time? I know we don't have a clear sense of the regional ice profile or how the basal shear stress changed over time, but the ridge sites should have been covered last and first out.
These are all great questions, but there is little data to address them. We agree that summits emerge early as the ice surface lowers (we have added wording about this to the paper) and low sites give only a minimum limit on exposure ages. Beyond that we don't view this comment as germane to the central focus of our paper.

4. Paleoclimate? Is the modern temperature and a plausible lapse rate consistent with cold-based ice at 21 ka….and cold-based both on Mt. Washington and on Little Haystack, some 300 m lower.
We are not sure how to respond to this statement. We have added a sentence to the text describing how permafrost is found on Mt Washington even today so that during colder glacial times it is plausible that high points such as Washington and Haystack were both cold based.

Comments/suggestions keyed to line numbers in the manuscript

29-32. Invert ideas a bit or break into two sentences; your Geology readers aren't familiar with cosmogenic 14C and you start with the inference (snow or ice covered)….rather than the young accumulation ages
This sentence has been rewritten in the abstract

34-35. Hope you can develop this idea in a plausible manner. None of these summits hold snow well in the modern environment—they're just too windy.
After revision, we have removed this sentence from the abstract and downplayed the idea in the paper.

39. Throw in Little Haystack and its elevation here? Snow and rime covered, perhaps, but generally the cover is thin.
After revision, we have removed this sentence from the abstract and downplayed the idea in the paper.

59-61. Wouldn't you guess that these summit areas would have been exposed somewhat earlier than the valley sites as ice thinned rapidly and mainly flowed through nearby low areas.
Yes, we have reworded the text to reflect this suggestion

67. This story of retreat seems half-told. What happened over the next 10,000 years between Cape Cod and the readvance north of the Presidential Range? Does the ice profile relax and thin early in this process, exposing the summit areas, or only after ~15 ka? I know you are out of room, but you could fill a bit of this gap…since there is quite a bit of detail in this section!
We have tried to add some more information here. We have considered more extensively the varve record in the Connecticut lowland as well as the cosmogenic data from Maine in Davis et al (2015).

68. Why accurate?
Removed "accurate"

70. Particularly true in high-relief terrain where different portions of the same ice mass are behaving differently?
Sure, more likely in high relief terrain. We have added a statement to this effect.

71. "that may" (for involving)
Sentence reworked for clarity

74. Though it is difficult to get a unique solution.
Wording added.

82. "Frost-riven"? Possible to tell where they came from, or only that they were "local"? In one sense it doesn't matter, since you have "too much " exposure. In another sense, you may have gotten different apparent exposure ages from blocks that represent lower parts of depth profiles in bedrock or beneath and eroding till cover. The young ages are a challenge.
We agree with this comment and have added discussion now that suggests this is the most plausible explanation for the low 14C ages extending in to the late Pleistocene.

88. Say from where? (one Mt. Washington; one from Katahdin)
Wording added.

90. Is the 14C content of samples stable—any mineralogic or microfracture effects?
There is no reason to believe that 14C is not stable in rock. See response above (things that concern #2).

100. 10Be ages have smaller errors, right?
Correct.

102. Is there meaning in this "too young" value?
Yes, thanks to reviewer suggestion we now believe that this "too young" sample

is important in that it suggests shielding and then loss of that shielding mass after the LGM. We cannot know whether that shielding mass was ice, snow, till or rock but any of the above are plausible in the dynamic cold periglacial environment. We have altered the manuscript to reflect this change in thinking spurred by the reviewers #1 and #3 thus resolving the issue that caused reviewer 1 to request revision of the manuscript.

114. The CRN evidence shows that erosion was ineffective locally; is there any morphologic evidence that allows you to generalize these results or to know how far down the mountain ranges you'd need to go to find effective erosion?
On the summits and near summit areas we see little evidence for warm based ice – no striae, no molded forms. Below the summit of Mt. Washington, we find a roche moutonnee at 1680m and evidence lodgment till at 1820 m. We have added a short description regarding this to the text in an attempt to generalize our results for workers in other areas.

130. Young or too young ages—interpretation possible? See line 167 as well.
Yes, see response to comment above (line 102)

138. On several of ….
Text added.

150-151. Do the 14C data allow a shorter time? Could these samples have been below a thin till cover without changing the 10Be exposure age significantly (since it is complex in any case)? The interpretation based on 14C seems too long for Laurentide ice cover, cirque glaciers would not have covered these sites and it is hard for me to believe that sites near these sharp summits could have preserved snow cover long term unless the wind regime was completely different than it is at present or you were in a drift zone.
This comment and similar comments by reviewer 1 led us to reconsider how we interpreted the 14C data and thus we have changed our approach in the manuscript. As described above in response to reviewer 1, we now provide multiple, testable hypotheses for why the 14C ages are younger than the timing of deglaciation rather than just suggesting ice and rime cover.

168-169. Could you do all of this with a thin till or rock-block cover? Would the heavily dosed samples have been reset significantly?
Yes we can and we have added this type of thinking to the manuscript.

170-174. So it is easy to imagine persistent cold during 800 years of the Younger Dryas, but most evidence seems to suggest no cirque glaciers during that time period. Before and after YD it was warm, at least according to the pollen in local bogs and many other things we believe. Summer should have been warm, melting rime ice and any snow cover away from persistent drifts. So it seems as though you need to invoke a different lapse rate, a persistent cloud cap, or some other mechanism that makes these mountain areas behave like the High Arctic?

Accepting suggestions from reviewers 1 and 3, the issue of maintaining ice on the peaks after regional deglaciation is now moot. We describe above how we have changed our interpretation of these data.

179. This idea would seem more plausible to me if it seemed as though sample sites were likely locations of long-lived drifts at present, and thus permanent drifts during colder times. Absent drifts, why not invoke a thin cover of drift that eroded away? The rocks are too hard for the removal of significant thicknesses in a short period.

We agree with the reviewer and now offer multiple hypotheses to explain the 14C data. We cannot know whether the now missing mass was snow, ice, rock, or till or whether the young 14C ages reflect rolling of blocks in a dynamic periglacial environment.

183-188. Cirque glaciers before Laurentide ice arrives seem reasonable, but what mechanism would allow them to extend up to cover the windswept summit areas at Washington and Katahdin…..and Little Haystack? Climate would not only have to be colder, but very different. Could the 14C ages be too young for some other reason?

We agree (see above responses) with the reviewer's reasoning and have changed the wording of the manuscript. As stated above, we see no methodological reason to suspect that 14C ages are too young – effects of production rate scaling, changes in atmospheric pressure history, and production rate determination are all small in comparison to the deficit we measure between 14C ages and 10Be and 26Al ages.

190-192. A little challenging to have it both ways?

This contradiction has been removed as we have (thanks to reviewer 1 and 3), rethought our interpretation of the 14C data.

200. But large elevation difference between Mt. Washington and Little Haystack—at PMP at all elevations in between?

With a limited number of samples, we don't know the basal thermal conditions between the sample points but we suspect that in the valley's there is fast flow (more heat from deformation) and more erosion and that there is less flow over the summits and therefore less effective erosion – especially if ice at the high points is frozen to the bed. There are likely interactions between flow, topography and basal thermal conditions.

331. Why do B and D include ages? Note in caption?

This has been corrected per reviewer 1 suggestions and an extra panel added to *figure 2.*

332. Same as PTDK-7, analyzed for 14C?
We have corrected this and checked all nomenclature for consistency

DP Dethier

Geology

Cold-based, Laurentide ice covered New England's highest summits during the Last Glacial Maximum

--Manuscript Draft--

Manuscript Number:	
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Abstract:	To better understand glacial history and process in New England, a mountainous area overrun by the Laurentide Ice Sheet, we measured three cosmogenic nuclides in nine upland samples. The concentrations of 10Be and 26Al in some samples collected near the summits of Katahdin and Mt. Washington are 2 to 10 times higher than expected for a single exposure period, considering field evidence indicating continental ice covered all New England peaks during the Last Glacial Maximum. In situ 14C exposure ages from the same summits are much younger than 10Be and 26Al ages, suggesting that high elevation sampling sites were ice-covered before and during the Last Glacial Maximum. Field and isotopic data are consistent with New England summits being covered in part by cold-based, continental ice unable to erode a significant thickness of rock. The contrast in erosion rates between stable summits and deeply eroded valleys may contribute to the development and maintenance of northern Appalachian topography.
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	Jack Ridge Jack.Ridge@tufts.edu Knowledge of area and chronology
	Jason Briner jbriner@buffalo.edu

	Knowledge of isotopes and cold based ice

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Cold-based, Laurentide ice covered New England's highest summits during the Last Glacial Maximum

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Abstract

To better understand glacial history and process in New England, a mountainous area overrun by the Laurentide Ice Sheet, we measured three cosmogenic nuclides in nine upland samples. The concentrations of ^{10}Be and ^{26}Al in some samples collected near the summits of Katahdin and Mt. Washington are 2 to 10 times higher than expected for a single exposure period, considering field evidence indicating continental ice covered all New England peaks during the Last Glacial Maximum. *In situ* ^{14}C exposure ages from the same summits are much younger than ^{10}Be and ^{26}Al ages, suggesting that high elevation sampling sites were ice-covered before and during the Last Glacial Maximum. Field and isotopic data are consistent with New England summits being covered in part by cold-based, continental ice unable to erode a significant thickness of rock. The contrast in erosion rates between stable summits and deeply eroded valleys may contribute to the development and maintenance of northern Appalachian topography.

Introduction

Northern New England is characterized by mountainous terrain repeatedly overrun by the Laurentide Ice Sheet (LIS). The highest peaks in Maine and New Hampshire are Katahdin (1606 m) and Mt. Washington (1917 m) (Figure 1). Their rocky summits are barren, windblown, and can be snow-covered for months each year. Blockfields testify to periglacial activity on the peaks (Goldthwait, 1940; Davis, 1989). More than a century of study (Thompson et al., 1999) has answered some questions about glaciation of this landscape, but mysteries remain.

