Response to review 1 (Jason Briner) of 'Reversible ice sheet thinning in the Amundsen Sea embayment during the late Holocene', by Balco, Brown, Nichols, Venturelli et al.

Nov 21, 2022

First, we would like to thank Jason Briner for a supportive and helpful review. We will respond to all the review comments (reproduced in *italic*) in order, and then at the end we will provide a summary list of all the proposed revisions. In general, this review was quite supportive of the paper, and many of the review comments did not represent issues requiring correction, but instead contained questions or comments suggested by aspects of the paper. Thus, we have thoroughly discussed all aspects of the review in the responses, but several of the review points did not lead to any proposed revisions.

1. *The glacier thickness history most compatible with the data, according to the interpretations laid out in the paper, is one that I would characterize as actually rather stable. Following a period of rapid ice thinning between ~6000 and 8000 years ago of >200 m in <1000 years is a subsequent period 1000s of years in duration with a thinning of only "30-35 m relative to present." One could interpret this amount of glacier thickness change across many millennia as minor, and maybe not a reason to question the irreversible retreat concept, as the title does. There is mention in the conclusion paragraph that 30-35 m of thinning "may have been associated with grounding line retreat of tens of kilometers upstream of present locations." With the emphasis of the paper being on the reversible nature of marine glaciers in West Antarctica, I might suggest the addition of evidence/discussion backing up the inference of 10s of km of grounding line retreat associated with 35 m of thinning.*

Our response to this has two important elements. First, whether something is "stable" or not is relative and depends on not only the frame of reference, but also in many cases the rhetorical point that a paper is trying to make. For this reason, we have avoided the use of "stable" and instead characterize amounts of thinning or retreat with numerical values. Rereading the paper reveals that we did not completely succeed in this goal, having failed to remove one instance of the word 'stable' on page 6 (see additional discussion of this below). We used "unstable" once in a mathematical/physical context ("stable" and "unstable" have clearly defined meanings in mathematics) to introduce the marine ice sheet instability feedback, not to describe ice sheet behaviour. Thus, our aim in this paper is explicitly not to characterize ice sheet behaviour as "stable" or "unstable," but instead to place quantitative constraints on past ice thickness. At no time have we stated whether our reconstruction of Holocene ice sheet thinning and thickening is "stable" or "unstable."

Second, although it is true that 35 m of thinning is small compared to hundreds of meters of LGM-to-present thinning, the point of our argument is that it was followed by thickening, not by more thinning. A central reason for concern about the irreversibility of present thinning is that, due to reversely-sloping subglacial topography and the marine ice sheet instability, grounding line positions inboard of present may not be stable (in the mathematical sense), so grounding line retreat inboard of present may result in irreversible retreat into the center of the ice sheet. The importance of our results is that if past thinning below present was also associated with past grounding line retreat inboard of present, as we infer from coeval thinning and retreat in recent decades, then grounding line positions inboard of present did not lead to irreversible retreat.

Although we discussed these points in Section 1 (p. 2, lines 5-10, p. 5, lines 1-10) and then again very briefly in Section 6 (p. 20, lines 5-10) we did not lay out this entire chain of reasoning in one place. To some extent, we did not highlight this reasoning because the aim of this paper is to present observational data, and not to speculate about the importance of the observations. However, we can clarify this reasoning by adding discussion to these sections of the paper to more clearly lay out the reasoning that

(i) grounding line positions inboard of present are potentially of concern, and (ii) the evidence for mid-Holocene thinning indicates that the mid-Holocene grounding line position was likely inboard of present (**Revision 1**).

- *I think that an underlying requirement for applying luminescence exposure dating in this situation is that a sub-glacial site of interest experiences(ed) non-erosive glaciation. If the base of the glacier is erosive, and the drill sites had been exposed in the middle Holocene, the glacier could have eroded the upper few of cm of the bed during late Holocene overriding. In this case, wouldn't the luminescence results would be the same as if the drill sites were not exposed at all during the Holocene (the current interpretation)?*

This review comment, as well as the subsequent ones, highlight an omission in the paper, for which we apologize. In fact, ice at the drill site could not have been above freezing during a late Holocene thickening event with a magnitude of tens of meters, so erosion could not have occurred. Mean annual temperatures at the drill site are well below freezing throughout the year, and hundreds of meters of thickening would be required to bring the ice-rock interface above freezing.

