Dear Dr. Koppes,

We are grateful to Dr. Goehring and an anonymous second referee for their thoughtful and detailed reviews of our manuscript, “Relative terrestrial exposure ages inferred from meteoric ^{10}Be and NO_3^- concentrations in soils along the Shackleton Glacier, Antarctica.” We have addressed the two reviews in detail with pertinent questions, comments, and concerns distilled below.

To summarize, we agree with both Dr. Goehring and Referee #2 that the manuscript would greatly benefit from re-framing and clarification, particularly in the introduction, methods and discussion. The manuscript in its current form is staged as a geomorphologic study. Although the measurements, data, and interpretations we present are useful and of interest to the glaciological community, the original design of the study was to support a biological survey. The goal of the study is still the same – to calculate relative surface exposure ages – but the original purpose in determining these ages was to better understand ecological succession and refugia following glacier advance and retreat. As Referee #2 points out, this is not mentioned in the manuscript. Additionally, much of the current text is focused on the broader interpretations of the data, as opposed to the data themselves. As this is the first work to relate meteoric ^{10}Be and nitrate concentrations in this manner, we agree that there needs to be a greater emphasis on method/proxy development and application.

For the revision, we will focus more on the points mentioned above and suggested by the reviewers. Although the suggested revisions are major/substantial, particularly for the introduction and discussion, with the framework developed from the referees’ comments, we believe the manuscript and its impact with be much stronger. Thank you for soliciting these useful reviews.

Best regards,

Melisa Diaz

Postdoctoral Scholar
Woods Hole Oceanographic Institution

Brent Goehring (Referee)
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Received and published: 14 August 2020

General Comments Diaz et al. present a compelling study showing the utility of combining measurements of meteoric 10Be with soluble nitrate as a means to determine surface exposure ages. In this case, they apply their new method to soils adjacent to Shackleton Glacier, Antarctica. However, their new methodology, particularly the combined use of nitrate and 10Be is not well-enough described. Additionally, and as noted below, there needs to be a rigorous uncertainty analysis completed. All that being said, I will very much enjoy seeing this paper published, but for now it needs revision. The methods and results are interesting from an applied sense in that it could be used elsewhere, but their work also adds to the glacial history of the Transantarctic Mountains. Below I present general comments and then further below I present a number of detailed comments and suggest changes.

As detailed in our response to Referee #2, we believe the manuscript will significantly benefit from the suggested re-framing. We will also greatly expand and describe our meteoric ^{10}Be and nitrate methodology, particularly regarding mobility and wetting history.

The one supplementary figure showing the relationship between max 10Be concentration and total 10Be inventory should not be buried in the supplement.

We will bring this figure into the main text.

I find that the introduction reads too much like a thesis introduction. All of the content is very good, but I think it could use a bit of streamlining that will help motivate the rest of the paper a bit better, as I think you need to also address the limitations of in situ exposure dating, as you mention later on, but it could benefit from being a bit earlier.

As per Referee #2's suggestions, we have re-framed and rewritten the introduction to focus on the original goals behind collecting and interpreting these data – to understand relative surface soil ages for biological survey purposes. We believe that with the re-framing, the manuscript be more streamlined and focused.

Bear in mind this is purely a stylistic opinion can certainly be ignored. Throughout the manuscript, anywhere there is a reference to an age, rather than a duration, need to use Ma instead of Myr.

We will make these changes to be in compliance with journal format.

There is overall a lack of uncertainty analysis that needs to be completed, particularly exploring the sensitivity of your various age determination models to parameter variance. The measurement uncertainties in this case are tiny compared to other uncertainties. A full error analysis will greatly strengthen the conclusions made in the paper and really *needs to be done before publication. A bootstrap approach should be sufficient.*

The models that we have used in this work have been described and tested in great detail in previous studies, which include sensitivity analyses (e.g. Willenbring and von Blanckenburg, 2010; Graly et al., 2010). In general, the exposure age estimates using equations 1-4 are particularly sensitive to erosion and deposition rates. Since these values could not be determined for each sampling location, we chose to refer to our ages in a relative framework. We believe this will be more evident in the revision.

There is far too much framing of the study around Pliocene glacier dynamics, and particularly the Sirius formation. I'd much prefer to see the expansion of the possible newish and important approach that can be implemented combining 10Be with nitrate as a measure of surface exposure duration.

We agree with Dr. Goehring and Referee #2. We are now focusing on estimating surface exposure ages and the use of atmospherically derived salts in estimating wetting history and exposure ages. This is detailed further in our responses to Referee #2.

Figure 8 demonstrates very nicely a coherent pattern of ice thinning/retreat. This needs to be played up, and the return late in the manuscript to the Sirius Group detracts from the novelness of the work.

We now focus on our novel approach to estimating relative exposure ages and how these data contribute to our understanding of ecological succession and glacier change.