The presence of erratics on the summits shows unambiguously that continental ice overrode all New England peaks (Tarr, 1900; Goldthwait, 1916, 1940, 1970; Davis, 1976, 1989). Poorly developed soils and thin weathering rinds suggest that Last Glacial Maximum (LGM) ice covered all of New England, but there are few quantitative estimates for the age and duration of this ice cover over the peaks. Moreover, it is not clear whether the ice was warm-based and erosive or frozen to the bed such that it preserved relict landscapes from prior interglacials. Here, we report measurements of *in situ* produced cosmogenic ^{10}Be, ^{26}Al, and ^{14}C in samples collected from the uplands of Katahdin and Mount Washington. We use these data to constrain the timing of upland deglaciation, determine the basal thermal regime of the LIS where it covered these peaks, and speculate about controls on landscape development in previously glaciated, mountainous regions.

Background

Accurate surface exposure dating assumes that a sampled surface was eroded deeply enough to remove nuclides produced during prior exposure (Bierman et al., 1999). Landscapes covered by cold-based, non-erosive glacial ice violate that assumption, and may preserve a record of multiple periods of exposure and burial (Bierman et al., 1999; Briner et al., 2006, 2014; Harbor et al., 2006; Corbett et al., 2013). In such cases, nuclides with different half-lives (e.g., ^{10}Be, 1.4 My; ^{26}Al, 0.7 My; ^{14}C, 5.7 ky) can be used together to constrain complex exposure scenarios (Granger and Muzikar, 2001; Corbett et al., 2013; Briner et al., 2014). In glacial landscapes dominated by cold-based ice, the ratio of ^{10}Be and ^{26}Al can be used to detect exposure followed by burial only if that burial lasts >100 ky; however, the shorter half life of *in situ* ^{14}C makes it useful for detecting shorter burial periods (≥ ky) (Miller et al., 2006; Goehring et al., 2011; Briner et al., 2014).

The deglacial history of New England's lowlands is well constrained by the Connecticut River valley varve record (Figure 1; Ridge et al., 2012) and suggests regional deglaciation of the Mt. Washington area occurred by ~14 ka, a finding supported by ^{10}Be exposure ages of glaciallytransported boulders on nearby moraines (Balco et al., 2009; Bromley et al., 2015). Cosmogenic exposure dating shows that the lowlands around Katahdin were deglaciated ~15-16 ka (Davis et al., 2015). Application of cosmogenic nuclides elsewhere in New England (with data recalculated using the regional production rate, Balco et al., 2009) shows that the LIS was at its maximum extent on Martha's Vineyard until ~27 ka and then slowly retreated tens of km to Cape Cod (Balco et al., 2002) and coastal Connecticut (Balco and Schaeffer, 2006).

The deglacial chronology of the northern New England uplands is poorly constrained. Wood and charcoal are rare in glacial and immediately post-glacial deposits, especially in alpine terrain where vegetation is scarce and the onset of primary productivity in lakes is delayed (Davis and Davis, 1980; Bierman et al., 1997). Ice-sheet profiles (e.g., Davis, 1989), modeled using basal shear stress values appropriate for rugged, crystalline terrain (0.6 to 1.0 bar), suggest that when the LIS was fully advanced, the high peaks of Katahdin and Mt. Washington would have been under ice (Supplemental Information). During advance, the peaks would have protruded from the ice as nunataks; similarly, during retreat, the summits would have been exposed while ice continued to flow through the adjacent lowlands.

Methods

We collected samples from frost-riven blocks and glacially molded bedrock surfaces (Figure 2; Supplemental Information, Table S1) on and near the summits of Katahdin (n=2), Little Haystack (n=1), and Mt. Washington (n=6). Be and Al were extracted at the University of Vermont (Table S2). We made isotopic analyses at Lawrence Livermore National Laboratory. About 5 g of pure quartz from two summit samples (one from Katahdin, the other from Mt. Washington) were processed for *in situ* ^{14}C analysis at the University of Arizona (Lifton et al, 2001; Table S3). Exposure ages (^{10}Be and ^{26}Al) were calculated using the CRONUS calculator (wrapper script: 2.2, main calculator: 2.1, constants: 2.2.1, muons: 1.1, Balco et al., 2008) and Lal (1991)/Stone (2000) time invariant scaling of the northeastern North America production rate (Balco et al., 2009). In situ ^{14}C exposure ages were calculated using a modified version of the CRONUS calculator.

Data

Samples from on and near the summits of Katahdin, Little Haystack, and Mt. Washington (1326 to 1896 m asl) have single-nuclide ^{10}Be, ^{26}Al, and ^{14}C exposure ages ranging from 9.3±0.6 to 156±8.3 ka (Table S1; Figure 3). Because ^{10}Be and ^{26}Al exposure ages are positively and linearly correlated (R^2=0.996; slope=1.03), we use the uncertainty-weighted average of ^{10}Be and ^{26}Al ages for discussion and in figures.

Exposure ages (^{10}Be and ^{26}Al) for most samples pre-date the LGM. ^{10}Be and ^{26}Al ages for seven of nine samples collected from the summits and uplands are greater, in some cases much greater, than the ~14-16 ka regional deglaciation ages (Figure 3). One sample from a frost-riven block on the summit of Mt. Washington, PTMW-03, has an average ^{10}Be and ^{26}Al exposure age of 153±5.8 ka, more than 10X the age of regional deglaciation. Similarly, a sample from the summit of Katahdin has an average ^{10}Be and ^{26}Al exposure age of 35.6±1.4 ka, more than 2X the regional, LIS deglaciation age of 15-16 ka. Samples collected near one another have very different average ^{10}Be and ^{26}Al ages. For example, four samples from bedrock at “Goofer Point” on Mt. Washington have average ^{10}Be and ^{26}Al exposure ages of 17.9±0.7, 18.4±0.9, 26.8±1.1, and 71.3±2.7 ka. Two *in situ* ^{14}C exposure ages on samples from the summits of Katahdin (PTK-07) and Mt. Washington (PTMW-03) are much younger (11.0±2.2 and 12.7± 2.8 ka) than corresponding average ^{10}Be and ^{26}Al ages (35.6±1.4 and 153±5.8 ka, respectively).

Discussion

Late Pleistocene ^{14}C exposure ages, theoretical ice profiles, unweathered erratics, and poorly developed soils all suggest that the uplands of Maine and New Hampshire were covered during the LGM by the LIS. However, pre-LGM ^{10}Be and ^{26}Al average exposure ages from the summits indicate that the LGM LIS did not substantially erode the peaks. Similar to those working in other glaciated terrains (e.g., Bierman et al., 1999; Briner et al., 2006, 2014; Miller et al., 2006; Corbett et al., 2013), we interpret these old ages as evidence for the presence of now-vanished, cold-based ice.

Other evidence is consistent with ice at summit elevations being frozen to the bed during the LGM. On and near summit areas, we found no striae, in contrast to those seen on lower bedrock summits in New England. Even today, >10 ky after deglaciation, the summit of Mount Washington remains cold. Permafrost has been identified at depths below 6 m in the summit water well, based on thermistor measurements (Bent, 1942; Howe, 1971), an observation supported by lapse rates and measurements elsewhere in the Appalachians (Walegur and Nelson, 2003).

The effectiveness of glacial erosion appears to vary over time and space, most likely due to ice near the pressure melting point (e.g., Briner et al., 2014). For example, some samples carry the equivalent of 10^5 yr of surface exposure, while others contain inherited ^{10}Be and ^{26}Al equivalent to only several ky of pre-LGM surface exposure. Not far below the 1917 m summit of Mt. Washington and sample site PTMW-03 (153±5.8 ka), we find evidence of warm-based ice: a roche moutonnée at 1680 m and lodgement till at 1820 m (Fig. 2C). In addition, some sample sites with pre-LGM ages appear glacially molded suggesting the presence of warm-based ice at some time in the past, prior to the LGM.

Measurements of *in situ* ^{14}C suggest that much of the ^{10}Be and ^{26}Al we measured were produced during an earlier period of exposure followed by a period of burial and preservation under ice rather than by continuous exposure of the summits as nunataks. Two summit samples (PTMW-03 and PTK-07) have old average ^{10}Be and ^{26}Al exposure ages (153±5.8 ka and 35.6±1.4 ka), but *in situ* ^{14}C ages of only 12.7±2.8 and 11.0±2.2, respectively. Together, the multiple isotope data demonstrate at least two different periods of exposure separated by a period of burial during which ^{14}C produced during earlier exposure decayed but long-lived ^{10}Be and ^{26}Al remained; otherwise, ^{14}C would be present at concentrations consistent with continuous exposure. Because the ^{14}C exposure ages are within 2σ uncertainty of regional deglacial ages, we infer that burial related to the LGM was long enough that pre-LGM ^{14}C decayed away. Removing pre-existing ^{14}C from a sample exposed to saturation (~25 ky) requires ≥5 half-lives (≥29 ky, Figure 3).

The periglacial environment of Mt. Washington just after deglaciation appears to have had little effect on most ages. PTMW-04, a rock glacier block >500 m below the summit, has an average ^{10}Be and ^{26}Al age (12.6±0.6 ka) only slightly less than the regional deglaciation age (14 ka) suggesting that the rock glacier stabilized rapidly as climate warmed. The summit of Katahdin appears more dynamic. There, a bedrock sample (PTK-06) gives an exposure age (9.6±0.4 ka) much younger than regional deglaciation (15-16 ka). The ^{14}C exposure age (11.0±2.2 ka) of an angular block sitting on this sampled bedrock also post-dates deglaciation. These young ages most likely reflect stripping of till cover (Gosse and Phillips, 2001), shielding by snow or ice (Anderson et al., 2008), and/or erosion of sampled surfaces after exposure, although deep, post-glacial erosion of these hard rocks seems unlikely.

