Dear Dr. Stroeven,

We are pleased to submit our revised manuscript, now titled "Relationship between meteoric ^{10}Be and NO_3^- concentrations in soils along the Shackleton Glacier, Antarctica." We have updated the title to reflect changes to the manuscript, which are aligned with critiques from Dr. Goehring and Referee #2. Dr. Goehring and Referee #2 have once again provided suggestions and comments which have greatly improved and streamlined our work. We are very grateful for their help. We have addressed specific comments in the pages below.

After considering the reviews and conversations amongst the authors, we have decided to shift the focus of this manuscript towards process. Specifically, we centered on the relationship of meteoric ^{10}Be and NO_3^- with depth. This change does not significantly change the writing itself, but instead mainly the order and manner in which we frame and present the data. The most substantial change was moving the ^{10}Be – NO_3^- dating and the ^{10}Be inventory dating (formally measured and estimated ages, respectively) to the supplement. The inferred ages using a previously established relationship between maximum ^{10}Be concentration and inventory are located at the end of the discussion (Section 6.2). While Referee #2's assumptions about our ^{10}Be – NO_3^- method are misguided due to our initial lack of detail in the text (see comments below) and the technique holds promise for future work, ^{10}Be systematics in CTAM soils still needs further interrogation before exposure ages can be accurately determined.

We have three different soil profiles: Roberts Massif – a hyper-arid site with long exposure, Bennett Platform – a site recently uncovered by glacial retreat with large moraines, and Thanksgiving Valley – a site with a nearby active hydrologic system. We hypothesized that the relationship between the concentrations of ^{10}Be and NO_3^- for these soil profiles would be different, and they were. This was to be expected given our understanding of the mobility of NO_3^- and ^{10}Be in soils with different wetting and glacial histories. While this makes for an interesting comparison between the sites and informs landscape disturbance (either by glaciers or wetting), the fact that all the sites are different makes evaluating the technique difficult. For example, Dr. Goehring encouraged us to pursue a sensitivity analysis for both the ^{10}Be – NO_3^- regression and the ^{10}Be inventory method. While this is relatively straightforward for evaluating the sensitivity of erosion in Eq. 4, the small number (3) of regressions makes the Monte Carlo statistical analyses questionable.

Our exposure duration estimations are comparable to cosmogenic exposure ages of respective nearby features in the Shackleton Glacier region (see supplementary figures S2 and S3), supporting this technique. However, with only three profiles that represent the complex soil environment of the region, we first need to describe the soil depositional environment and demonstrate that geochemical relationships exist before the ages can be verified.

The revision to our manuscript positions this work as a foundation to build upon for understanding landscape development, disturbance, and exposure age dating for Antarctica soils using meteoric ^{10}Be and NO_3^-. Most ages terrestrial ages from Antarctica are ages of boulders and moraines, not soils. It appears that only one other study has measured salts and meteoric ^{10}Be in soil from the Central Transantarctic Mountains (CTAM) (see Graham et al., 1997). Graham et al. conclude that meteoric ^{10}Be systematics needs further study as an exposure proxy in the CTAM, but there has been no progress until our study. We show that meteoric ^{10}Be inventories in the CTAM are similar to other hyper-arid soils in Antarctica, though interestingly the depth profiles themselves are variable. This is true for NO_3^- as well. Since it is expected that NO_3^- and ^{10}Be would have similar concentration depth profile patterns in hyper-arid soils, deviations from this relationship help us understand if/when the soils were disturbed.

By focusing on the concentrations, patterns, and relationship of meteoric ^{10}Be and NO_3^-, have taken the first step towards determining accurate soil exposure ages for the CTAM. Our work is not only critical for glaciologist and geomorphologists seeking to understand glacial advance and retreat as well as paleoclimate, but also for biologist searching for Antarctic refugia.

To summarize the contents in the following pages, we have made the suggested changes by both Dr. Goehring and Referee #2, which mainly entailed some reorganization and moving the ages to both the end of the discussion and the supplement. We hope to do a full sensitivity analysis of our exposure methods in a future study once we have collected sufficient measurements.

