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Earth Surface Dynamics Discussions

Interactive comment on "Relative terrestrial exposure ages inferred from meteoric ^{10}Be and NO_3^- concentrations in soils along the Shackleton Glacier, Antarctica" *by* Melisa A. Diaz et al.

Anonymous Referee #2

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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. However, I don't think the paper is ready for publication at the moment, because many sections of the paper are incomplete, have a weak relationship to what I think are the important points of the paper, or were written in too simplified or simplistic a way. Perhaps some of the

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Discussion paper

oversimplifications are only a consequence of the practice of charging open access publication fees on a per-page basis, but they are a serious problem for this paper. At the moment, this paper contains interesting and useful observations, but is not in a condition that will lead readers to understand this.

As will be immediately evident, I spent a lot of time reviewing this paper and looking at the data, again because I think the data are worthwhile and I'd like to see them published. In fact, this review may be longer than the paper. Thus, I hope the authors take this in a positive way as an effort to help the paper live up to its potential. All the issues I've noted below can be fixed – although fixing some of them will require abandoning some major parts of the paper as written – to make a good paper, and I hope the authors will do this.

One problem with this review is that my concerns with the paper mostly relate to fairly large-scale aspects of the paper organization and data analysis, and cannot be addressed with a few line edits. They will require some reorganization of the paper. Thus, the organization of this review is that I have covered what I think are the major issues in several sections at the beginning, and then at the end suggested an improved organization for the paper. A few minor comments are also added at the end.

II. Overall motivation of paper.

The overall motivation of this paper as it is written now is that the measurements are presented as having been made for the purpose of characterizing glacier change in the Shackleton Glacier area. This was a surprise to me, because most of the authors are associated with ecological and microbiological research in Antarctica and I had some recollection of hearing about this project from the authors at scientific meetings. Thus, I looked up the funding source for this project and found that my recollection was correct: the project is not focused on glacier change per se, but instead on learning about the relation between microbial ecology and soil exposure duration by investigating biological communities in soils that were and were not covered by ice during the last glacial maximum. As I understand it, this is extremely interesting: soils that are exposed for extremely long periods of time in the TAM without disturbance build up high concentrations of salts which limit the survival of microbial communities, but then on the other hand, recently disturbed soils with low salt concentrations that are more habitable were recently covered by ice. I am sure I am oversimplifying this, but the facts that the microbial communities can't survive in the old soils, and might not survive glaciation either, leads to a compelling mystery about how they recolonize and move around as the ice advances and retreats.

This research question is really interesting. In my view it is much more compelling than the motivation given in the paper to learn about glacier changes in a few places in the Transantarctic Mountains. A lot is already known about that from previous research, and there have been several other projects that are in progress or recently completed that were specifically designed to learn about past glacier change in the Shackleton Glacier area in much more detail than is possible for this study. Learning about glacier change is certainly important, but fairly routine. The refugia-and-recolonization question is much more interesting and exciting. However, it is not mentioned at all in this paper.

From the perspective of this paper, this is important for two reasons.

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. A well-designed study aimed at quantifying glacier change in some ice-marginal area from exposure-age measurements of some sort would involve geomorphic mapping of glacial deposits and determination of their relative age, followed by collection of a large number of exposure-age samples from a range of stratigraphically ordered glacial deposits in each ice-marginal area, including replicate sampling of each landform to test for inheritance and recycling effects. A good example of such a study is the Balter paper that focuses on Roberts Massif and is cited here – that study involved only one of the ice-free areas discussed in the present paper, but included extensive mapping followed by several hundred exposure age measurements, including many replicates from each landform as well as sampling from landforms with a known relative age relationship, providing many opportunities to test the assumptions of their methods. The present paper does not include any of these elements. There are also similar examples from the southern TAM, including research by Bromley and Todd at sites slightly farther away near Scott and Reedy Glaciers.

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. By extension, it causes the conclusions of the paper to appear to be based on substandard data.

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.