Detailed Comments

Line 37: Please provide a citation or two for the first part of the sentence. There is actually quite sparse direct evidence for smaller interglacial extents relative to the Holocene and much is largely inferred from distal evidence or modeling. Additionally, the Ross Embayment is a large area and thus this statement is somewhat vague.

We will better clarify and support these points.

Line 51: How are calculated and estimated exposure ages any different from each other? I know this seems nit-picky, but it is somewhat strange wording as your estimated exposure age had to be calculated first.

We will expand and clarify our methodology and terminology.

Line 62: Unsure what "these studies" are. Are you referring to those cited at the end of the sentence or the sentence prior? If the sentence prior, why do you have a new set of citations?

Section 2.1 Should be worked more into the introduction in my view.

With the re-framing of this manuscript to focus more on the data present and their specific implications, much of the introduction will be re-written. We will be sure to clarify throughout.

> *Line 78: Nishiizumi et al., 2007 is not actually a half-life study, an outcome of the standardization is that a different half-life than had been used must be used. Recommend citing: â˘A´c Korschinek,*
> *G., Bergmaier, A., Faestermann, T., Gerstmann, U., Knie, K., Rugel, G., Wallner, A., Dillmann, I., Dollinger, G., Gostomski, C., Gostomski, C., Kossert, K., Maiti, M., Poutivtsev,*
> *M., Remmert, A. (2010). A new value for the half-life of 10Be by Heavy-Ion Elastic Recoil Detection and liquid scintillation counting Nuclear Instruments & Methods In Physics Research Section B-Beam Interactions With Materials And Atoms 268(2), 187 - 191. https://dx.doi.org/10.1016/j.nimb.2009.09.020 â˘A ´c Chmeleff, J., Blanckenburg, F., Blanckenburg, F., Kossert, K., Jakob, D. (2010). Determination of the 10Be halflife by multicollector ICP-MS and liquid scintillation counting Nuclear Instruments & Methods In Physics Research Section B-Beam Interactions With Materials And Atoms 268(2), 192 - 199. https://dx.doi.org/10.1016/j.nimb.2009.09.012*

We thank Dr. Goehring for the reference and will update our citations.

> *Line 101: Given the general absence of anything resembling soils or till in most of Antarctica, one could argue that applying meteoric 10Be is far more spatially limited, e.g. to regions of the*
> *Dry Valley, for example. Thus, I am not sure I would argue for your method by arguing that in situ exposure dating is limited, but instead argue that they are complementary.*

We will be sure to clarify our methodology in the revision.

> *Starting line 107: I am not sure the bedrock lithology is all that relevant. I understand you want to show the protolith for weathering products, but I think it could be said more concisely. I think the geologic setting paragraphs could be combined.*

We will make the geologic overview more concise and focus on soil properties and landscape features.

> *Line 123: Suggest changing "glacial dynamics" to "glaciers"*

We will make this change.

> *Line 128: By two samples, do you mean two surface samples? Suggest clarifying the text here, especially since you have depth profiles samples from elsewhere.*

> *Line 130: In your reference to sample distance from*
> *the glacier, are you largely referring to further away as controlled by elevation, or by horizontal distance? I think some clarification of this could be useful, as depending on*

the valley geometry, changes in ice thickness might not be significantly further away from the glacier, or vice versa. It might be more constructive and more generalizable to perhaps say that two samples were collected, one adjacent to the glacier, characteristic of times similar to the current extent and one further away representative of significant changes in glacier size (larger). A useful column in your table and the way most Antarctic glacier change is expressed is as change in ice thickness.

We will clarify and expand upon our sampling methodology.

Line 142: Why not report the fraction between 2mm and 425 microns? Was none present? Sand usually extends to 2 mm.

We set our limit to medium sand size and will clarify in the text.

Line 170: Suggest not starting paragraph with "However. . .."
I suggest that when laying out your calculation methods, that the equations flow more within the paragraph, rather than being at the end of each paragraph. I found it somewhat hard to ready.

We will re-organize this section.

Line 179: Suggest adding "any" before "have meteoric" Line 197: Delete "which"

We will make the correction.

Line 202: Confused because didn't you calculate two samples from every location, only profiles from only a few?

We measured meteoric ^{10}Be and nitrate concentrations from at least two samples (generally near glacier and furthest away) at all sites. We measured one profile at each site for nitrate and profiles from Roberts Massif, Bennett Platform, and Thanksgiving Valley for ^{10}Be. We will make this clearer.

Line 206: The lack of an expected concentration based on regressions against distance and elevation might just be spurious and making predictions from these regressions very tenuous. I suggest removing this sentence.

We will remove this sentence.

Line 222: The ages are not necessarily minimum ages, as while you may be overcorrecting for inheritance because you don't know the background inventory, *you also do not a priori know the erosion rates of the soils, even though you make assumptions. I suggest that rather than couching the ages as minimum, as they are only minimum relative to your max limiting no inheritance ages, you just present them as best estimate given knowledge of the parameters.*

We thank Dr. Goehring for the suggestion and will follow his recommendation.