There is evidence from similar sites around Antarctica that hundreds of meters of LGM thickening did result in ice above the freezing point at low elevations that are near the present ice margin. Such observations are described in, among others, Stone et al. (2003, Science) and Balco et al. (2014, QSR). At these sites, geomorphic evidence of erosion by wet-based ice (polish, striations) is observed only at the lowest currently exposed sites that are hundreds of meters below the LGM ice surface. Cosmogenic-nuclide data show concordant bedrock-erratic ages only at these lowest sites. At all higher elevations, cosmogenic-nuclide concentrations are higher in bedrock, and erratics with ages predating Holocene deglaciation are present, indicating cosmogenic-nuclide inheritance and therefore frozen-based conditions at the LGM. It is likely that the ice margin at Kay Peak Ridge is near a similar transition: Be-10 data on boulders and bedrock above the present ice margin display inheritance and therefore a lack of LGM erosion, whereas much lower Be-10 concentrations attributable to only Holocene exposure in core tops indicate likely LGM erosion at the core sites. Regardless, Be-10 inheritance near the present ice margin indicates that erosion was not widespread here even at LGM and early Holocene conditions with hundreds of meters thicker ice. Thus, tens of meters of late Holocene ice cover could not result in erosion at the core sites. Because erosion and sediment transport at the bed could have occurred during LGM conditions, we attribute the debris-rich basal ice to the LGM.

The point here is that these observations are, in fact, critical to understanding why the core tops could not have been ice-free during the Holocene, but we omitted to discuss them. Presumably, this is because we mostly work in Antarctica and it seemed obvious to us. However, we can clarify this in a revised manuscript by adding material at the end of section 2. After we have clarified that the ice-rock interface is, in fact, frozen, and described the debris-rich ice in contact with the bedrock, we can discuss the constraints on basal temperature and our reasoning as to why the debris-rich ice can only have formed during LGM conditions. (**Revision 2**)

- *The authors might therefore mention that the drill sites are cold-bedded (I didn't find anywhere in the paper that mentions this, but I might have missed it). I believe Winke operations require a cold bed at the time of drilling? (not sure about the packer set-up, if that could still obtain bedrock cores with minor drill fluid loss at a warm ice-bed interface) Knowing the basal temperature would be best (but maybe not possible). If the bed is only slightly below pressure melting, then it may be difficult to know if it was always cold-bedded during the Holocene. It might come across as a surprise to some that this is a site of cold-bedded glaciation, given the marine-based, fast-flowing ice-steam nature of these glacial systems; literature suggests* *velocities on order 1 km/yr and beds nearby that are not frozen, although there must be sharp boundaries in a landscape like this.*

See above. In addition, although certainly fast-flowing ice is present in major glaciers near the site, fast-flowing regions of these glaciers are hundreds to > 1000 m thick. The core site is not within a region of fast flow at present (although it may have been closer during LGM conditions).

Also, yes, the drill system requires a frozen bed. We will clarify this (**Revision 2**)

- *The authors could attempt to rule out another possibility for the OSL signal to be saturated at the surface during an ice-free period, which would be that the two drill core sites with the OSL data were covered by debris during a Holocene ice-free period. Such debris could shield underlying bedrock from being bleached. If this is unlikely, say why. Perhaps more information from the ice-free portion of the bedrock ridge would be useful – does it lacks debris, or is debris sparse? Similarly, back to the glacial erosion part, maybe some descriptions from this ridge would be useful here as well. Does it lack glacial molding, other glacial-erosional landforms and striations (particularly below the 1966 limit)?*

Again, we address this in the discussion above. If, as we conclude, the debris-rich basal ice can only have formed under LGM conditions, then the core tops could not have been ice-free during the Holocene.

- *Given the two reasons above that could lead to a saturated OSL signal despite an ice-free middle Holocene, it could be worth mentioning whether the CRN results alone are or are not compatible with an ice-free period during the Holocene. If not, then great, and the interpretation in the present manuscript is further bolstered.*

The Be-10 and C-14 concentrations, no matter what they were and even if they were indistinguishable from blank, could always be compatible with a very short ice-free period. For example, ice-free conditions for one year would produce about 15 atoms/g of C-14 and 5 atoms/g of Be-10 at the rock surface, which would be undetectable. Even though the random search inversion rejects thinning histories that have less than a couple of meters of ice for a couple of thousand years, it would always be impossible to disprove the hypothesis that there was a short ice-free period that was too short to produce a measurable amount of Be-10 or C-14. On the other hand, an ice-free period as short as hours or days would significantly perturb the OSL signal. This is why the combination of cosmogenic-nuclide and OSL data, which have contrasting (integral vs. instantaneous) responses to exposure, is so useful in this application.