Best regards,

Melisa Diaz (on behalf of all authors)

Postdoctoral Scholar
Woods Hole Oceanographic Institution

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Received and published: 14 August 2020

General Comments

> *There is still a lack of uncertainty analysis and quantification. This is a major issue in my mind. The authors presented results with and without erosion, and the differences are sometimes large, sometimes not; this screams for a full analysis of the sensitivity to erosion. You also rely on regressions to assess 10Be concentrations/inventories, any regression model has uncertainty and must be incorporated. The fact that uncertainties are discussed in other papers, means that those authors discuss the possible conceptual uncertainties in the application of the method. I was not referring to this, and rather referring to the resulting uncertainties on your results. I do agree that there is not a single number that results, but rather a distribution of values. I really do think this manuscript, and methodology, would benefit from this particularly since there is a new method, it relies on a regression, and all regressions by definition have some measure of uncertainty.*

We are very grateful to Dr. Goehring for explaining the need for the sensitivity analysis. We agree that our surface exposure age method and calculations appear sensitive to the erosion term. Additionally, we acknowledge that the uncertainty in our regressions needs to be considered. Given the comments from Reviewer 2 among other concerns, we have decided to move the ages calculated using the ^{10}Be inventories and the ages using the ^{10}Be – NO_3^- regression to the supplementary materials, and have moved the inferred ages to the end of the discussion. With our three sediment profiles from Roberts Massif, Bennett Platform, and Thanksgiving Valley, we have demonstrated that there appears to be a relationship between ^{10}Be and NO_3^- with depth in hyper-arid TAM soils and this relationship can help inform wetting history, landscape disturbance/development, and possibly exposure age. These sites were selected because we hypothesized that they had different wetting and glacial histories, which is what our analyses ultimately showed to be true. However, since they are all different cross comparison is difficult and we ultimately would like to collect more and analyze more data for a future study specifically dedicated to interrogating our exposure age proxies. In this future study, we will certainly run Monte Carlo simulations for both the ^{10}Be inventory and ^{10}Be – NO_3^- ages.

Detailed Comments

> *Line 20: Sentence starting this line seems incomplete. I feel it needs some sort of comparison to regions elsewhere. Also, the statement about largest changes in the TAM is not true for the entirety of Antarctica, but really only applicable to EAIS.*

We have corrected this sentence.

> *Line 25: How can you calculate a measured age? I think this sentences just needs to say very clearly what was done and remove the parenthetical aspects. Maybe rephrase as "We measured meteoric 10Be and NO3- concentrations to calculate exposure ages using the total 10Be inventory, the NO3 concentration, and infer exposure duration from the 10Be surface concentration."*

We have edited the abstract and removed this sentence since the ^{10}Be ages have moved to the supplement.

Line 27: Swap lower and relatively

This sentence has been removed.

Line 29: Change to indicate

We have changed the tenses in the abstract.

Line 51: All evidence points to WAIS and EAIS max extent not synchronous with the canonical LGM (26-19 ka) and likely were largest approx. 14ka.

We have made this correction.

Line 80: Delete "it"

We have made this correction.

Line 81: You is spelled Yiou

We checked the original publication and the author's name is indeed You.

Line 87: Insert "of" after "measurement"

We have made this correction.

Line 116: Maybe instead of "measured" say "as measured". That being said, the explanation here is better than in the abstract. Can you try to clarify the abstract a bit more?

We have moved the measured ages to the supplement.

Line 163: Many readers wont know what UVM stands for, suggest spelling out even though totally inconsequential for the manuscript.

We have defined this acronym.

Line 182: replace the comma with and.

We have made this correction.

Line 185: Expressing E as length per time in the text and then adding with respect to density is confusing, as the function really works with mass depth per time for erosion, E. Consider revising or rewording the text.