Section 5.3.1 This section is very confusing in terms of what you did and is not represented in the methods at all, thus the results presented here come out of nowhere. There needs to be a clearer explanation of what was done. I think the approach is really neat and valuable, but right now it just isn't explained well-enough. I am also very confused upon the first and second read as to what was done with what profile, as the second paragraph mixes results from sites with both measurements and sites without. Section 5.3.2 Like the prior section, where there are a number of inferred methodological requirements, more expansion of the discussion is needed to aid the reader that may only have casual knowledge of meteoric 10Be knowledge as I can see many readers being most interested in the inferred ice history. I think one thing that will help immensely is that this and the prior section are more traditionally considered as part of the discussion and the results purely your 10Be and NO3- measurements. Now, if you were to present the calculation methods using nitrate and the inventory vs max concentration analyses in the methods, then you could keep in the results. At present, there is just a bit too much mixing and overall not enough time dedicated to these important sections that you then use extensively in the discussion below. Also, best I can tell Figure 8 does not show the relationship between max concentration and total inventory, please investigate, or do you mean to only present the max exposure ages.

We will reorganize these sections to present our results in a more logical manner. We will expand the nitrate and ^{10}Be methodology, which should help clarify our results and discussion. Figure 8 includes both the max exposure ages from the “inventory method” and the estimated ages using the “nitrate method”. We will make this clear.

Line 247: Please elaborate or define what the model limits are, as this is not defined. Presumably just the influence of the time scale to 10Be saturation given an erosion rate. I also wish there were different terminologies used with regards to calculated vs estimated. Perhaps refer to one as the apparent max limiting age and the other a model age?

We briefly mention that the maximum age the model can calculate is ~14 Ma and will make the model limit clear. We will also change and define our terminology for clarity.

Line 260: The correspondence with in situ ages is quite remarkable. What is lacking though is a clear representation of the two different data sets. This is why I suggested that perhaps you determine the elevation above modern ice surface and thus you can then make age vs elevation plots

for your data and the in situ data. I think will drive home much more clearly the correspondence.
Or you could consider maps showing the various bits of data, but I think they will get very busy very quickly. While the correspondence in many scenarios is striking, one thing to consider and make sure you make clear is whether the in situ data are from bedrock or from erratics, as they will have quite different exposure ages and thus your soil ages might always be older than nearby in situ erratic exposure ages. The fact that your meteoric ages, including nitrate corrected, agree so much with in situ erratic ages suggests some mechanism for resetting and flushing of 10Be or that your model is determining the pre-LGM inherited concentration quite clearly. I think this needs further discussion and is important to highlight more.

We agree with Dr. Goehring and Referee #2 that our data need to be better compared to the in-situ ages from previous studies. We will plot the previously published ages alongside ours and indicate which were sampled from moraines and boulders. We are also expanding our interpretation of the relationship between nitrate and ^{10}Be and possible implications for disturbance history.

Line 272: Need a reference for exposure dating results from Beardmore Glacier.

We will move the reference up so that it is clear.

Line 288: The arguments about the suitability seem out of place and kind of come out of nowhere and seem to set up a strawman for no apparent reason. I suggest removing and focusing on the apparent success of the nitrate correction given the good agreement with in situ exposure dating.

We will move the text regarding the suitability of nitrate as an indicator of relative surface exposure age to the introduction. We believe it is important to indicate why we chose this atmospherically-derived constituent for our study. The discussion will focus again on testing and validation of our data.

Starting line 292: The first few sentences of this paragraph read too much like a conclusions section. Suggest revision.

We will revise.

Line 303: As mentioned above, the nitrate regression models needs further description and elaboration, particularly since this really is the first major combined use of these two measures.

We will elaborate the nitrate model throughout the text.

Line 306: Wouldn't a lack of correlation be expected given the exponential fall off of a 10Be profiles, so that below a certain depth there will be little to no variance in the 10Be concentration and presumably the same in nitrate?

Yes, a lack of correlation would be expected. We will clarify our assumptions and hypotheses in the text.

> *Line 352: Suggest rather than saying delayed response that you more generalize it and just say different response from Ross Ice Shelf confluent outlet glaciers, or something to that effect.*

We will edit this text.

> *Line 358: This conclusion is spot on and is a major finding of the paper, however its use, the details, etc. are not elaborated on enough earlier in the manuscript.*

With the proposed re-structuring and re-framing, there is much more emphasis on our nitrate and meteoric ^{10}Be data.