Multiple nuclide data can better constrain the timing of summit burial and exposure. The $^{26}Al/^{10}Be$ ratio in New England summit samples (average 6.68±0.39; Table S1) is indistinguishable from the production ratio of these nuclides (6.75; Balco et al., 2008), precluding long burial times (100s of ky) after initial exposure. Intermittent and repeated burial lasting tens of ky of every 100 ky glacial cycle is plausible without changing the ratio enough that such burial would be detectable (Supplemental Information). The ^{14}C data indicate at least ca. 29 ky of burial. Using this metric, data from all samples except PTK-06, PTMW-03, and PTMW-04 are consistent either with initial exposure beginning between ca. 102 ka and 47 ka (stated ages on Figure 3 and in Table S1 plus ≥29 ky of burial around the LGM when no nuclides were produced) when climate was substantially colder than today (Figure 3). However, PTMW-03, with an average exposure age of 153 ka, requires additional exposure prior to MIS 6, the previous glacial period. Using the LGM and ≥29 ky of burial inferred above as an analogy, initial exposure of this sample must have occurred ≥200 ka (Figure 3).

The need for ≥29 ky of burial to decay away pre-LGM ^{14}C suggests that ice covered the summits starting at least ~40 ka. This ice was likely local because sea level records indicate that major expansion of the LIS did not begin until ~31 ka (Lambeck et al., 2014) and the LIS was not fully expanded until ~27 ka (Balco et al., 2002). Accumulations of ice on the summits may have fed pre-LGM alpine glaciers that cut the cirques on both Katahdin and Mt. Washington (Waitt and Davis, 1988) before being overwhelmed by continental ice that likely advanced through Maine (and by inference, northern New Hampshire) ~29 ka, based on calibrated ^{14}C ages on shells, paleosols, and wood found in the basal sections of lake sediment cores (Dorion, 1997; Table S4). Post-LGM, climate warmed and equilibrium lines rose too quickly for the cirques to be reoccupied by alpine ice after regional deglaciation (Waitt and Davis, 1988; Loso et al., 1998). At Mt. Washington, the similarity of the summit ^{14}C exposure age (12.7±2.8) and the regional deglaciation age (~14 ka) is consistent with rapid lowering of the LIS surface during deglaciation, similar to the inference made by Davis et al. (2015) for Katahdin.

Implications

Data from three different cosmogenic nuclides produced in New England summit outcrops and frost-riven blocks show that ineffective glacial erosion, and thus the presence of cold-based ice frozen to the bed, is not limited to polar regions (e.g., Bierman et al., 1999), high latitudes (Marquette et al., 2004), or the thin ice sheets of the mid-continent (Colgan et al., 2002). Comparison with samples collected at lower elevations (Davis et al., 2015; Bromley et al., 2015) indicates that weakly erosive ice was restricted to the summits, likely because ice was thinner and below the pressure melting point only there. The limited distribution of cold-based ice fits well with the small number of New England boulders carrying significant concentrations of inherited nuclides (Balco et al., 2002, 2009; Balco and Schaefer, 2006; Davis et al., 2015; Bromley et al., 2015) and suggests that most LIS boulders were sourced from lowland areas where the ice was warm-based and erosive. We show that in high-relief terrain, portions of the same ice mass can behave differently, with cold-based, non-erosive ice covering the uplands, and warm-based, erosive ice in the deep valleys providing a mechanism for producing relief in an environment shaped, at least in part, through glaciation by a large, continental ice sheet.

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Figure Captions

Figure 1. Location of sampling sites at Katahdin, Little Haystack, and Mt. Washington indicated by dotted lines. Connecticut River valley, location of New England varve chronology, is also shown. Elevation indicated by shading.

Figure 2. Location of samples, with ages, and photographs of three sample sites. A. Overview of Katahdin showing location of summit samples. B. Sample site PTK-07 on summit of Katahdin. C. Overview of Mt. Washington showing location of summit samples (PTMW-01,-02,-03, and PTD94-20, 21) and rock glacier block sample (PTMW-04). D. Sample site PTMW-03 on Mt. Washington. E. Overview of Franconia Ridge showing location of sample PTD94-19 on Little Haystack. F. Sample site PTD94-19 on Little Haystack.

Figure 3. Schematic history of exposure of samples included in this paper. Benthic ^{18}O record proxy for global ice volume (Lambeck et al., 2014). Grey bars are uncertainty-weighted average (^{10}Be, ^{26}Al) exposure age for each sample. White arrows are *in situ* ^{14}C exposure ages. Grey shaded area represents five half-lives of ^{14}C (~29 ky) required to decay ^{14}C created prior to overrunning by LIS. Regional deglacial age (14-16 ka) shown by dotted line. Two isotope diagram (inset) shows ^{26}Al/^{10}Be ratios of samples. Error bars are 1 SD.

Figure 1. Bierman et al.

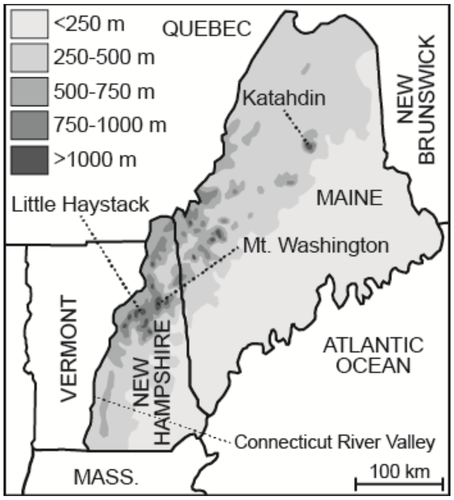


Figure 2. Bierman et al.

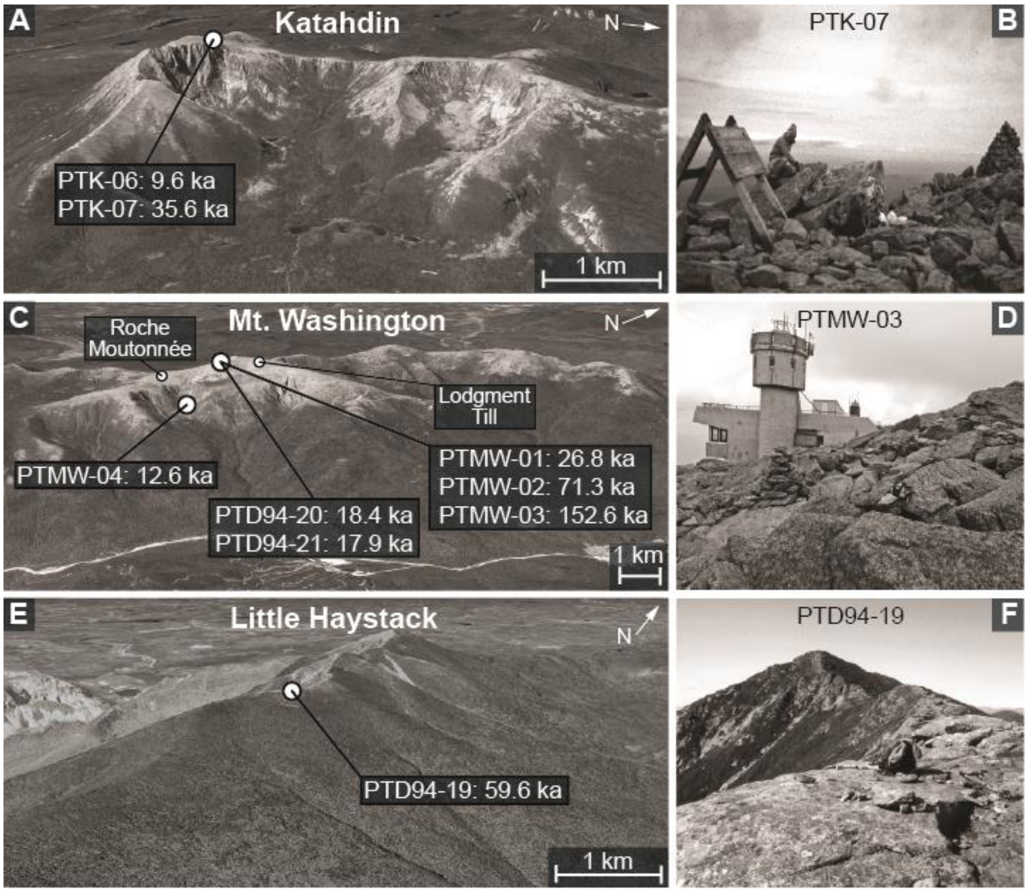
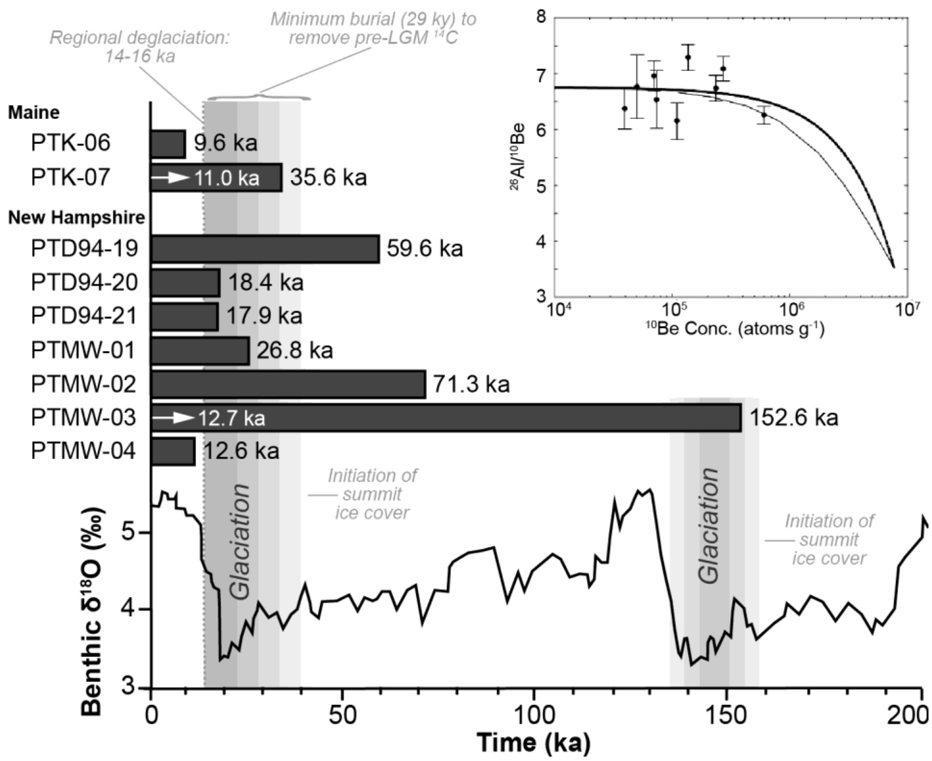


Figure 3. Bierman et al.