We have reworded the text. It now reads, "The concentration of meteoric ^{10}Be at the surface (*N*, atoms g^{-1}) per unit of time (*dt*) is expressed as a function, where the addition of ^{10}Be is represented as the atmospheric flux to the surface (*Q*, atoms cm^{-2} yr^{-1}), and removal is due to both radioactive decay, which is represented by a disintegration constant (λ, yr^{-1}), and erosion (*E*, cm yr^{-1}) (Eq. 1). Particle mobility into the soil column is represented by a diffusion constant (*D*, cm^{2} yr^{-1}). The differential in depth is represented by *dz*."

Line 285: Delete "regressed"

We have deleted this.

Line 292: Suggest presenting model ages in same order, first no erosion and then with erosion as above.

We have taken this suggestion for the ages, now in the supplement. See Section S2 in the supplement.

Line 295: Here and throughout there are some inconsistencies with tense and passive voice. Strongly suggest going through and editing for this and some other grammatical issues I noticed. I tried to point out many of them but skipped over many.

We thank Dr. Goehring for bringing some of these errors to our attention and have corrected our grammar throughout.

Line 377: Delete "study" before second clause.

We have edited this section.

Line 426: Should be stable

We have made this correction.

Line 438: Insert "of" after "most"

We have made this correction.

Line 460: MacKintosh is the same person as Mackintosh. The latter is correct. I noticed this elsewhere and suggest fixing before copy editing.

We have made this correction.

Figure 8: The black triangles are hard to see (even with the outline) on the imagery as the rock is so dark, and combined with ribbons of snow makes the two hard to differentiate. The symbols would also greatly benefit from displaying the ages on the maps.

We have updated the symbol colors for Figure 8 (now Figure S2). We tried adding ages to the figure, but the figure quickly became difficult to interpret considering the wide range of ages from the literature. The ages are still included in the text and in Tables 3 and S3.

Figure 9: Age-elevations plots are usually presented as elevation on the y-axis and age on the xaxis. I suggest flipping axes.
Figure 10: Swap axes like in Figure 9 for Figure 10b. For figure 10b, since you are comparing age vs elevation and showing along the length of Shackelton Glacier, where elevations span 1000 m, I suggest presenting as elevations relative to the ice surface. This will remove the slope effect on the absolute elevation.

We have decided to keep elevation and distance from coast on the x-axes for what is now Figure 8.

Table 3: Uncertainties here and for all other columns should be presented using the same exponential as the concentration, otherwise just adds confusion. I know this was mentioned in my first review, there would only be one leading zero for most of the samples. One other note is that PRIME Lab reports values at two decimal places, I suggest only reporting to this precision. There is unlikely a need to present non-background corrected ratios, they are so high that it is unlikely that many will be have any significant correction.

We have changed the uncertainties to the same exponent as the concentration data and have moved the non-background corrected ratios to Table S2. Table 3 has now combined with Table 1 and includes the NO3 data.

Anonymous Referee #2
Received and published: 15 August 2020
Second review, Diaz and others, Esurf

> *1. Introduction. This is enormously improved. It is basically fine now, although it would be helpful to better orient the reader (who perhaps has skipped over the abstract) by beginning the introduction with a sentence describing the actual research in the study, like "This study reports concentrations of atmospheric fallout constituents in soils in the southern TAM. These data can be used to understand soil age and disturbance frequency, which are biogeographically important because one of the most intriguing questions..."*

We thank Referee #2 for the suggestions on the introduction. We have rearranged the Introduction and Background sections to be more streamlined. The study goals are now in the second paragraph of the Introduction.