> *Line 365: The broader question then becomes, how do we differentiate between a site with inherited meteoric 10Be that was covered by LGM ice from a site that was never covered during the LGM and more recent glaciations. This is a question that the in situ community has struggled with. We are only starting to get clarity from a focus on erratic exposure dating with long-lived nuclides or application of in situ 14C to erratics and bedrock. Recent work in the Weddell Embayment with very old erratic and bedrock in situ ages were clearly covered by LGM ice as shown by in situ 14C, including preservation of delicate features like moraines (e.g., Nichols et al., 2019). Thus, during a say 10 kyr long ice cover period, how much of a reduction in the meteoric 10Be signal can be expected? What about reduction in nitrate? Presumably unless the ice is wet based, neither will be mobilized and then you need the correct pH conditions. These thoughts are briefly touched on, but the manuscript could use a bit more elaboration on the long-term interpretation of the signal recorded by your methods and what its implications are for interpreting surface processes in Antarctica. Thus, it could be useful to elaborate on the presence of polythermal moraines, why are some areas reset for the meteoric and in situ methods.*

Dr. Goehring brings up some very important questions. However, the answer to many of these questions are unknown. Due to uncertainties with sediment transport, both modern and in the past, it is unclear how meteoric ^{10}Be and nitrate would be affected over extended periods of time. Under persistent arid conditions, we expect nitrate to be largely conserved. As stated previously, these concerns will be addressed in the revision.

> *Figure 1: Not sure if this is supposed to be this way of if some strange PDF artifact, but the exposed rock areas are banded. I also think you could make the overview map larger scale to give readers a better context of the Shackleton Glacier.*

The exposed rock areas where we samples are indeed banded, hashed, and checkered in the figure to indicate lithology as per the key. We will make the overview map larger.

Figure 3: A similar figure thinking about the fate of nitrate during ice cover would be informative.

We hope that the expanded text will suffice instead.

Figure 4: Add panel labels please. Also, it is confusing that in the Shackleton glacier map, the coloring represents concentration, but you then use the same colors for the different sites, or is it only the arrows? This is somewhat confusing, and I suggest not using colored arrows that are
the same as the color scaled points for concentration. Here the figure is trying to show too much.

We will update this figure.

Figure 5: This figure and all figures. Are uncertainties shown, but smaller than the symbol? Please note this or add uncertainties if need be.

Due to the log scale, the measurement uncertainties are small, as indicated in Table 1.

Figure 6: Suggest removing the lines connecting the points, as it implies that there is a trend in grain size % between the points. The measurements are point measurements.

We will update this figure.

Figure 7c: Please provide equations for the fits along with uncertainties on the fit parameters. These uncertainties then need to be used for error analysis on the resulting ages.

We will add these elements.

Table 2: I suggest presenting uncertainties using the same exponent for the measured value and Uncertainty.

We will update this table.

Anonymous Referee #2
Received and published: 15 August 2020
I. Summary.

The summary of this review is that the data collected in this paper are useful, interesting, and valuable to publish. In general, the idea that accumulation of atmospheric constituents in Antarctic soils is useful for estimating soil ages and residence times is important from many perspectives, including glacier change, paleoclimate, and biology, and this paper contains a lot of data that are relevant to this topic.

II. Overall motivation of paper.

II.1. The way the paper is motivated makes the experimental design look bad when, in fact, it is not.

The experimental design of this study is very well designed from the perspective of a biological survey. The use of atmospheric fallout constituents of soils to rapidly get an approximate idea of the soil age, and distinguish soils that were ice-covered during the LGM from soils that have not been ice-covered for millions of years, is a smart, well-designed approach that is likely to be effective for its intended purpose. On the other hand, the study is not well designed for the purpose of reconstructing past glacier change.

The point here is that if the present study was motivated by the original objectives of collecting geological information needed to study ecosystem succession, it would be perceived by readers as well-conceived and well-designed. If motivated as a study of glacier change as in this paper, on the other hand, the experimental design appears weak and inadequate by comparison to other studies.

I very strongly urge the authors to change this emphasis. They should clearly explain the purpose of the overall project that led them to the experimental design used here. It is true that the data collected for this purpose also have value in quantifying glacier change, so there is nothing wrong with focusing additional discussion on that later in the paper, but motivating the entire paper from this perspective makes the paper much weaker than it should be.

Referee #2 is indeed correct that the samples collected for this study and for this analysis were for a larger study on ecosystem succession following changes in climate – in this case, glacial advance and retreat. The goal of this smaller study remains the same. We sought to determine relative surface exposure ages of ice-free areas along the Shackleton Glacier. Though these data can be used in understanding glacial change, we agree that the introduction and discussion should be refocused to emphasize our broader goals and significance to ecological refugia.

II.2. The way the paper is motivated leads the paper off into vague theories that can't be addressed by the data.

The most problematic part of the paper from this perspective is the first two paragraphs of the introduction (lines 33-45) and section 2.1 ("Stability of the EAIS"), lines 55-76. The introduction discusses the fact that the Antarctic ice sheets are proposed to have been a lot smaller during some warm periods in the past. While it is certainly true that this has been hypothesized and that in a very general sense this is a strong motivation

for studying past changes in the size of the Antarctic ice sheets, there is almost no connection
between this overall idea and the specific observations described in this paper.
As discussed above, if this is the motivation for the work, the work looks inadequate.