Supplemental Information – Bierman et al.

1. Laboratory and data reduction methods

For ^{10}Be and ^{26}Al analysis, about 250 µg of 1000 ppm SPEX ^{9}Be carrier was added to each sample and to the two process blanks included with each batch of 6 samples. If needed, ^{27}Al carrier was added to samples and about 2000 µg of ^{27}Al (1000 ppm SPEX Al standard) was added to the process blanks. We removed two small aliquots (representing 2.5% and 5% of the sample, respectively) from each sample directly following digestion. Using these aliquots, the total mass of Al and Be was quantified using Inductively Coupled Plasma Optical Emission Spectrometry. Following isolation of Be and Al, samples were oxidized, mixed with Ag powder, and packed into cathodes for isotopic analyses at Lawrence Livermore National Laboratory.

When measured, Al data were normalized to standard KNSTD9919 with an assumed $^{26}Al/^{27}Al$ ratio of 9919 $\times 10^{-15}$. When measured, Be data were normalized to standards LLNL1000 and LLNL3000 with assumed $^{10}Be/^{9}Be$ ratios of 1000 and 3000 x 10^{-15} (See Table S2) Median ratios (and one standard deviation) for blanks processed with samples from New England were $2.40\pm1.81 \times 10^{-15}$ for $^{26}Al/^{27}Al$ (n=8) and $2.44\pm0.23 \times 10^{-14}$ for $^{10}Be/^{9}Be$ (n=9). These ratios were subtracted from measured ratios and the uncertainty propagated in quadrature.

Approximately 5 g of pure quartz from two of the samples (PTDK-7 and PTMW-3) was processed for *in situ* ^{14}C analysis following Lifton et al. (2001) and Miller et al. (2006) using extraction and purification systems at the University of Arizona. *In situ* ^{14}C was extracted from each sample using the recirculating system and techniques described by Lifton et al. (2001), Pigati et al. (2010), and Miller et al. (2006). The ^{14}C content of the samples was analyzed at the Arizona AMS Laboratory and blank-corrected following Lifton et al. (2001), using data reduction techniques described by Hippe and Lifton (2014).

Exposure ages (^{10}Be and ^{26}Al) were calculated using the CRONUS calculator (wrapper script: 2.2, main calculator: 2.1, constants: 2.2.1, muons: 1.1, Balco et al., 2008) assuming the northeastern North American production rate and Lal (1991)/Stone (2000) time invariant scaling (Balco et al., 2008) using the standards against which the samples were measured and the concentrations calculated from the measured isotopic ratios, the mass of quartz used, and the amount of stable ^{27}Al and ^{9}Be present (see Table S2). Note that the concentrations in Table 2 reflect the assumed value of standards at the time of measurement and that use of the CRONUS calculator takes into account recent changes in nominal values for these standards. The $^{26}Al/^{10}Be$ ratios in Table S1 correspond to those generated using the standard values now generally accepted (Nishiizumi et al., 2007).

Ages for *in situ* ^{14}C were calculated using a version of the CRONUS calculator modified for use with *in situ* ^{14}C, and Lal (1991)/Stone (2000) time invariant scaling. Global production rates for *in situ* ^{14}C were derived using calibration datasets from Lake Bonneville, Utah (Lifton et al., 2015), northwestern Scotland (Dugan, 2008), New Zealand (Schimmelpfennig et al., 2012), and western Greenland (Young et al., 2014). Each dataset was first recalculated following Hippe and Lifton (2014). Replicate analyses on individual samples were combined using inverse relative error-weighted means, and each site was then calibrated to a sea level, high latitude (SLHL) production rate separately using CRONUS calculator code. The arithmetic mean and standard deviation of the site-derived SLHL production rates was then computed and used in the exposure age calculations. Note that the lack of a regional ^{14}C calibration data means that we must rely on a global calibration.

2. Additional calculations: snow and ice cover, burial effect on $^{26}Al/^{10}Be$ ratio

It is possible that seasonal snow or ice cover could have reduced exposure ages For example, reducing an exposure age from 14.5 to 12 ky requires a nearly 20% reduction in cosmic ray dosing, which could be achieved by covering the samples with ~35 cm of water equivalent year-round (Schildgen et al., 2005). Since soft rime and wet snow, both common on the summits, have densities ranging between 0.2 and 0.6 g cm^{-3} (COST-727, 2007), to achieve the reduction in age we measure there would need to be between 1 and 3 m of frozen material present for 6 months per year since deglaciation 15 ky. This seems to be more ice and snow than is present today.

Intermittent burial of sampled outcrops by ice has minimal effect on the $^{26}Al/^{10}Be$ ratio of subsequently exposed rocks when exposure duration is greater than or equal to burial duration. As shown by Bierman et al (1999), only samples that have on average been buried for many times longer than they have been exposed will have $^{26}Al/^{10}Be$ ratios that are reliably below those resulting from steady exposure at the surface. In the case here, we posit <30 ky of burial by ice and 90 ky of exposure. As shown by the plot below (from Bierman et al., 1999) even a 50:50 ratio of burial to exposure would alter the ratio so that it dropped detectably below the steady exposure line only after many exposure/burial cycles with no surface erosion.

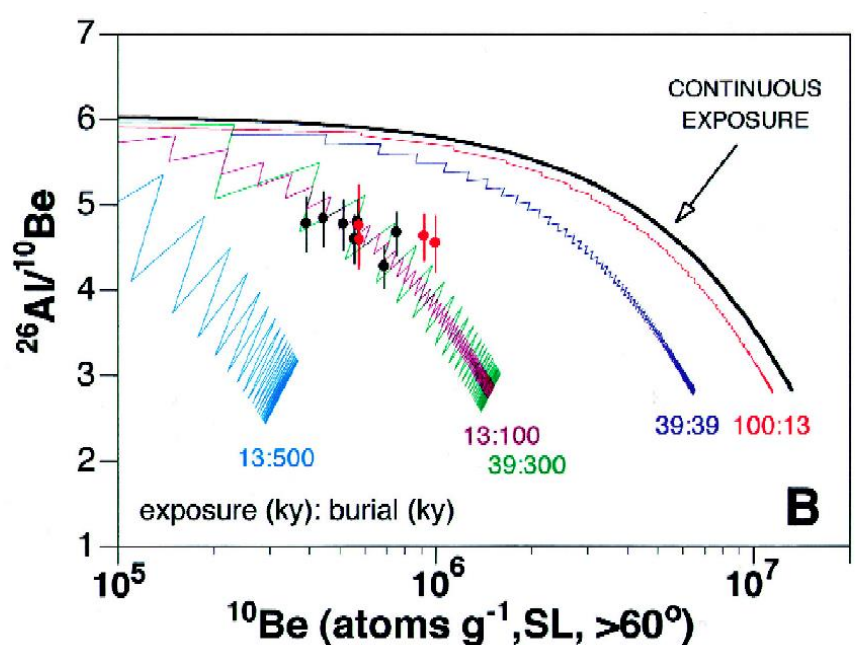


Diagram from Bierman et al. (1999) showing trajectories of samples exposed and buried. When exposure exceeds burial or is equal to burial in duration, it takes many glacial/interglacial cycles before the $^{26}Al/^{10}Be$ ratio diverges enough from the production ratio that such changes are detectable.

Table S1. Sample location and age data, New England summits

Sample	Site Location and Description	Elevation (m)	Latitude	Longitude	Thickness (cm)	^{26}Al Age (yr)*	^{10}Be Age (yr)*	Uncertainty-weighted average exposure age (yr)	^{26}Al/^{10}Be Ratio[@]	^{14}C Age (yr)[#]
PTD94-19	Franconia Ridge: ~200 m north of Little Haystack summit, glacially molded bedrock	1575	44.14078	-71.64402	3.0	59860 ± 3210	59390 ± 3310	59630 ± 2300	6.74 ± 0.23	
PTD94-20	Mount Washington: "Goofer Point," ~100 m SW of summit, glacially molded bedrock	1896	44.26982	-71.30483	3.0	18010 ± 1600	18570 ± 1010	18410 ± 850	6.54 ± 0.51	
PTD94-21	Mount Washington: "Goofer Point," ~100 m SW of summit, glacially molded bedrock	1896	44.27004	-71.30483	3.0	18200 ± 1030	17650 ± 950	17900 ± 700	6.96 ± 0.26	
PTMW-01	Mount Washington: "Goofer Point," ~100 m SW of summit, glacially molded bedrock	1896	44.27049	-71.30483	1.0	25370 ± 1730	27790 ± 1500	26750 ± 1130	6.15 ± 0.33	
PTMW-02	Mount Washington: "Goofer Point," ~100 m SW of summit, glacially molded bedrock	1896	44.27049	-71.30483	3.0	73910 ± 4140	69350 ± 3650	71340 ± 2740	7.09 ± 0.22	
PTMW-03	Mount Washington: summit, ~20 m N of Tip Top House, frost-riven block, perched atop larger frost-riven block	1895	44.27049	-71.30483	2.0	149200 ± 8200	156100 ± 8330	152600 ± 5840	6.26 ± 0.16	12710 ± 2770
PTMW-04	Mount Washington: Tuckerman Ravine, landslide block that was reoriented by a late-glacial rock glacier	1326	44.26149	-71.29511	1.0	12590 ± 1140	12590 ± 700	12590 ± 600	6.76 ± 0.55	
PTK-06	Baxter Peak, Katahdin Summit, glacially molded bedrock	1606	45.90422	-68.92161	4.0	9280 ± 600	9860 ± 540	9600 ± 400	6.36 ± 0.32	
PTK-07	Baxter Peak, Katahdin Summit, angular block perched atop bedrock with sample PTK-06	1607	45.90471	-68.92191	4.0	37260 ± 2070	34310 ± 1770	35560 ± 1350	7.29 ± 0.23	11040 ± 2190

*Ages calculated from Lal/Stone scaling scheme using CRONUS (Balco et al., 2008) assuming no geomagnetic correction and assuming northeastern North American production rate (Balco et al., 2009). Uncertainty is external error from CRONUS (Balco et al., 2008). CRONUS considers different standards used to normalize isotope ratio measurements. Topographic sheilding was negligable for all samples.