- *The authors pioneered the 14C/10Be chronometer for Holocene ice burial. I might have missed it, can there be mention of this ratio in the bedrock cores and whether it or supports or excludes a scenario of ice-free conditions followed by ice burial? Are there any 10Be measurements from the ridge between today's ice height and the 1966 limit? Having a C/Be ratio in this portion could be informative in constraining the pre-1966 duration of ice at this location.*

We intentionally did not mention the 14/10 ratio in the core samples. There are two reasons for this. First, the model is fit to the direct measurements (the concentrations), not to a derivative of the measurements (a ratio). Because the ratio is just computed from the concentrations, the ratio cannot support or exclude anything that the concentrations by themselves do not support or exclude. Second, in most applications of surface exposure dating, the purpose of calculating a ratio of two nuclides is to plot data on a commonly-used exposure-burial diagram (the so-called 'banana diagram'). The purpose of this diagram is that if one assumes that a sample has experienced a single period of surface exposure followed by a single period of burial, the diagram allows graphical estimation of the duration of these two events. However, an important element in constructing this diagram is that measured concentrations in a sample must be normalized to a surface production rate so that they can be compared to idealized isochrons of exposure and burial duration, which are constructed by assuming a nuclide production ratio at the surface. This is an effective way to represent data that were collected from rock surfaces. However, it is not effective for representing data collected below the rock surface, because nuclide production ratios vary with depth. Each data point in a depth profile must not only be plotted as a separate point on a two-nuclide diagram, but must also be compared with a separate, distinct set of exposure and burial isolines. This leads to a nearly uninterpretable diagram. In addition, in this work, because we expect that the thickness of ice covering a core site is changing through time, we do not even know in advance the depth at which production in our samples took place, so we cannot even construct the burial-exposure isochrons. A burial-exposure diagram without any burial-exposure isochrons would be meaningless. Another way of saying this is that if we don't know in advance what the exposure depth is, we don't know what the production ratio is, and if we don't know what the production ratio is, then we can't interpret the measured ratio to mean anything by itself. For these reasons, discussion of the 14/10 ratio in the subsurface samples would most likely be misleading rather than illuminating, and we have intentionally avoided discussion of the ratio.

The paper does include paired 14/10 data for two bedrock samples above the modern ice surface. What the review is proposing is that these data could be used, as they have been in some situations from alpine glacier forelands in temperate regions, to infer durations of Holocene exposure and burial. However, in our case the apparent Be-10 ages are pre-Holocene, indicating that Be-10 inheritance is present, so the 14/10 ratios yield no information about Holocene events. In the alpine glacier examples, all inheritance was removed by subglacial erosion, so two measurements (Be-10 and C-14 concentrations) can be used to solve for two unknowns (length of Holocene exposure, length of Holocene burial). In our case, there is an additional unknown (the amount of Be-10 inheritance), but still only two measurements, so the solution is undetermined.

- *The exposed bedrock below the 1966 limit is a playground and offers much. It is unknown when this portion of the ridge was re-occupied by ice, but I believe it is thought that the episode of thinning earlier in the Holocene led to the exposure of the portion of the ridge between the 1966 level and the present ice surface – and the 14C numbers reflect this. Does the recent ice cover (unknown period preceding 1966) need to be considered when interpreting the 14C ages of bedrock in this zone? I guess the 14C ages here are apparent ages? Also, is there any evidence of glacial abrasion in this zone? If so, I suppose some or all of it could date to the LGM. But if all of it doesn't date to the LGM, could the 14C age scatter (in the paper explained by 'locally derived snow and ice') be explained by minor amounts of uneven late Holocene glacier erosion of the bedrock surface?*

The data from this part of the ridge are not as informative in this regard as this comment suggests. Because Be-10 concentrations indicate pre-LGM inheritance (see above), paired 14/10 data on bedrock provide no information about Holocene events. In addition, there are no properties of the data set from this part of the ridge that require late Holocene cover. For example, in a similar situation, Balco et al. (2012, QSR) observed both mid-Holocene and extremely recent (~100 yr) exposure ages in an elevation range known to have been covered by ice in recent decades. In that case, late Holocene thickening could be inferred from the exposure-age data; the two co-occurring populations are not easily explained without a multistage exposure history. At Kay Peak, however, there are no properties of the exposure-age data that could not be explained by continuous exposure since the mid-Holocene: the age distribution and scatter are similar from the ice margin to 250 m above, without evident discontinuities. In general terms this would tend to indicate that the pre-1966 period of ice cover of this site was short relative to the C-14 half-life (hundreds of years rather than thousands), but can't be interpreted more quantitatively without additional assumptions.