Section 2.1 is much more problematic.
It would be clearer to simply state that it is not yet known whether or not the East Antarctic Ice Sheet was significantly smaller during past warm climates. The second problem in this section has to do with confusion between ice sheet change and climate change.
The discussion of how long polar desert conditions have prevailed in the TAM is important in
this paper because it gives context for one potential application of salt deposition in soils, i.e.
the idea of a "wetting age" in which the amount of salt that has accumulated can give information on when liquid water was last present. However, this important implication of the idea is not at all mentioned here.

We are changing the focus of the introduction to discuss ecological dispersal and refugia during glacial periods, the overall glacial history of Antarctica, the need to understand exposure ages in this region, the goals of this study to understand soil ages, and the applications both to ecology and geomorphology. We will remove the text and section(s) on East Antarctic Ice Sheet stability and instead shift the focus to persistent arid conditions, as the desert climate is particularly important for salt accumulation and the development of our nitrate proxy.

III. Oversimplified explanation of atmospherically produced Be-10.

With regard to section 2.2, the main thing the authors need to get across here is that meteoric Be-10 builds up in soils, so the total amount of Be-10 present in a soil profile is related to the age of the soil. This information is here, but it is missing some important context and mixed up with other confusing things. One, the authors should clearly state that meteoric Be-10 is mobile in the soil, so it is not the concentration at any particular location that is proportional to the exposure age, but instead the total inventory in the entire soil profile. Two, the behaviour of meteoric Be-10 and salts in soils may be quite different, for example because Be-10 remains bound to particles even when the soil is wet, whereas salts are mostly mobile in water.

While we do discuss meteoric ^{10}Be systematics later in the text, we agree that it would be beneficial to better describe the system in more detail here and expand upon salt accumulation/mobility.

The other important area here that needs to be either here or in the section on study sites is a discussion of exactly what landforms were sampled and how that relates to meteoric Be-10 systematics.

We will add a table listing on the landforms and features we sampled at each location and any notable features, such as nearby ponds, polygonal ground, etc. We will also include additional overview text in the study sites section. Mapped geomorphologic features, such as drifts and moraines, are poorly documented in this region. Though we did not focus on identifying such features, we agree that the sample location descriptions will be informative for both this study and future studies.

> *Section 4.3 is about how to quantitatively interpret Be-10 concentrations as an exposure age of the soil. This section would benefit from several improvements. Specifically, Equation (1) seems to be missing important elements.*
> *A common approach in the meteoric Be-10 literature to simplify this relationship and make it more useful is to write the governing equation for the soil inventory I (atoms per cm2, vertically integrated) instead of the concentration, like:*
> *dI/dt = Q − _I − ENs (2)*
> *where Ns is the surface concentration (atoms/g) and E is the erosion rate in mass per area units. Using this equation instead of Equation (1) would make this paper much clearer. Alternatively, this paper could simply refer to other literature that describes meteoric Be-10 systematics in detail – it is not necessary to reinvent the wheel here.*

We understand that the simplicity of Eq. 1 may be misleading. We will remove the equation and replace it with a more comprehensive equation.

> *Finally, an important point for these sites is that it is not even clear that erosion is taking place throughout the ice-free at areas all. Perhaps the only process that can bring new sediment to the surface and permit deflation would be periglacial disturbance of the soil. This issue reminds me that an important thing that needs to be added to section 3 is some discussion of the surface characteristics of each site, including presence or absence of boulder pavements and periglacial features like cracks and polygons, because these features are relevant to interpreting the Be-10 data.*
>
> *The overall point of this section is that it is not at all clear to me that erosion should even be included in the relationship between inventory and age for these sites. For this paper, I think it might make the most sense to simply relate inventory to exposure age by dI/dt = Q − _I, i.e. disregarding erosion and deposition, and accept that this approach might be either under- or over-estimating exposure ages.*

As mentioned previously, we are adding a table describing the surface features of each sample location, including whether the samples were collected on valley floors or hillslopes. While we did not sample features such as polygons and boulder pavements, it is crucial to indicate such. Once the samples are further described, we believe the inclusion of erosion rates will become more clear.

> *[T]his section has to clearly explain how one measures the Be-10 inventory. As already discussed in the paper, this can be done in two ways, either by measuring a complete depth profile and integrating, or using an empirical relation between surface*

concentration and inventory as in the Graly paper.
An additional problem with this section is that "inheritance" is not clearly defined, which is confusing.
Finally, a clear definition of "background" in the context of a depth profile is needed here. The basic concept (that the concentration is supposed to decrease with depth until you reach a depth where the concentration becomes invariant with depth) is correctly
described near line 182, but what is missing is a clear statement of how one knows that one has observed this. Overall, what I suggest doing here is noting that in principle the depth profile method is one possible way to estimate I, but it can't be used in this application because insufficient data were collected – and then move on to discussing the approach of using an empirical correlation between N and I to estimate I.