> *2. Background section. Also much improved. Great. Minor points:*
> *In lines 98-99, do you mean lower elevations, like water is running downhill for a significant distance, or greater depths in the soil? Clarify.*

We've edited this sentence on lines 115-116. It reads, "Once deposited on the surface, nitrate salts can be dissolved and transported down gradient or eluted to depth when wetted…"

> *In line 111. "Considerably fewer" sounds strange to the reader here because you are saying there are fewer studies in the CTAM than in NTAM/NVL, but you cite more studies for CTAM. I would just remove the "considerably fewer" and note that there have been scattered exposure-dating studies all over the Transantarctic Mountains.*

The sentence on lines 98-99 now reads, "There are scattered exposure age studies from across the CTAM using a variety of in-situ produced cosmogenic nuclides…"

> *3. Methods section. This is much better. Could use minor clarification in a couple of places, as follows:*
> *Line 151. Perhaps clearer to say "...to represent soils likely to have been covered during the LGM and exposed by more recent ice margin retreat."*

The sentence on line 157-158 now reads, "A second sample was collected closer to the glacier (between ~1,500 and 200 m from the first sample) to represent soils likely to have been covered during the LGM and exposed by more recent ice margin retreat."

> *Line 189. The usage of 'dz' is mathematically strange here. dz is just a generic differential in depth, it is not a parameter needed to evaluate the equation, so it is unclear what 'highly dependent on dz' means. I think what the authors are trying to say here is that the concentration gradient dn/dz depends on what D is and also varies with time, and D is unknown, so it is not really possible to calculate dn/dz. In addition, in line 187, the authors use 'concentration gradient' to describe d2n/dz2, which is confusing*

because 'gradient' usually means the first derivative of something (dn/dz). The second derivative (d2n/dz2) would typically be described as 'curvature.' In any case, both of these points give the impression of carelessness and this section needs to be carefully checked to make sure it makes mathematical sense.

We thank Referee #2 for identifying this confusing section. We have made modifications to clarify. See Section 4.3.

Lines 211-12. This doesn't seem to make sense, because if Be-10 is supplied from the surface, the concentration has to decrease with depth at some point, no matter what. I believe the difference the authors are trying to point out is that in a normal soil one would expect a fairly smooth decrease, but in a perigacial soil one might expect a well-mixed active layer with constant concentration abruptly overlying a frozen layer with much lower concentration. However, this is not what this sentence says. In any case this sentence is oversimplified to the point of causing confusion, and it's not really very important, so I would remove it.

We have modified lines 215 - 217 for clarity. The text now reads, "However, an accurate initial inventory can only be determined for soil profiles that are deep enough to capture background concentrations. This may not be the case in areas of permafrost where 10Be is restricted to the active layer."

4. Results. The basic description of the results, up to section 3.2, is good. At this point, however, we get into the subject of the relationship between meteoric Be-10 and NO3 concentrations, which needs work.

Correlated inventories of Be-10 and NO3 that both increase monotonically with exposure age is, of course, what we expect, not only from first principles but due to the Graly study at Mt. Achernar, which showed these amazing correlations between exposure age and salt concentrations in sediments in a blue ice moraine. That is exactly how it is supposed to work and, like the authors, I want it to work that way in this study. Unfortunately it doesn't. I thought about this issue a lot in putting together this review – because, as noted, I would like this to work – but when you look at the actual observations in this study, the only possible conclusion is that it does not work this way.

Before we address specific comments on the ^{10}Be – NO_3^- dating method, we want to emphasize that it was never our assumption that ^{10}Be and NO_3^- would have the same relationship across the Shackleton Glacier region. It is clear through satellite imagery that landscape development and evolution is variable for our sampling locations. As such, we intentionally selected three sites that we thought represented some of this variability. We apologize this was not clearly mentioned before. We have added the following sentences to Section 4.1, "We selected Roberts Massif, Bennett Platform, and Thanksgiving Valley as locations for the most in-depth analysis for the depth profiles. These locations were chosen to maximize variability in landscape development: Roberts Massif represented an older, likely minimally disturbed landscape; Thanksgiving Valley represented a landscape with possible hydrologic activity, as evidenced by nearby ponds; Bennett Platform represented a landscape with evidence of recent glacial advance and retreat, and substantial topographic highs and lows (Table 2)."