[#]Assuming production rate of 12.7 ± 1.1 atoms/(g*yr) and Lal/Stone scaling scheme

[@]^{26}Al/^{10}Be ratio calculated by CRONUS and normalized to accepted value of AMS standards per Nishiizumi et al. (2007)

Coordinates in WGS84

Table S2. Isotopic Measurements, New England Summits

Sample Name	Quartz Mass (g)	Be Carrier (g)*	Al Carrier (g)*	Measured Total Al (ug)**	Measured $^{10}Be/^{9}Be$ Ratio***	$^{10}Be/^{9}Be$ Ratio Unc.	Measured $^{26}Al/^{27}Al$ Ratio***	$^{26}Al/^{27}Al$ Ratio Unc.	^{10}Be Conc. (atoms g^{-1}) ****	^{10}Be Unc. (atoms g^{-1})	^{26}Al Conc. (atoms g^{-1}) ****	^{26}Al Unc. (atoms g^{-1})
PTD94-19	39.230	0.252	0.000	7579	2.303E-12	6.049E-14	1.321E-12	2.674E-14	9.782E+05	2.599E+04	5.696E+06	1.154E+05
PTD94-20	25.360	0.253	0.000	27732	6.139E-13	1.439E-14	9.134E-14	6.745E-15	3.933E+05	9.717E+03	2.224E+06	1.647E+05
PTD94-21	41.320	0.251	0.000	7724	9.452E-13	2.187E-14	5.396E-13	1.566E-14	3.739E+05	8.929E+03	2.248E+06	6.541E+04
PTMW-01	26.650	0.254	0.000	15620	9.611E-13	2.256E-14	2.431E-13	1.141E-14	5.969E+05	1.444E+04	3.175E+06	1.493E+05
PTMW-02	30.890	0.355	0.000	29769	1.905E-12	3.652E-14	4.132E-13	1.019E-14	1.450E+06	2.808E+04	8.884E+06	2.193E+05
PTMW-03	39.409	0.253	0.576	3229	7.524E-12	1.372E-13	9.528E-12	1.661E-13	3.218E+06	5.888E+04	1.742E+07	3.039E+05
PTMW-04	40.228	0.355	0.000	20341	3.168E-13	7.961E-15	9.184E-14	6.976E-15	1.767E+05	4.796E+03	1.033E+06	7.878E+04
PTK-06	39.915	0.254	0.000	7106	4.330E-13	1.017E-14	2.416E-13	1.034E-14	1.739E+05	4.438E+03	9.565E+05	4.118E+04
PTK-07	40.730	0.254	0.000	7060	1.364E-12	2.317E-14	9.809E-13	2.541E-14	5.584E+05	9.706E+03	3.792E+06	9.835E+04

*Be and Al carriers added to samples both had a concentration of 1000 ppm.

**Refers to the total Al in the sample (including both native Al in quartz and Al added via carrier, if applicable) quantified in duplicate by ICP-OES directly following digestion.
***During AMS analysis, all Be samples were normalized to standard LLNL3000 (except sample PTK-07, which was normalized to LLNL 1000) and all Al samples were normalized to standard KNSTD9919.

****Concentration considering accepted value of standards at time of measurement, used for CRONUS

Table S3: In situ ^{14}C sample analytical data

Sample Name	**Lab Number**	**AMS Number**	**Mass Quartz** *(g)*	V_{CO2} *(mL)*	V_{dil} *(mL)*	F_M	$[^{14}C]$ 10^5 *at* g^{-1}
PTDK-7	RN-785	AA-54556	4.9975	0.0137 ± 0.0011	2.1115 ± 0.0203	0.0271 ± 0.0006	3.3534 ± 0.1130
PTMW-3	RN-786	AA-54557	5.0069	0.0443 ± 0.001	1.3774 ± 0.0131	0.0527 ± 0.0007	4.3015 ± 0.1036

Notes: $\delta^{13}C$ of both diluted samples assumed to be -35.0 ± 2.0 ‰ (typical value for diluted samples). Uncertainty in quartz mass: ±0.0002 g. Fraction modern (F_M) values corrected per Hippe and Lifton (2014). Concentration calculated after subtracting long-term extraction system process blank of (1.2367 ± 0.3531) x 10^5 ^{14}C at.

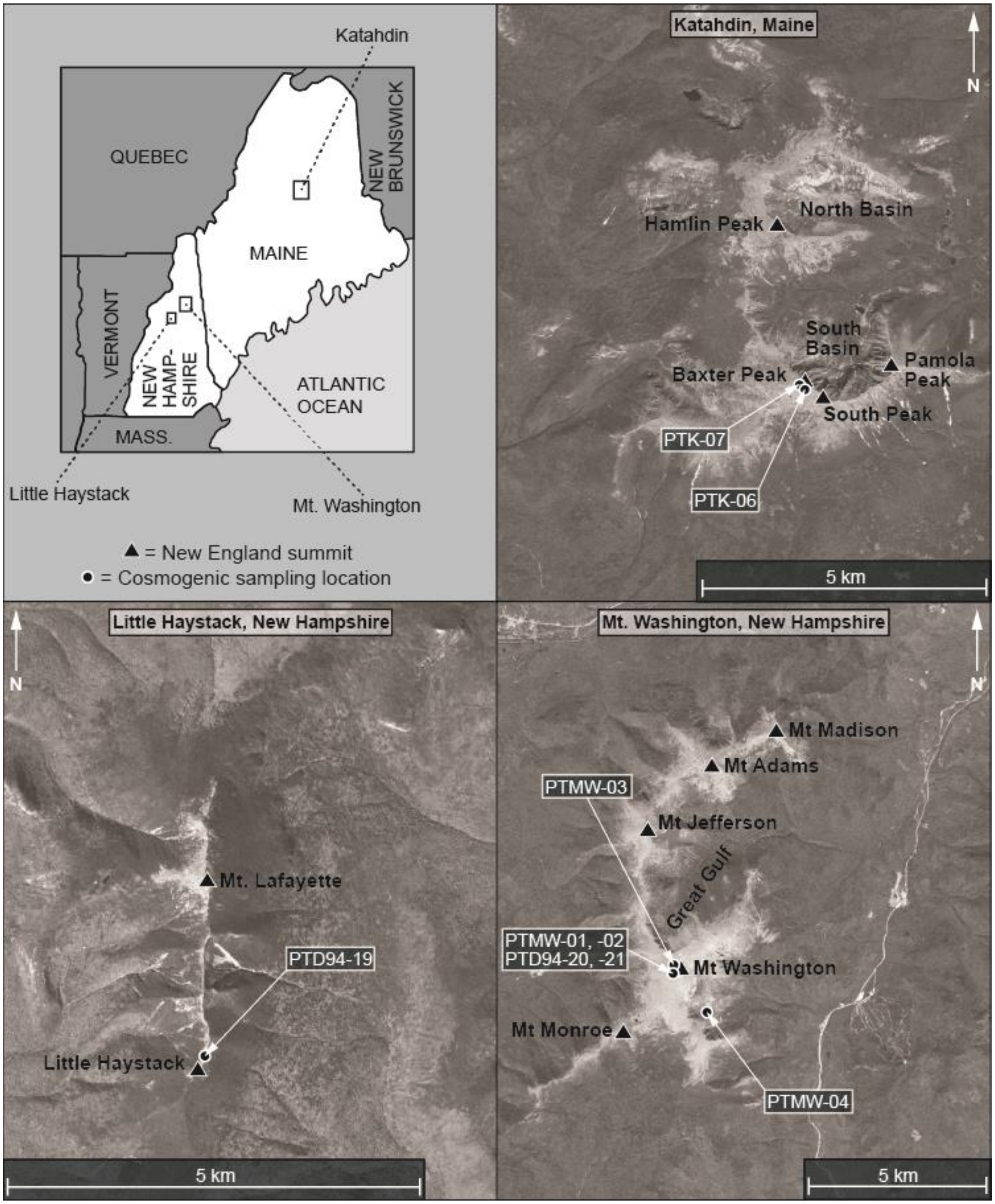
Table S4. Selected radiocarbon ages older than Last Glacial Maximum from New England

Site Name	Latitude (°N)	Longitude (°W)	^{14}C Age (yr BP)	Lab Number	Material	Calibrated Age (ka BP)[a]	Original Reference
Gould Pond	44 59 33	69 19 09	25280±1010	SI-5372	Marine Shells	29040 (27106-31083)	Anderson et al., 1992
Isie Lake	47 04 15	68 39 23	24300±110	OS-6435	paleosol	28340 (28020-28652)	Dorion, 1997
Jo Mary Pond	45 34 38	68 02 19	24500±130	OS-3170	paleosol	28550 (28208-28829)	Dorion, 1997
Upper South Branch Pond	46 05 00	68 54 00	29200±550	SI-4519	wood	33240 (31763-34275)	Anderson et al., 1986

[a]Age estimates include the median intercept and the minimum and maximum ages in parentheses based on 2 standard deviations from minimum and maximum intercepts using CALIB 7.0 (Reimer et al., 2014) and considering combined IntCal04/Marine04