Though Referee #2 acknowledges that we have introduced and described inheritance, we will clearly define both inheritance and background in the context of our study. In our study, we provided two estimates of inheritance: 1) integrating the lowest concentration at the bottom of the depth profile and 2) an empirical correlation between surface N and I. Referee #2 correctly mentions that we have not satisfied the typically criteria for attaining background measurements of meteoric ^{10}Be using method #1. We will better emphasize the uncertainty of these calculations/estimates and focus on method #2.

IV. Data analysis.

I did not understand what the purpose of these regressions is [Fig. 5].
Because I don't see any basic physical relationship that would support linear regression of concentration against elevation/distance, as a reader I am left with the impression that the authors simply felt that there should be some linear regressions in the paper. I am not sure this is the impression that the authors want to give the reader. It makes the paper seem weak and confused, and I urge them to remove this section of the paper.

The purpose in including the regressions between meteoric ^{10}Be concentration and elevation and distance from the coast was to demonstrate that there is a geographic component to ^{10}Be concentration, probably related to glacial history. While Referee #2 correctly mentions that different drift sequences in a single sampling site would yield different ^{10}Be concentrations, we argue that the potentially different drift sequences are due to differing glacial histories. Samples at lower elevations near the glacier were likely exposed to more periglacial processes than samples collected further inland and at higher elevations. This is demonstrated in our regressions, and we will de-emphasize this section and make these points more clear in the text.

The second area that seems problematic to me in this section of the paper is how the authors approach estimating the Be-10 inventories in section 5.2.
What I suggest doing here is removing section 5.2, noting that the depth profile data do not allow estimating I accurately, and rely entirely on the empirical-correlationbetween-
I-and-N approach for estimating I, which is already clearly covered in section

5.3.2. This is not really a major substantive change to the paper, because at most of the sites there are only surface data in any case.

As stated in a previous comment, we will shift away from calculating I though integration and instead focus on our values estimated from the empirical correlation between N and I.

The third area that I think needs additional discussion in this section is the discussion of the relation between Be-10 and nitrate concentrations. To summarize, this section needs to be made much more clear so that the reader can understand when concentrations, surface concentrations, and inventories are being discussed, and what differences in behaviour of Be and NO3 could lead to positive or negative correlation. This may require making this section substantially longer in order to explain the reasoning step by step so that the reader can follow it.

We agree with Referee #2 that this section can and should be greatly expanded upon. Additional text will be added describing the relationship between ^{10}Be and nitrate for each of the three soil profiles and the factors which have likely contributed to the observed concentration behavior.

V. Discussion and interpretation areas.

The first aspect of the discussion that needs additional work is that the most basic prediction of the experimental design is that, first, Be-10 inventories and/or concentrations
should increase with distance from the ice margin at each site, and, second, Be-10 inventories/concentrations for the ice-proximal samples that are supposed to have been exposed after the LGM should have magnitudes that are appropriate to post-LGM exposure, i.e. 10-15,000 years of surface exposure.
I would do this with a figure for each site showing distance from the nearest ice margin on the x-axis, and Be-10 and NO3 concentrations on the y-axis.

We agree that an additional figure showing ^{10}Be and nitrate concentration versus distance from glacier would be beneficial in supporting the overall experimental design.

The second aspect of the discussion that is incomplete/too abbreviated is the section beginning on line 260 that compares the results to existing exposure-age data from glacially transported boulders. Personally, what I would view as minimally adequate here is a map view of each site where there are existing/published exposure age data, showing the location of the soil pits described here, the location of any moraines or drift boundaries including any hypothesized LGM ice limit, and also the location of the independent exposure-age data, which will be mostly boulders dated by some in-situ produced nuclide. Alternatively, instead of maps, these could take the form of plots with distance from the ice margin on the x-axis, and exposure ages calculated from the various data on the y-axis.

A second issue here is that some of the other exposure-age data (e.g., Thanksgiving Point, Mt. Franke) appear to be available in online databases but not yet published in *journal articles. I am sure the data are fine, but this may cause some citation problems. I refer that issue to the editors.*

Though there are only published data from Roberts Massif, we agree that it would be helpful to plot the in-situ data from previous studies and ICE-D alongside our data to support our comparisons. Confident estimates of the LGM trimline and mapped drifts for the other sites and features we sampled in the Shackleton Glacier region do not currently exist. Regarding the citations, we will cite Spector and Balco, 2020, which include the ICE-D dataset.

In addition, some of the text in this section gives the impression that the authors have a misunderstanding of the existing exposure-age data set. For example, consider the remark in line 273-ish about exposure ages from the Beardmore Glacier region, which states that exposure ages become younger downglacier for Shackleton and Beardmore Glaciers. In principle, it is possible that pre-LGM deposits are less common at low elevations, but that would have to be established via systematic mapping of these deposits. Thus, this section of the paper needs to be significantly reworked to focus on a comparison between specific mapped deposits of known or estimated ages, and not on a broad geographic analysis of a set of ages that is probably the result of selection bias.