> *The following section of this review explains why in probably too much detail.*
> *So, more specifically, the Be-10/NO3 relationship should be close to linear for relatively young soil ages, but as soil age increases enough that Be-10 decay is important, the slope of the relationship will change as the Be-10 inventory asymptotically approaches an equilibrium value where deposition is balanced by radioactive decay. Stated in math, this means that the relationship between Be-10 and NO3 inventories is given by parametric equations in t (time, yr) for the NO3 inventory*
> *So what just happened there was we went from two simple assumptions to a quantitative prediction for how measured inventories of Be-10 and NO3 should be related. There are some additional side predictions that will be important later. One is that the Be-10/NO3 ratio should be constant and equal to the deposition flux ratio for young soils, and will be lower than the depositional ratio for old soils because of radioactive decay. The Be-10/NO3 ratio can't be higher than the depositional ratio with these assumptions.*
> *Continuing, the authors then make a third assumption, which is that Be-10 and NO3, once deposited, are transported together. If this is true, then not only the inventories, but also the concentrations, will be highly correlated. The slope of the relationship could vary in old soils, or in parts of the soil profile that have not exchanged Be-10 with the atmosphere for a while, because of Be-10 decay, but all three assumptions together predict a positive correlation between measured concentrations.*
> *This assertion is the basis for what the authors do next, which is to further assert that if they can establish a correlation between measured Be-10 and NO3 concentrations, they can then use this correlation to estimate Be-10 concentrations and therefore inventories in samples where only NO3 was measured. They go on and do this, and many of the apparent exposure ages that are eventually presented in the paper are from estimated Be-10 concentrations.*
> *So far, the only problem with the paper is that the authors have not actually clearly stated the assumptions that led to their assertion that they can use NO3 concentrations to predict unmeasured Be-10 concentrations. However, I am now going to point out a lot of other problems.*

As we stated earlier and throughout our response to Referee #2, we have moved the ^{10}Be – NO_3^- dating approach to the supplementary materials (see Section S2.2). We agree that we did not clearly state our assumptions before interpreting the relationship between ^{10}Be and NO_3^-. With the focus shift in the narrative from dating towards process, we expand upon our assumptions in greater detail in Section 6.1.

Specifically, lines 304-315 read, "Given sustained hyper-arid conditions, minimal landscape disturbance, and negligible biologic activity, one can expect meteoric 10Be and NO3- to be correlated throughout a depth profile given the similar accumulation mechanism (Everett, 1971;

Graham et al., 1997). Further, their inventories (Eq. 2) should increase monotonically with exposure duration. Deviations from this expected relationship could be due to 1) soil wetting, either in the present or past, 2) deposition of sediment with different 10Be to NO3- ratios compared to the depositional environment, 3) changes in the flux of either 10Be or NO3- with time, and 4) additional loss of NO3- due to denitrification or volatilization. The latter two mechanisms are likely minor processes, however, NO3- deposition fluxes are known to be spatially variable (Jackson et al., 2016; Lyons et al., 1990). As described above, Roberts Massif, Bennett Platform, and Thanksgiving Valley were selected for further investigation as locations which may represent different depositional environments: hypothesized hyper-aridity, recent glacial activity with large moraines, and active hydrology, respectively. By comparing differences in the expected and observed relationship between 10Be and NO¬3-, we can infer the processes which have influenced their relationship."

> *Problem 1 is that assumptions 1 and 2 predict a specific quantitative relationship between Be-10 and NO3 inventories. Both inventories were measured at three sites. The following figure compares these inventories to the predicted relationship from the equations above.*
> *The point of all this is that the data clearly show that whatever we think ought to be happening, Be-10 and NO3 inventories are not correlated, and Be-10 and NO3 concentrations in surface samples are not correlated. Therefore, there is zero reason to believe that the authors' attempt to predict Be-10 concentrations or inventories in samples where they were not measured is correct. Of course it might be correct by accident, but this seems unlikely.*
> *Note that the authors actually tried to do this in a more complicated way. They showed that within each of the three depth profiles where both Be-10 and NO3 were measured, they could be related by a power-law relationship. These relationships were different for all three soils. Then they used these relationships to predict Be-10 concentrations at unmeasured sites simply by asserting which unmeasured soil was most like which measured soil.*