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

The second reason that motivating the paper as a glacier change study has negative consequences for the paper is that it leads the authors to a number of broader motivations and conclusions that are not well addressed by the data that are actually presented here. Specifically, the authors spend a lot of time in the introduction discussing the issue of East Antarctic Ice Sheet "stability." However, as written, this discussion is very vague and it is not clearly related to the actual observations. In general, if you have some broad, continental-scale hypothesis and you want to test it with a few local observations, you need some sort of clear, quantitative prediction that follows from the big hypothesis and can be tested or falsified with your observations. This sort of connection is not present in the paper. This issue does not affect my conclusion that the observations in the paper are valuable and should be published, but I think setting up the paper in this way is a poor decision by the authors that makes the paper look less valuable than it really is. In reading this paper it almost seemed that the authors did not think that their own observations were valuable or interesting, so they felt obligated to add a number of unrelated things that sounded more important. Unfortunately, this had the opposite effect for me: I felt like the authors wrote a whole bunch of checks in the introduction that they had no way to eventually cash. In the next paragraphs I'll specifically highlight the sections that I thought were overbroad and acted to reduce, rather than increase, the impact of the paper. I encourage the authors to remove all of these sections.

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. First, the use of "dynamic" in this section refers to a dispute in the Antarctic ice sheet change literature during the 1990's having to do with whether or not the EAIS collapsed during the Pliocene, which makes this section quite difficult to understand for readers who are not already familiar with the 1990's literature. 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 references in this section include both model simulations that show that the EAIS could have been smaller during warm periods and also observational studies arguing that deposits in the TAM require uninterrupted polar desert conditions since the Miocene. These two things are not comparable: the presence or absence of polar desert conditions is not a proxy for ice sheet size. 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.

To me the overall effect of this section was mainly to confuse things by introducing a vague digest of older literature without clear indications as to how it is relevant to the paper. There is no pathway for the reader to relate this background information to the study. The reader is left to think that certainly what happened to the EAIS during past warm periods is important, but more thinking will lead to the observation that there are already several thousand exposure-age measurements from around Antarctica that have not answered this question. How are the additional handful of measurements here, which are mostly more complex to interpret than the existing data because they are meteoric rather than in-situ-produced Be-10, going to help?

To summarize, it seems to me that setting up very broad questions but giving the reader no pathway for how the authors' observations are going to help answer them nearly ensures that the reader will be disappointed. In fact, when the paper gets to the conclusions, the reader is disappointed, or at least I was. Specifically, the section on line 321 "Our data support....that EAIS was not synchronous.." is particularly disappointing from this perspective. To begin with, nowhere have the authors defined "synchronous." Synchronous with what? What is the relation between "stable" and "dynamic" in the introduction and "synchronous" here? What would the observations in the paper look like if they were not "synchronous"? Again, there is no pathway for the reader to understand exactly what "synchronous" means, and how the observations here could distinguish it from the alternative (which is also not defined).

III. Oversimplified explanation of atmospherically produced Be-10.

This part of the review mostly focuses on section 2.2 and 4.3, which explain how meteoric Be-10 can be used to estimate the exposure age of a soil. Although nothing in these sections is specifically incorrect, this part of the text is hard to understand and in some areas is oversimplified, which I think later leads the authors into oversimplified or weak conclusions.

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. For example, it is not true that the purpose of measuring meteoric instead of in-situ-produced Be-10 is because quartz is absent. In all the example studies given here, plenty of quartz was present. So that is very confusing to readers. This section is also missing two critical points. 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.

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. At most of these sites one could sample from either a constructional landform deposited in an ice sheet advance, typically some kind of a moraine, or from a surface between moraines. Sediment in a moraine could be subglacially derived, would most likely have been emplaced all at once or in a short period, and could therefore be emplaced with a fairly low bulk Be-10 concentration. In this case the Be-10 concentration measured now would likely reflect the age of the moraine. Inter-moraine surfaces, on the other hand, may have been covered and uncovered by ice repeatedly, perhaps with gradual addition of small amounts of sediment each time. In this case the bulk Be-10 inventory would be unlikely to reflect the amount of time since the site was most recently uncovered, but instead the total amount of time spent ice-free during a long succession of glacial-interglacial cycles. Interpreting Be-10 data from these two sorts of sites might be quite different.