And, again, bedrock erosion under tens of meters thicker Holocene ice is not glaciologically feasible, as discussed above.

Finally, are the CRN depth profile shapes themselves of any use in determining where in depth space the drill cores are – ie, has there been any surface truncation? Some of the profile data (H4 and H5) look remarkably flat, that is, fairly unchanging with depth (in Fig 7A-H5, the statistical profiles have a rather uncomfortable fit with the data). Perhaps this is a product of residing below meters of ice and a few cm of surface erosion would not be possible to detect.

The answer to this is yes, the shape of the depth profiles is critical in fitting the model. Basically, the model fitting is seeking to do two things: match the concentrations and the shape of the profile. This explains why the best-fitting model results display a tradeoff between lowstand duration and ice thickness. Increasing the ice thickness during an ice-free period increases the effective attenuation length, but decreases the production rate, so thicker ice requires a longer lowstand. If there was no depth profile and only a surface sample existed, then any length of lowstand could be fit by adjusting the ice thickness, and any ice thickness could be fit by adjusting the length of the lowstand. It would not be possible to constrain either parameter. We explain this relationship on pp. 17-18 in the original MS, and it has also been discussed at length in other references, for example Schaefer et al. (2016, Nature).

3. *As I wrote in my intro remarks, this manuscript does a remarkable job explaining a complicated dataset simply. The figures are outstanding. Yet, they are mostly highly technical. This is, of course a technical paper in a technical journal. Nevertheless, I wonder about a summary figure that shows the glacier history in a more cartoon fashion. A multi-panel cross section or something that shows the ice thickness history in several time slices through the Holocene. Something for the broader readership. Additionally, there seems to be some important interpretations that are rather nuanced and difficult to visualize without such a figure, such as the potential that the drill sites are slightly off ridge and could be covered by a ramp of glacier ice. This is rather important, as this point is used to suggest that the calculated ice thinning may be a minimum. Finally, some field pictures of sample sites could be really helpful, and what about any photographs of the cores themselves? For those of us who like to teach about very cool studies like this, these visualizations go a long way.*

Although we appreciate this point, a cartoon or visualization figure that aimed to show the ice sheet configuration at various times in the Holocene is problematic in the context of this paper, because drafting such a figure would by necessity involve extrapolating the reconstructed ice thickness history at the core sites to other locations. What we have done in this paper is reconstruct the ice thickness history at one site and one site only. This is extremely important because the significance of our reconstruction depends on how one extends this observation to other locations. Specifically, does mid-Holocene thinning at this site imply regional grounding line retreat, as the observations from recent decades imply? Or is thinning at this site purely a local effect that is unrelated to the grounding line position? Because this question is so important, we have intentionally been extremely clear in this paper that we have reconstructed ice thickness changes at a single site only. This reconstruction is a major accomplishment, because it has never been done before in a targeted bedrock recovery drilling project. However, the larger significance of this result depends on how the observations from one site are related to conditions elsewhere. The only evidence we have that addresses this question is the association between thinning at the site and grounding line retreat observed in recent decades. This evidence does imply that mid-Holocene thinning was also associated with grounding line retreat, so we make this point and then stop. The next step in evaluating the implications of our results for the regional ice sheet configuration would be a high-resolution modeling study, which is not part of this work. For this reason, we have specifically presented the results of this work as an ice thickness change history at one site (as shown in Figs. 7, 9, and 10), and not as a reconstruction of the ice sheet configuration in nearby areas. Thus, we conclude that a cartoon or visualization figure would be potentially misleading.

Finally, with regard to photos of the cores, it is true that these cores are notable because there are only a few cores that have ever been collected from bedrock beneath ice sheets, but from the geologic perspective they are not very exciting – they are completely normal-looking cores of unremarkable biotite gneiss that look exactly like the outcrop on Kay Peak Ridge a few hundred meters away. There's nothing about the appearance of the cores that is significant to the paper in any way, so there does not appear to be any value in showing photos of the cores in the main text. As noted, the supplementary data contains links to the core photos, but we could certainly include the photos themselves in the supplement.