The next several paragraphs of the review suggest that Referee #2 assumed we believed that ^{10}Be and NO_3^- would have the same relationship across the region. This was not our intention and we have clarified throughout the manuscript. In our baseline assumptions, we argue that ^{10}Be and NO_3^- are deposited by atmospheric deposition at a fairly constant rate and at a fixed ratio. Further, assuming neutral soil pH and sustained hyperaridity, their concentrations within a soil profile will be similar since they are both conservative. Deviations in this expected relationship can help us better understand the history of the landscape.

Referee #2 argues that ^{10}Be and NO_3^- are not correlated in the CTAM soils. While Referee #2 is correct that if you combine all measurements and plot them, there is not a clear relationship, Roberts Massif, Bennett Platform, and Thanksgiving Valley all demonstrate that there is indeed a correlation between ^{10}Be and NO_3^-, albeit a complicated one. In Fig. 6b, we show the concentrations of ^{10}Be and NO_3^- in the depth profiles. For Roberts Massif, the pattern is the same: the concentration increases just below the surface and then starts to decrease again. The concentrations for Thanksgiving Valley are similar and do not vary significantly throughout the profile. On the other hand, the ^{10}Be concentrations at Bennett Platform decrease with depth, while the NO_3^- increase with depth. Despite each of these locations having different concentration – depth relationships and probably different inheritance, ^{10}Be and NO_3^- still have statistically significant (though we acknowledge the low number of data points) and clear positive or negative correlations. Considering each location depth profile separately was not to the "more complicated way" since we hypothesized and showed that ^{10}Be and NO_3^- varied depending on wetting history and inflation/deflation. Once again, we apologize for the confusion and that our assumptions were not clearly stated in the beginning.

The summary of this rather long discussion is that there is exactly zero observational support for the authors' scheme for predicting Be-10 concentrations from NO3 concentrations, and, in addition, zero theoretical support for some aspects of the scheme like the decision to use a power-law fit. In fact, comparison of theory to observations indicate that this should not work, except by accident. Even if the basic assumptions are correct, the expected correlation is not present due to some combination of background effects and NO3 leaching. Thus, the predicted Be-10 concentrations for sites where only NO3 was measured are incorrect. The authors must remove this aspect of the paper and consider only Be-10 and NO3 concentrations that were actually measured.
Correcting this problem will involve:

–entirely removing section 5.3.2.

Removed from the main text and into the supplement.

-Removing the results of the calculations in 5.3.2. from all tables and figures, so that only measured data are shown throughout the paper. That includes removing Table 4.

Removed from the main text and into the supplement.

—Removing and rethinking any discussion that relied on the estimated Be-10 concentrations.

Removed from the main text and into the supplement. The discussion has been refocused.

− Removing the discussion of this subject in section 6.2.

Removed from the main text and revised for the supplement.

− Removing discussion of this subject from the abstract and conclusions.

Removed from the main text and into the supplement.

A couple of final notes on this aspect of the paper here. First, I want to assure the authors that the paper will be just as good, in fact, better, without this element of the paper. I get the idea that the authors have an interesting data set that is difficult to easily interpret, so they are kind of struggling to make the paper have some elements that they perceive as

more significant than just a set of empirical observations. However, the authors need to keep things in perspective here. These are a set of totally new observations from a part of Antarctica and a type of setting that no one has looked at from this perspective before. Most (possibly all) of the existing meteoric Be-10 data in Antarctica are from really old dry soils that have been ice-free for a really long time. The data in this paper are from much more complex sites with a complicated history of ice cover and exposure.
If this paper presents the data, points out that they are much more complicated than we expect from simple relationships found at more simple sites, and then stops, that is a big contribution. Instead de-emphasizing the complexities and simply asserting without cause that we know what is going on with these data doesn't make the paper better, it makes it worse. A clear and comprehensive observational study is extremely valuable. It is not necessary to try to explain everything, or to add speculative material, to increase the perceived interest of the study.