Considering the concerns Referee #2 raised regarding this section, we have decided to largely remove it.

The third aspect of this part of the review is that I could not understand the paragraph in lines 292-302. This mixes observations that the relationship between Be-10 and NO3 concentrations in depth profiles is complicated (which is true) with statements that have no clear connection to this observation such as "through a coupled approach...we developed a useful model for estimating soil exposure ages."
I suggest starting again with this paragraph and trying to lead more clearly from observations to conclusions.

Given the overall manuscript reframing and editing of the discussion, we will improve clarity throughout.

Finally, the last important thing here is that I found the disconnect between observations and conclusions to be most serious in section 6.3 ('Implications for ice sheet dynamics.'). This section contains several very broad statements. Only one of them (the discussion of the Sirius Fm.) is clearly related to the observations.
The other conclusions here are not related to the observations, and I think this area of the paper needs work. For example, "Our data support models...suggesting that EAIS advance and retreat was not synchronous..." (line 321). The fact that higher-Be-10 concentration soils are only found at more inland sites only shows that the authors were able to locate older deposits at inland sites, but did not find them at lower-elevation sites.
The discussion around line 333 also appears oversimplified and to not take into account

basic glaciological principles. To conclude that one site has a younger exposure age than another should involve showing that the difference between measured concentrations is significantly larger than we expect based on the scatter of the data used in the concentration-inventory transfer function. My overall point is that the oversimplified nature of this discussion gives the impression that the authors have not thought very hard about this. To get from the actual observations in this paper to a conclusion about glacier change, I would expect to the following steps: first, clearly describe, map, and identify glacial deposits that have been sampled; second, show whether or not samples from the same deposits are the same age, and then, third, conclude whether or not each mapped deposit is synchronous or time-transgressive. Many of these steps are absent here.

These are all valid points. Given the other suggestions and changes throughout the manuscript, the revisions should rectify these concerns. Instead of focusing on EAIS behavior, the revised manuscript focuses on the coupling of meteoric ^{10}Be and nitrate to estimate relative ages. Since there are few, if any, data from many of the ice-free areas we sampled, we believe our data and measurements are still important. Additionally, by focusing on smaller-scale processes, we can make inferences regarding arid conditions in the CTAM. As we and Referee #2 point out, nitrate and ^{10}Be profiles should appear and behave similarly in static persistent arid conditions since both constituents are atmospherically derived. Deviations from this expected relationship can indicate wetting or possibly erosion/deposition, which have particularly important implications for ecological succession. The points will be expanded and will primarily constitute the discussion and conclusions.

VI. Suggested reorganization.

This section makes some suggestions for how I would rewrite this paper to make it better. Mainly, I suggest significantly simplifiying the paper, focusing much more on the data that were actually collected in this study and not on broader topics that may seem more important but lack a clear relation to the data, and also being much more clear on the chain of reasoning between observations and conclusions. I suggest an outline that looks like the following:

1. Begin the paper by describing why the study was designed and conducted in the way that it was – as a means of estimating surface age for biological survey purposes – and then pointing out that the purpose of this paper is to describe the soil age data, which may also be useful for understanding geomorphology and glacier change in this area. I would remove the claim in the introduction that these data are likely to provide significant information as to the stability of the Antarctic ice sheets in warm periods.

2. Describe the sample sites and the approach of sampling a likely-post-LGM and likely-pre-LGM site in each area. Discuss in detail the physical and geomorphic characteristics

of the site as well as any evidence for the mode of deposition of the parent material and also whether the soil is inflationary or deflationary.

3. Explain how meteoric Be-10 in soils works in a way that is simpler and clearer than it is in the present paper, by removing Equation 1 and focusing on the relationship between inventory and age and the need to relate concentration to inventory to make an estimate of the age from one surface sample. Explain both ways of relating N to I.

Be clear about what "inheritance" is.
4. Explain the expected relationship between Be-10 and NO3.
5. In the data analysis section, begin by establishing whether the basic premises of the study (ice-distal sites should have more Be-10, and LGM-age sites should have the amount of Be-10 expected to have accumulated since the LGM) are true. Note that the depth profile data are not adequate to estimate background concentrations, and remove this section of the discussion. After addressing the basic validation of the approach, move on to secondary questions such as whether presumed LGM-age sites have similar Be-10/NO3 inventories up and down the glacier, and differences in Be-10/NO3 inventories among pre-LGM sites.
6. Convert concentrations to exposure ages and compare these to the expected distribution
of LGM deposits as well as other exposure age data for the sites where there are some data. Use maps of these sites to clearly show the geographic relationship between your and other data.
7. With regard to the implications of these results for larger-scale issues having to do with ice sheet change during warm periods, I don't think the exposure age aspect of these results significantly changes the overall picture that previous research has derived from the existing several thousand exposure ages from Antarctica. On the other hand, the idea that salt accumulations can give some information on past warm climates (was it warm enough for liquid water to be present in soils, and if so, when?) could be very significant. Unfortunately, there is very little discussion of this in the paper. From first principles, I would expect NO3 and Be-10 to be correlated in dry soils, because both would accumulate and not be removed. But as soon as water is present and leaching of NO3 can occur, one would expect a lack of correlation. Thus, the relationship between these two soil age proxies could be quite valuable for paleoclimate. I would give this more attention in a revised paper.
In general, in rewriting this paper, I very strongly urge the authors to focus much more on the specific things that they measured and observed.