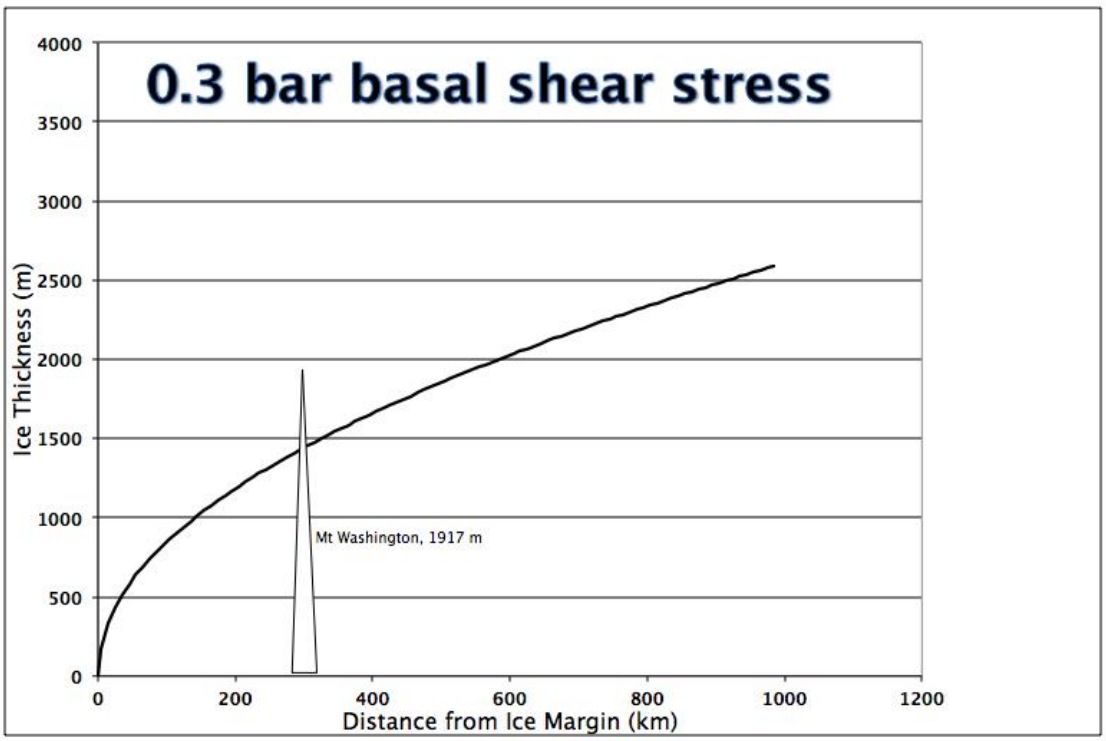
note, original information compiled in Dorion, 1997

7. Maps of sampling sites

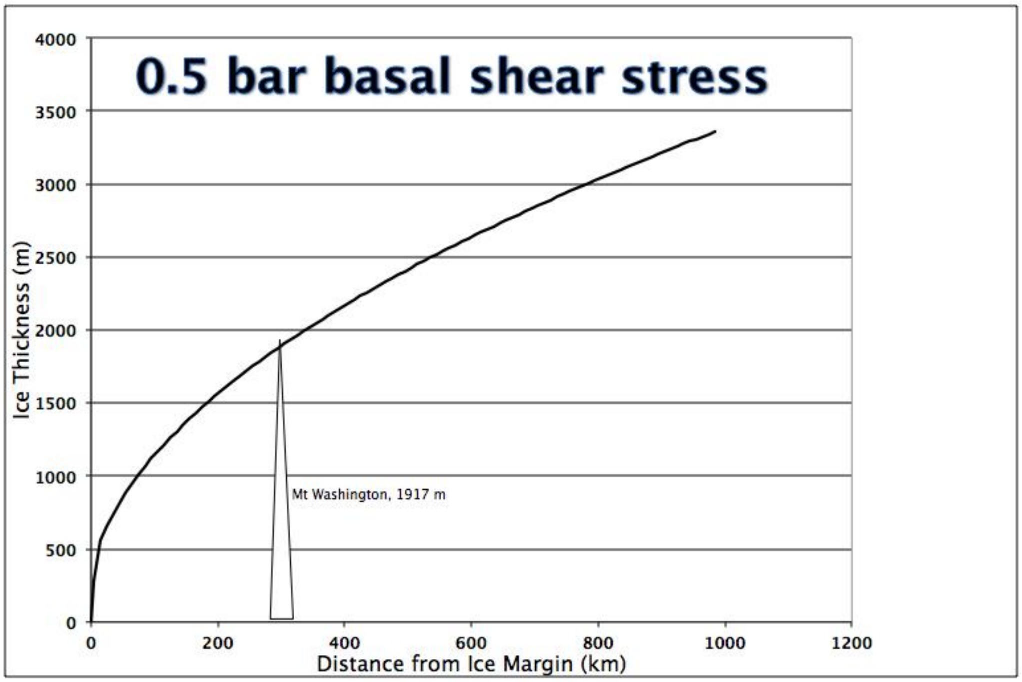


8. Modeled ice profiles and explanation/approach

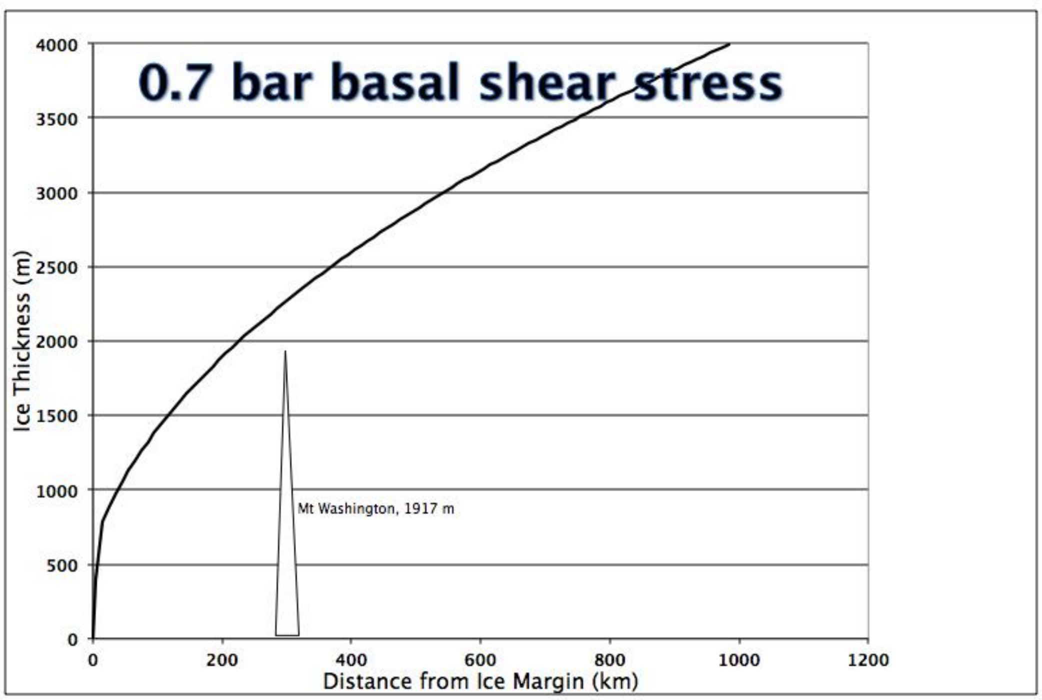
We used a simple spreadsheet model for ice profiles based on the model of Nye (1952) following the approach of Davis (1989). We presume that the ice margin extended to near Martha's Vineyard at the Last Glacial Maximum (LGM). To get Mt. Washington exposed at the LGM requires a basal shear stress < 0.5 bar – unlikely on the bare rock crystalline terrain in the uplands of New England. Basal shear stresses > 0.5 bar bury the summit in ice. Having thin ice over the peaks is likely important not only to keep the ice cold but to prevent pressure melting and glacial erosion. We conclude that basal shear stress in the rough, mountainous terrain of central New England was at least 0.5 bars.



Ice sheet profile model based on the equation of Nye (1952) for basal shear stress = 0.3 bar at glacial maximum. Summit is exposed. This is not consistent with isotopic data.



Ice sheet profile model based on the equation of Nye (1952) for basal shear stress = 0.5 bar at glacial maximum. Summit is just buried by ice. This is most consistent with isotopic data indicating cold-based ice at the summit and erosive, warm-based ice just below.



Ice sheet profile model based on the equation of Nye (1952) for basal shear stress = 0.7 bar at glacial maximum. Summit is deeply buried by ice and thus likely warm based and not consistent with isotopic data.

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Ref.: Ms. No. G37225
Cold-based, Laurentide ice covered New England's highest summits during the Last Glacial Maximum
Geology

Dear Dr. Bierman ,

Three reviewers have now commented on your paper " Cold-based, Laurentide ice covered New England's highest summits during the Last Glacial Maximum " submitted to Geology. You will see that they are advising revisions to your manuscript, with two reviewers suggesting minor revisions (Accept), one major revisions. After reading the manuscript and reviews carefully, I decided that moderate revision will probably work. Note that the more critical reviewer is not based in the US, and can be seen as representating a broad audience that is not very familiar with the US settings. Specifically, for instance. the complaint about the map (and where the CT river goes) appears not unreasonable.

For your guidance, reviewers' comments are appended below. Reviewer #3 provided the review in an attachment, which you can find by logging into the online system (unfortunately, this system doesn't allow attachments to be sent with emails).

When you submit the revised manuscript, please include in your cover letter a detailed, point-by-point list of changes made in response to the reviewer comments (including these in the attachment), or a rebuttal against each point that was raised by the reviewers.

In order to expedite publication of your paper, you must use the formatted and reference-checked manuscript file provided by the GSA Editorial office when revising your Geology paper. Failure to use that formatted file will result in your revision being returned to you.

Your revision is due by Oct 04 2015 11:59PM .

To access your formatted and sized manuscript, and revision instructions, please log in to Editorial Manager (http://www.editorialmanager.com/geology).

You will see the menu item "Submissions Needing Revision." Click this link. On the next page, hover your mouse over the "Action Links" and click "View Attachments" to access the MSWord manuscript and marked-up figures.

Yours sincerely,

Dr. Ellen Thomas

Yale University
Geology

Reviewers' comments:
Reviewer #1: Bierman et al., cold ice over New England summits… (G36839)
This is an exciting report, based on a small number of samples, but the multiisotope data provide new inferences on the nature of upland erosion by the Laurentide Ice Sheet, a subject of broad interest to glaciologists, paleoclimate workers and geomorphologists concerned with long-term erosion. The authors also offer a novel mechanism for producing or maintaining relief in an environment shaped by ice erosion.
My review of this resubmission suggests two subject areas that the authors should continue to think about; they cannot be treated in the space available, but there might be room for a phrase or two to extend the discussion of these topics:

1. When did summit areas emerge and how rapid was thinning of the ice?
2. Paleoclimate? Is the pattern of burial and summit (re)emergence consistent with our broad understanding of regional Paleoclimate, or are the authors helping point in a new direction?