- page 2 line 9, could be useful not only to mention that people have modeled irreversible glacier retreat systems, but what they have found.

We can clarify this section (**Revision 3**)

- page 2 line 20, here the authors are describing how OSL-ED and CRN systems work in the remote case of long-lived thin ice cover. Why not start by describing the more obvious case of an ice-free then ice-covered system. The text at present certainly foreshadows your interpretations, but sort of skips the basics first.

Well, we were trying to make the point that OSL can't detect a period of thin ice cover, but cosmogenic-nuclide data can. Clearly this needs some revision. We will clarify this section. (**Revision 4)**

- figure 1, add legend of the ice velocity. Also, sometimes authors use hotter colors for high velocity (reds) instead of "colder" colors like blue.

The ice velocity is intentionally shown in receding, "cool," colors because the purpose of the figure is to show grounding line positions, and the ice velocity is only there to highlight where the glaciers are. The magnitude of the ice velocity has no relevance to the paper subject matter. We will make clear in the caption that the ice velocity is presented in a nonquantitative way. (**Revision 5**)

- figure 2, I struggled a little bit here. The pink dots shown on the graph are fewer in number than the pink dots on the image - I'm guessing that the plot doesn't encompass the entire transect shown in the image. But I had to stare and dig deep to conclude that. Also, there is mention of a sample at 171 m asl, but this plot doesn't seem to have a dot at that exact elevation.

As foreshadowed in the first part of this review comment, the 171 m sample is actually off scale to the left on the inset axes, so not visible. We will make clear in the caption that the inset axes only include the lowermost portion of the ridge (**Revision 6**). Note that all the elevations of erratic and bedrock samples above the ice surface are correctly represented in Figs. 4 and 7, so it is not necessary for the reader to rely on Fig. 2 for this information.

- page 6 line 21, here it states that stable ice for millennia is unlikely; isn't that more or less the interpretation that the manuscript goes with? The ice-thickness histories plotted in Fig 7C during the middle Holocene (the middle segments) appear flat and pretty unchanging.

As discussed above, we made a mistake and used "stable" here. What we mean to say is that the hypothesis that the ice thickness was EXACTLY THE SAME between the mid-Holocene and the present is unlikely. We will fix this. (**Revision 7**)

- page 8 line 35 – could this "fragmented rock" be surface debris? Back to above comment about shielding the underlying bedrock surface during an ice-free period. This debris could theoretically be ephemeral, exist during an ice-free period but then be transported off a rock core site during subsequent overriding.

Well, we don't know what it is. As the bedrock is densely jointed, the boundary between "bedrock" and "surface debris" is a bit arbitrary. However, this clast was embedded in debris-rich ice, not in contact with the bedrock, so it could not have been resting on the surface. We will clarify that whatever it is, it was embedded in the debris-rich ice and not in contact with bedrock. (**Revision 8**)

- figure 6, fantastic idea to re-occupy an older rock sampling divot. Could you model a profile that shows a plausible history alternative to the one this paper points to? - that is, middle Holocene exposure until something like 2 ka, and then subsequent burial to present? It could be nice to see such a scenario stand in contrast to these data that were generated, much in the way that the null hypothesis is plotted in Figure 7A/B. Also, as an aside, it sure would be cool to have a core with OSL data from the above-1966 ridge just to see if you can match the 14C exposure ages – would be a nice proof of concept.

Figure 1 below shows this model calculation. This scenario would predict L_N/T_N signals very different from what we observed, and can be easily excluded.