We are grateful to Referee #2 for such deep thinking and such a detailed review and have used their suggested organization as a guide for our revisions.

VII. Minor comments, by line number.

Line 37 (The WAIS has been drastically reduced in size) and line 52 (A growing body of work that suggests...susceptible....). These areas incompletely describe the evidence for ice sheet change during warm periods. There exist model simulations that show that deglaciation of very large marine-based areas of the ice sheets is possible during warm climates. These are not evidence, but hypotheses that the model simulations show are physically possible. There is some indirect evidence (e.g., marine oxygen isotope data) that, given several assumptions, may be consistent with this hypothesis, but is also consistent with the hypothesis that minimal deglaciation occurred. There is one piece of direct evidence (Be-10 in Siple Coast subglacial till; see Scherer and others) showing that the WAIS was smaller by an unknown amount sometime during the later Pleistocene. There is no direct evidence that hypothetical collapses simulated by ice sheet models took place. In fact, the best effort so far to test this hypothesis by

subglacial bedrock recovery drilling in West Antarctica (Stone and others, recent WAIS meeting abstracts describing bedrock recovery drilling at Pirrit Hills) did not show any evidence for WAIS collapse. Thus, ice sheet collapses during warm periods need to be presented as a hypothesis and not as an accepted fact.
Note that the text around line 75 is much more clear in this regard and correctly distinguishes
evidence and model predictions.

We will be sure to make these distinctions regarding WAIS stability and collapse in the revised manuscript.

Near Line 100 . The authors should not mix up evidence for sustained aridity in icefree areas with evidence for changes in the size of the ice sheet. Aridity does not necessarily require a large ice sheet, and ice sheet collapses due to marine ice margin instabilities could have occurred during cold, arid conditions. These two lines of reasoning should be kept separate.

We will make these distinctions in the revised manuscript.

Line 101-102. I did not understand these sentences.

We will revise and clarify.

Line 117. "High rates" is incorrect. Because this area is extremely arid by global standards, salt is delivered at a very low rate when compared to normal places. What is different here is not a high rate of supply but a low or zero rate of removal.

We will make this correction.

Line 122-3. This discussion gives the impression of not being well founded in glacial geological
observations. The critical difference between moraines deposited by frozenbased and wet-based ice is not their size, but rather their sedimentology. I looked at imagery of the Bennett Platform moraines and although they are large, they appear to be mostly composed of large boulders. No evidence is given in this paper that they include a fine-grained, matrix-supported till with striated clasts that would indicate formation by wet-based ice. If the authors did observe this, they should certainly describe it, with pictures, because matrix-supported tills near the ice margin in this region would be very surprising. It seems more likely that these moraines are typical boulder moraines deposited by frozen-based ice, and their anomalous size may simply be related to the supply of boulders from large overhanging cliffs.

We agree with Referee #2 and will make this correction.

Line 140-ish. I think this could be stated more clearly simply by saying "We collected surface samples at all sites and 3-sample depth profiles at three sites."

We will clarify the sampling procedure.

> *Line 198ish. Because the sites you are sampling are soils and not rocks, I don't think these rock surface erosion rates are relevant. I suggest looking at papers by Dan Morgan and Jaakko Putkonen about the Dry Valleys to get an idea of the expected range for erosion rates of unconsolidated material. However, as noted above, most of these data are from hillslopes (although not all) and it's very possible that sediment deposition, rather than erosion, is taking place at some of the sites in the present paper.*

Though it is well documented that ash layers and hillslopes have relatively high erosion rates, likely much higher than expected for soils in the CTAM, we will re-evaluate our erosion rates and overall usage.

> *line 204. What is the "coast"? It appears that the "coast" here is where the glacier flows into the ice shelf, but that makes very little sense in this context if one is thinking of the ocean as the source of salts. Open ocean is much farther away.*

Coast in this context represents the point where the glacier is no longer constrained by the TAM and flows into the ice shelf. We do not rely on distance to open ocean due to seasonal and yearly changes in this distance from sea ice extent. We will clarify in the text.

> *Line 269. The amount of time that soils are ice free must be longer for sites that are farther away from the glacier simply because of geometry. The ice sheet cannot cover more ice-distal sites unless it has already covered the ice-proximal sites. Thus, for any ice advance-retreat history, ice-distal sites will always be exposed longer. My point is that this is not a conclusion of the study (which is what this text sounds like), but it must be true under any circumstances no matter what the results.*

We agree and will clarify these points in the text.