Comments/suggestions keyed to line numbers in the manuscript
22. northern New England
30. "unable" "that did not erode"
73. You might use a phrase or short sentence to remind reader that the timing of ice advance (and duration) is not well known, since your data speak to that point.
123. Does bedrock type remain more or less constant? Can you be sure that frost-cracking is reasonably similar in both environments?
128. "to have varied?'
132. do such erosional features/deposits have to form beneath warm-based ice?
134. molded,
157. Discussion of this evidence is much improved over the previous draft!
164-167. Either? You make several inferential points in this complex sentence. Suggest you break it up and begin by reminding reader of your 29 kyr requirement.
170. And burial, when it occurred, cannot have been accompanied by uniform erosion.
171. Implies?
173. What does your Chesapeake bulge say about this paradigm of LIS growth?
175. Be cautious with your wording here. You can't have your summit ice flowing too much!
182. This statement seems safe, but not well constrained by the data. Near Mt. Washington, better dating of the Bethlehem /Jefferson moraine complex and associated ponded lakes will constrain the main ice mass, though perhaps the

summits are still buried by unmoving ice?
186. And/or variable erosion.
184. This is good; you could even use a sentence to anticipate what mixture of dating and modeling comes next as you work to constrain what happened to the upper surface of this poorly constrained ice sheet
190. "weakly erosive" seems like a fine and safe way to characterize the state of erosion throughout.

Figs. 1 and 2—Very nice portrayal
Supplemental information—Required reading for those who follow the text closely. I wish that there were room for more of these data in the main part of the paper.

Reviewer #2: This paper is excellent and applies very recent methodology to an old problem to produce a solid conclusion, putting to rest a long standing debate about the history of glaciation, the role of glacial erosion, and ice sheet basal conditions in the high mountains of New England. I was very positive about this paper in my first review and it has improved.

There are several minor editorial fixes with references that can be made:

1. in the references - I could not find a citation for Bierman and Caffee, 2002 and it should be listed after Bent (1942) in the reference list.

2. I did not find a citation in the text for Hallet and Putkonen (1994).

3. I did not find a citation in the text for Davis et al., 2006.

4. In section 1 of the supplemental information (Laboratory and data reduction methods) - in paragraph 3 citations for Lifton et al. 2001 should be Lifton and Quade, 2001.

Reviewer #3: see attachment

Reviewers' answers to Review Questions:
Reviewer's Responses to Questions

Is the manuscript appropriate for this journal and its audience?

Reviewer #1:
*Presents a significant advancement in the field of geoscience

Reviewer #2:

*Presents a significant advancement in the field of geoscience

Reviewer #3:
*Builds upon current knowledge in a significant way

Are the objectives and rationales of the study presented clearly?

Reviewer #1:
*Rationale and objectives are clear

Reviewer #2:
*Rationale and objectives are clear

Reviewer #3:
*Rationale is unclear

Are the methods and data adequate to support the hypothesis?

Reviewer #1:
*Methods and data support the objective and hypothesis

Reviewer #2:
*Methods and data support the objective and hypothesis

Reviewer #3:
*Missing essential references

Are the conclusions clear and supported by the data?

Reviewer #1:
*The summary of results is concise and accurate

Reviewer #2:
*The summary of results is concise and accurate

Reviewer #3:
*Vague, overstated, or understated applicability

Are figures and tables pertinent and legible?

Reviewer #1:
*Figures and tables support the data and conclusions and are legible

Reviewer #2:
*Figures and tables support the data and conclusions and are legible

Reviewer #3:
*Figures and tables support the data and conclusions and are legible

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Is the supplemental information used appropriately?

Reviewer #1:
*Supplemental material is supplemental and supports the objectives of the paper

Reviewer #2:
*Supplemental material is supplemental and supports the objectives of the paper

Reviewer #3:
*Not applicable

As far as you know, has any part of the manuscript been published previously?

Reviewer #1: No

Reviewer #2: NO

Reviewer #3: no

If you wish to be identified as a reviewer, please type your name in the box below. If not, please type “Anonymous”.

Reviewer #1: Dethier

Reviewer #2: Jack Ridge

Review of manuscript by Bierman, Davis, Corbett, Lifton and Finkel entitled "Cold-based, Laurentide ice covered New England's highest summits during the Last Glacial Maximum" for consideration in *Geology*.

The authors present a straightforward article based on an interesting dataset from the highest mountains in New England, where the occurrences of three cosmogenic isotopes in quartz drawn from bedrock and blocks detail the history of multiple ice shielding and exposure events, as well as the need for subglacial preservation and, hence, cold-based conditions. This has been shown for a number of other locations and settings in North America and Europe, including the use of the three isotopes used in this study, but it is a neat dataset nonetheless.

There is a notable lack of attention to detail, as exemplified by the many comments that arise when reading this manuscript (see below), mistakes in the reference list, mistakes in sample labelling, and convey an impression of untidiness.

I hope the authors will forgive my impertinence to having viewpoints on the use of the language in the manuscript as they are native speakers and I am not.

Abstract:
Lines 31-33: "The contrast in erosion rates between stable summits and deeply eroded valleys may contribute to the development and maintenance of northern Appalachian topography" Why the use of "may", is there a choice at all here- if overriding ice was cold-based over the summits (previous sentence) and not so in the surrounding landscape, relief enhancement must occur?

Introduction:
Lines 39-40: "More than a century of study (Thompson et al., 1999) has answered some questions about glaciation of this landscape, but mysteries remain." I would have hoped that a century of study would have answered "<u>many</u>" questions, but that <u>some</u> remain? I don't think mysteries should be part of a science paper.

Line 43: "Poorly developed soils and thin weathering rinds…" requires a reference.

Line 43 and elsewhere (ten occurrences, including the abstract): replace "suggest" by "indicate" and reserve "suggest" for such use in conjunction with persons, i.e. Davis (1989) suggests…

Line 44: "…covered all of New England..." requires a reference

Lines 44-45: "…but there are few quantitative estimates for the age and duration of this ice cover over the peaks." requires a reference

Lines 45-46: "Moreover, it is not clear whether the ice was warm-based and erosive or frozen to the bed such that it preserved relict landscapes from prior interglacials." This is an interesting statement – especially against the backdrop of the authors sampling "glacially-molded bedrock" which would seemingly indicate warm-based ice, but whose isotope inventory yield that erosion was at least insufficient to reset this inventory during the LGM. Is this a relict landscape? The authors refer to relict landscapes in conjunction to "from prior interglacials" and so consider the relict landscapes to be of non-glacial origin. The science of geomorphology has been well ahead of cosmo studies in pin-pointing that certain landscape elements in mountains are of non-glacial origin and should have survived subglacially through cold-based ice. Cosmo studies have since verified and strengthened those inferences. I don't know the literature of the New England summits studied here, but I presume that "More than a century of study (Thompson et al., 1999)…" would have led to someone concluding that summit blockfields may be a relict landscape unit? Hence, I have a feeling that "it is not clear…" is a rather sweeping statement which could have some qualification?

Lines 46-48: "Here, we report measurements of *in situ* produced cosmogenic ^{10}Be, ^{26}Al, and ^{14}C in samples collected from the uplands of Katahdin and Mount Washington." What happened to "Little Haystack", or is this considered to be part of Mount Washington? This is not clear from the rest of the article. Also there is the use of "Mount" and "Mt." and I would urge to be consistent in this usage. Finally, it would be good if the authors introduce where Katahdin and Mount Washington are located before we learn that they report measurements from there. At the very least a figure reference to the locations of the mountains would be required here.

Background:
I find the referencing in this manuscript to be incomplete and too heavily slanted to North American literature. Certainly, some of the early cosmogenic isotope studies to show that glacial landscapes have experienced multiple periods of burial and exposure are from Scandinavia, and they do so for summit regions as discussed in this paper, as well as for lowland tors. Certainly some of the papers by Derek Fabel and colleagues need to be quoted here.

Lines 59-61: "In glacial landscapes dominated by cold-based ice, the ratio of ^{10}Be and ^{26}Al can be used to detect exposure followed by burial only if that burial lasts >100 ky…". Fabel and Harbor (1999; *Annals of Glaciology*) show the requirements for complex exposure dating neatly- they also discuss the complication of the post-glacial exposure duration for the question of complex exposure dating. This is not mentioned here; do the authors consider this as unimportant?

Line 62: missing information about the length of the "shorter burial periods", it merely states "(≥ ky)".

Lines 64-72: in a presentation of the deglaciation history of New England, it would make much more sense to start with the LGM and work your way to younger ages (and towards the mountains, the subject of study). Hence, I suggest recasting the paragraph.

Lines 64-65 and Figure 1: "The deglacial history of New England's lowlands is well constrained by the Connecticut River valley varve record (Figure 1; Ridge et al., 2012)…". It is unclear from figure 1 where the Connecticut River valley (should it be "Valley" as per figure 1 use?) starts or ends. Assuming it flows southwards, it seems to dry-up at the "MASS." border! Please provide us with a map that works regarding the location of the CRV. New England is comprised of six states, you only show the location of three of them, and a little bit of "MASS.", and the topographic information of only two states. Would it be more correct to call this "Northeastern New England" throughout the paper? Add a "North arrow" to figure 1, and two sets of coordinates would further improve its usage.

Lines 69-72: It would seem that there is a need for a proper map over New England, the latest reconstruction of the deglaciation chronology across New England, and indicated the locations of Martha's Vineyard, Cape Cod, and Connecticut. This map could also show the Connecticut River Valley in its full extent.

Lines 76-79: "Ice-sheet profiles (e.g., Davis, 1989), modeled using basal shear stress values appropriate for rugged, crystalline terrain (0.6 to 1.0 bar), suggest that when the LIS was fully advanced, the high peaks of Katahdin and Mt. Washington would have been under ice (Supplemental Information)." It would be good if the authors could make some mention of the geology of Northeastern New England if it is simple, or a map if it is complex, and indicate the location of the modelled transect on the deglaciation map that I suggested in the former comment. The use of the profiles in the supplemental information seems to be to say that with reasonable values for the basal shear stress, the summit of Mount Washington could have been covered by ice. Given the simplistic approach, a lack of discussion whether this approach is reasonable (for example, the effect of ice streams or outlet glaciers in this region on this approach), the mere fact of erratics on the summit of the mountain is far better evidence for this mountain having been covered by continental ice. The authors have the opportunity to either much improve on their description of this modeling exercise, or remove it from the manuscript.