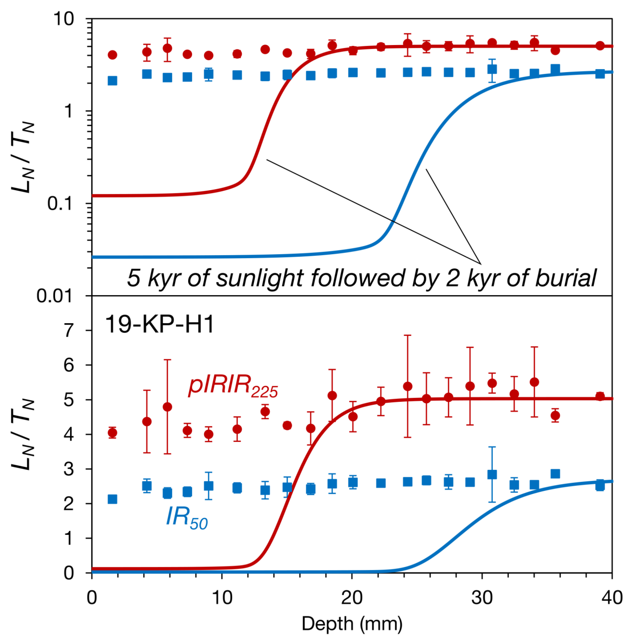

Figure 1. Observed IR_{50} (blue boxes) and $pIRIR_{225}$ (red circles) luminescence depth measurements from core top sample 19-KP-H1 are compared to depth profiles from a hypothetical, simulated exposure history in which the rock surface was completely exposed during a Holocene ice thickness lowstand. In this history, the sample was exposed to sunlight for 5 kyr and then buried by ice for 2 kyr before measurement. This highlights that the measurements are incompatible with this scenario. The two panels show the same information on logarithmic and linear y-scales.

-page 12 line 11, not sure I entirely understand this sentence. It says that the 14C "concentrations are 2-3 times typical detection limits," but then also "and are significantly lower than routinely measured." Lower on line 25, it says these samples are "near detection limits." Collectively, these statements leave me a little confused.

We can and will clarify this. Note that we discuss the issue of the C-14 blanks at length in the response to the other review by Nat Lifton. (**Revision 9**)

- figure 7C, is there a reason to choose the history ending with the present ice thickness at H5 in the scenario-modeling instead of a higher surface that could correspond to the "1966" thickness, which is 30 m higher? I suppose all you could do is estimate. Perhaps it is of no real consequence.

The reason is that it would add complexity without affecting the results. Because of the need to match both the long apparent attenuation (by making the ice thicker during a lowstand) as well as the concentrations (by making the lowstand longer), the random search optimizer nearly always chooses thickness change histories in which thickening from the lowstand back to the modern elevation happens at the latest possible moment. This is evident in Figure 7: nearly all the well-fitting models shown in green come up to the modern ice surface extremely steeply at the end. The result of this is that if we force the models to come up to the 1966 elevation and then back down in 2020, the period before 1966 during which the ice is thicker than present becomes so short that it is negligible in relation to the total nuclide concentrations. Thus, adding this constraint to the model makes it more complicated but doesn't affect the results.

- page 16 line 26, interesting interpretation of the basal debris-rich ice. It sort of seems that erosive ice during the Holocene is ruled out, therefore this debris must relate to the LGM. But I didn't find anywhere in the paper where this is stated. Ruling out more firmly a Holocene origin could help to bolster the interpretation in the paper.

This issue is (hopefully) thoroughly discussed above.

- page 20 line 6, here there is a lot packaged into one sentence. This amount of grounding line retreat is somewhat critical for justifying the paper's title which challenges the irreversible glacier concept. I'm not against the authors' choice to not include a lengthy "implications" portion to this paper, but they could add some supporting discussion here.

As discussed above at more length, this is an important element of the paper. We have just made observations at one site. It is possible to make a reasonable hypothesis about how to extend these observations to a wider area by comparing them with observations in recent decades. Anything after that would be speculative without a serious modeling study. Regardless, this section clearly needs some clarification. We will improve this in a revised MS. (**Revision 1**)

- page 20 line 9, similarly, a lot is packaged into the sentence on the RSL-rebound control on the grounding line in this system, readers might find bit more on this useful.

We can clarify this. (**Revision 10**)

- page 20 line 12, I generally try to avoid making stylistic/subjective comments about writing, but I'll make one here – sentence beginning with "Thus" is lengthy and I found it a little difficult to follow.

We can clarify this. (**Revision 10**)

To summarize, proposed revisions to address these review comments are as follows:

1. Clarify reasoning about irreversibility/grounding line position vs. thinning in Section 1 and elsewhere
2. Explain clearly about cold-based ice/no erosion during late Holocene cover in Sections 1 and 2
3. Additional context for 'irreversible' in Section 1
4. Clarify cosmogenic-nuclide vs. OSL sensitivity in Section 1

5. Clarify that ice velocity representation is nonquantitative in Fig 1 caption
6. Clarify that profile axes are truncated in Fig 2 caption
7. Remove 'stable' on p. 6
8. Provide more context for loose rock recovered in fourth borehole
9. Clarify detection limit issue for C-14
10. Clarifications and editing for concluding remarks in Section 6