We thank Referee #2 for acknowledging the value of our data and the importance of our interpretations. We are excited to continue studying the relationship between meteoric ^{10}Be and salts for future CTAM studies.

Second, if you had asked me before I read this paper whether there would be a strong correlation between Be-10 and NO3 in these samples, I definitely would have said yes. The results from the Graly paper about Mt. Achernar are extraordinarily clear in this regard, and I would have expected a similar situation here. The actual observations that show no correlation between sites are amazingly different from the Graly results. This clearly indicates that there is something that we are missing and the setting is much more complicated than we thought, and it is extremely clear from these results that the expected behaviour is wildly oversimplified. At present, this paper just asserts that the expected behaviour is true even if the observations don't agree with it. It would be much more valuable for this paper to highlight that apparently the expected behaviour is very oversimplified relative to reality and that we are missing important things, most likely having to do with variable inheritance and/or NO3 mobility in water.

We have revised the text to emphasize the complexity of CTAM terrestrial environment. Much of this discussion is presented in Section 6.1.

Coming back to the overall paper, the rest of the discussion having to do with the apparent exposure ages is in pretty good shape. A few comments on the discussion:
The sentence in lines 336-339 doesn't contribute anything and should be removed. Start with "The Shackleton Glacier region..." It seems like the authors had some trouble getting started in this section...they should just simplify things by starting right into the observations they want to highlight.

This sentence has been removed.

Area around line 364. It seems to me the easiest explanation for the greater-than-LGM apparent meteoric Be-10 ages for the lower-elevation covered-at-LGM sites is just that the inherited Be-10 inventory is large compared to the relatively short exposure time at this site. Inherited Be-10 equivalent to 10,000-100,000-ish years exposure seems unsurprising.

This have been corrected in the supplement on line 127 in Section S3.

Area around line 425. As discussed in the first review, the authors need to be more careful not to mix up evidence for aridity with evidence for ice sheet change or lack thereof. These are not at all the same thing.

We thank Referee #2 for this reminder. We were attempting to make two different statements here about the past climate and glacial history. We have ensured that our references are appropriate throughout.

Line 436. I don't understand why a shallow active layer implies that Be-10 was able to migrate deeper into the soil in the past. How do we know that it hasn't been the opposite – the active layer was shallower in the past and has been thickening over time?

We have updated this sentence on lines 333-334 to read, "This suggests that the active layer may have deepened and shallowed throughout time, and modern 10Be mobility is limited to the top ~20 cm for most of the Shackleton Glacier region."

Line 448. I don't understand this argument. You can get to an increasing NO3/Be-10 ratio lots of ways. As is evident in many otherwise dry soils in Antarctica, there is commonly a subsurface maximum in salt deposition just because of brief wetting by snow events moving salts below the surface and depositing them when the water sublimates. As NO3 has more pathways for mobility than Be-10, it seems much easier to explain this by enhanced NO3 transport instead of by some complex inheritance effect.

We have modified this portion in lines 345-356 to consider both processes.

5. Conclusions. Besides the need to remove discussion of estimating Be-10 from NO3 in lines 489-492, I only have one comment here. As noted above, I don't understand why the presence of soil ice requires a past warmer climate. Soil ice can easily form and remain under equilibrium conditions. Explain?

We agree this was confusing and have removed it from the conclusions.

6. Other items:

As discussed in my request from the editor for a revised table, it was unnecessarily difficult to get all the data from the tables that were provided, because it was hard to connect which exact samples from which depths did or did not have Be-10 and/or NO3 measurements. The table that the authors provided in response to my request is much clearer. The authors should use that table instead of the existing Table 1, and leave the *details of the Be-10 measurements in a separate table, which would be basically the current Table 3.*

We thank Referee #2 for this suggestion. Table 1 now includes both the ^{10}Be and NO_3^- data. The metadata for ^{10}Be are now included in the supplement as Table S2.