Lines 79-81: "During advance, the peaks would have protruded from the ice as nunataks; similarly, during retreat, the summits would have been exposed while ice continued to flow through the adjacent lowlands." It seems clear to me that the deglaciation of the summits should then precede the deglaciation of the lowlands (what they call the regional deglaciation). The authors could merit more in-depth discussion (later-on) to the fact that the available radiocarbon dates seem to suggest that the peaks were deglaciated later (or simultaneously- within 2 sigma uncertainty, line 144) than the lowlands – especially given this statement. The list of possibilities in lines 155-157 (These young ages most likely reflect stripping of till cover (Gosse and Phillips, 2001), shielding by snow or ice (Anderson et al., 2008), and/or erosion of sampled surfaces after exposure, although deep, post-glacial erosion of these hard rocks seems unlikely) is enticing and in demand of further qualification. Why would there not have been a final cold-based ice carapace across the mountains that shielded from cosmic rays while the surrounding lowlands deglaciated? The authors infer burial of the sample sites and cirque deepening prior to the LGM by local ice – so why not an ice configuration similar to that following the LGM but perhaps cold-based?

Methods:
Line 85: refer to Figure 1 for the location of the mountains.

Lines 89-93: "Exposure ages (^{10}Be and ^{26}Al) were calculated using the CRONUS calculator (wrapper script: 2.2, main calculator: 2.1, constants: 2.2.1, muons: 1.1, Balco et al., 2008) and Lal (1991)/Stone (2000) time invariant scaling of the northeastern North America production rate (Balco et al., 2009). In situ ^{14}C exposure ages were calculated using a modified version of the CRONUS calculator." There is a clear asymmetric treatment of ^{10}Be and ^{26}Al on the one side, and ^{14}C on the other. Given this information, it would be entirely possible to recalculate the former given the information above, but impossible to recalculate the latter. Can the authors please refer to a site where the "modified version of the CRONUS calculator" can be accessed?

Data:
Line 96: Would it not be more transparent to talk about "apparent" exposure ages at this point?

Line 97: 156 should be 156.1

Line 97: Figure 3 doesn't show the range of ages because it shows averages. Remove mention of Figure 3 here.

Line 98: why underline "uncertainty-weighted average"?

Line 99: Add reference to Figure 3 here.

Line 100: "Exposure ages (^{10}Be and ^{26}Al) for most samples pre-date the LGM." Isn't this a misleading statement because only 4 out of 9 exposure ages conform to this statement?

Lines 101-102: would it not be better to use "older" instead of "greater"?

Line 104: 153 should be 152.6

Lines 104-105: I am not fond of "X", so rather than 10X and 2X I would write "ten times" and "twice".

Line 106: remove comma after "regional".

Line 111: 153 should be 152.6

Discussion:

Lines 117-118: "Similar to those working in other glaciated terrains…" The referencing used here would be more consistent if the qualification of "…of Northeastern North America" was added to the statement (I know there is a Greenland reference in there). The statement is true if references from other regions were added to the list, which I would urge the authors to do. In this regional list given at the present, Marquette is strangely absent.

Line 122: "…we found no striae…" ought to have been presented in the earlier part of the paper when these uplands are discussed.

Lines 122-123: "On and near summit areas, we found no striae, in contrast to those seen on lower bedrock summits in New England." This statement needs a reference.

Lines 128-129: "The effectiveness of glacial erosion appears to vary over time and space, most likely due to ice near the pressure melting point (e.g., Briner et al., 2014)." This reference should be augmented with some original papers by, for example, David Sugden and Johan Kleman. I would suggest Sugden (1974; *Institute of British Geographers Special Publication*) and Kleman and Stroeven (1997; *Geomorphology*).

Line 132: 153 should be 152.6

Lines 133-135: "In addition, some sample sites with pre-LGM ages appear glacially molded suggesting the presence of warm-based ice at some time in the past, prior to the LGM." I'm not sure why the authors write "some sample sites" because all their bedrock sites conform to this statement? Also, why is it necessary that the molding happened solely during glaciations prior to the LGM? I would invite an analysis of how much erosion could have been accommodated during the LGM in the 0-3 m range. Limited but visible erosion could have occurred and have resulted in removal of ^{14}C (burial time) but inheritance of ^{10}Be and ^{26}Al isotopes.

Line 139: 153 should be 152.6

Lines 149-151: "…a rock glacier block >500 m below the summit, has an average ^{10}Be and ^{26}Al age (12.6±0.6 ka) only slightly less than the regional deglaciation age (14 ka) suggesting that the rock glacier stabilized rapidly as climate warmed". While this appears correct, it is extremely awkward to attach much significance to the apparent exposure age of one sample. I suggest modifying the language such that reasonable caution is expressed.

Line 159: "…(average 6.68±0.39; Table S1) is…". Please note if the average is for all nine samples, and please add this value to Table S1 which is referenced.

Lines 158-161: "Multiple nuclide data can better constrain the timing of summit burial and exposure. The $^{26}Al/^{10}Be$ ratio in New England summit samples (average 6.68±0.39; Table S1) is indistinguishable from the production ratio of these nuclides (6.75; Balco et al., 2008), precluding long burial times (100s of ky) after initial exposure." What is the effect of the duration of postglacial exposure on this statement (cf. Fabel and Harbor, 1999)?

Line 165: remove "either".

Line 165: 102 ka should be "103 ka" (73.910 + 29 ka).

Lines 164-167: this sentence suffers from poor grammar. Please rephrase.

Line 168: 153 should be 152.6

Line 170: the value of 200 ka is highly uncertain. The ^{14}C data does not demand there to have been any burial during MIS6. Hence, I cannot see that the minimum requirement would be more than 185 ka (156.100 + 29 ka)?

Lines 172-174: There seems to have been at most 4000 years for the Laurentide ice sheet to have expanded across New England to its maximum position. For the reader it remains entirely unclear where the ice margin resided prior to the advance at 31 ka. Given the modeled ice profiles in the Supplemental information, the distance to be covered would have been at least 300 km, which requires an ice expansion of at least 75 m/year. Given this quick expansion of ice, also across uplands, it calls into question whether the profile for 27 ka as modeled would be realistically described by the equation of Nye (1952). I would invite the authors to illuminate this question, and provide the reader with better information of where the ice margin was positioned prior to the expansion to the LGM, and how the ice sheet surface profile might have differed from the one described using the Nye (1952) equation.

Lines 176-179: "…before being overwhelmed by continental ice that likely advanced through Maine (and by inference, northern New Hampshire) ~29 ka, based on calibrated ^{14}C ages on shells, paleosols, and wood found in the basal sections of lake sediment cores (Dorion, 1997; Table S4)." If these are dates of overrun lake sediments, wouldn't you want to know what the youngest ages in the section were to constrain the timing of ice overriding – rather than ages on the oldest "basal" sections?

Implications:

Lines 187-188: These three references from North America don't cover the labelling of "polar regions", "high latitudes" and "thin ice sheets" without the qualification of "in North America"?

References Cited:
Bierman & Caffee 2002 (lines 221-223), Davis et al. 2006 (lines 255-257) and Hallet & Putkonen (lines 277-278) should be removed because they lack referencing in the manuscript.

Figure captions:
Figure 1. Line 321: A more logical order would be "Katahdin, Mt. Washington, and Little Haystack" (North to South as well as East to West). Remove (lines 321-322) "indicated by dotted lines"- this is not qualified for the Connecticut River Valley! Line 323: Add "(meters above sea level)" after "shading".

Figure 2. Line 326: qualify the sample names after "..location of summit samples". Add "the" before "summit". Add "view towards…" after "…summit of Katahdin". Lines 328-329: Add "view towards…" after "…on Mt. Washington". Line 330: Add "view towards…" after "…on Little Haystack". Is the imagery from Google Earth (or some other product) and does this need mentioning?

Figure 3. Line 336: Add reference after "…dotted line".

Figures:
Figure 1. Add north arrow, consider adding two sets of coordinates. Make sure the Connecticut River is continuous to the figure frame.

Figure 2. In panels B, D, and F, please indicate where the samples were taken.

Figure 3. There is no mention of why two samples fall above the exposure curve. In the manuscript, please raise this issue.

Supplemental information:
In this section I only regard mistakes, and take no issue with the science, because of a lack of a numbering system. Change "8. Modeled ice profiles and explanation" to " 8. Modeled ice profiles and explanation/approach". Change PTDK-7 to "PTK-07". Add period after first sentence of "2. Additional calculations…". Add period after "Bierman et al".

Table S1: Add sample lithologies, Add boulder dimensions. Are uncertainties at 1 sigma- mention.

Table S2: specify the years after "Concentration considering…at the time of measurements".

Table S3: Change PTDK-7 to "PTK-07" and PTMW-3 to "PTMW-03".

Table S4. Please synchronise the lat-long presentations between tables S1 and S4. Are paleosol ages "bulk" ages?

7. maps of sampling sites: Please provide a figure caption. Indicate what sort of imagery has been used. Please indicate coordinates.

8. Modeled… the text is insufficiently developed (see above). Please indicate also the equation of Nye (1952) that is used, and discuss the break in slope that is visible in all three plots. Please modify Mt Washington to "Mt. Washington" in all three plots.

9. references… Change "Material" to "Information".

Used references:

Fabel, D. & Harbor, J. The use of in-situ produced cosmogenic radionuclides in glaciology and glacial geomorphology. *Annals of Glaciology* **28**, 103-110 (1999).

Kleman, J. & Stroeven, A. P. Preglacial surface remnants and Quaternary glacial regimes in northwestern Sweden. *Geomorphology* **19**, 35-54 (1997).

Sugden, D. E. Landscapes of glacial erosion in Greenland and their relationship to ice, topographic and bedrock conditions. *Institute of British Geographers Special Publication* **7**, 177-195 (1974).

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September 15, 2015

Dr. Paul R. Bierman
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