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. This equation is intended to indicate that the Be-10 concentration at the soil surface increases due to deposition (Q), and decreases because of radioactive decay (λN) and erosion. However, it is missing the equally important process of downward transport of meteoric Be-10 into the soil. Depending on the process that is moving Be-10 around in the soil, this could be quite complicated, but if you think of it as diffusion it would be a partial differential equation looking something like this:

$$\frac{dN}{dt} = Q - \lambda N - E\frac{dN}{dz} + D\frac{d^2N}{dz^2} \tag{1}$$

A complete solution for this can be potentially very complicated, especially as diffusion is probably not isotropic and also variable with depth in the soil profile. Thus, a complete advection-diffusion equation is not generally used in this application. Regardless, Equation (1) is at least incomplete and also confusing for the reader, because as it is written it does not include any process that can move Be-10 below the immediate surface layer.

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:

$$\frac{dI}{dt} = Q - \lambda I - EN_s \tag{2}$$

where N_s is the surface concentration (atoms/g) and E is the erosion rate in mass per area units. This representation also highlights the fundamental problem in interpreting single measurements of the surface concentration: relating the inventory (which is the quantity that scales monotonically with the exposure age) to the surface concentration (which may or may not always be proportional to the exposure age). 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.

Finally, an important point for these sites is that it is not even clear that erosion is taking place throughout the ice-free areas at all. In flat areas covered by unconsolidated glacial diamicts, after deflation of fine-grained material takes place (which is probably shortly after deposition) and leaves a bouldery lag covering the surface, there are really not any processes that can cause erosion. 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. In addition, if the authors observed inflationary silt layers beneath gravel pavements at any of these sites, they should make note of it – interpreting Be-10 concentrations from an inflationary layer would be quite different from an eroding matrix. In any case, observations of long-term soil erosion in the Dry Valleys are mostly on hillslopes, and there is some evidence that flat, valley-bottom areas in the lee of glacier tongues are actually sediment sinks, where fine-grained sediment removed by wind deflation from hillslopes and surrounding rock areas can accumulate. Thus, it is quite possible that the Be-10 inventory at these sites is increasing due to fine sediment deposition, not decreasing due to surface erosion. 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 $dI/dt = Q - \lambda I$, i.e. disregarding erosion and deposition, and accept that this approach might be either under- or over-estimating exposure ages.

In my view, the important things that need to be in this section, some of which are already here in part but in an incomplete way, are as follows.

First, this section has to define what the inventory is. That is Equation (2) as written, but it would be much simpler to just write that in integral form.

Second, this section has to relate the inventory to the exposure age. This needs to be accompanied by a discussion of the field evidence for or against the existence of erosional or depositional processes at the sites. The model for relating age to inventory has to be based on the physical observations of the processes that are happening at these sites.

Third, this 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. There are two possible interpretations of "inheritance" here which are quite different. First, if the site is a constructional landform that was deposited at a particular time by a glacier advance, then "inheritance" is the Be-10 that was present in the glacial sediment at the time of deposition. In this case, one would expect it to be constant with depth, and it would be the same thing as the "background" Be-10 discussed later. The second possibility applies to the situation where the same soil surface is repeatedly exposed and covered by ice during multiple glacial-interglacial cycles. In this case, the "inheritance" is not well defined. Is it the Be-10 that was there before the last period of ice cover? This would not be expected to be constant with depth, so it would not be the same as "background" and it might be impossible to distinguish from Be-10 that was deposited after the last ice retreat. Alternatively, it could be Be-10 that was deposited in the soil parent material at some long distant past time before all the ice cover events, which might be the same as "background." A final possibility, as discussed above, is that some of the soils may be inflationary, and then "inheritance" would be the Be-10 concentration in silt at the time it was added to the soil column. The point is that it is important to clearly define what "inheritance" means and whether it is or is not the same thing as "background".

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. One cannot say that it is possible to estimate the background from a depth profile unless the depth profile shows two things: a decrease in concentration between the near-surface and deep parts of the profile, and then at least two samples at the bottom of the profile that show the same concentration. In this paper, these two criteria for whether or not the background can be estimated leads to the problem that none of the depth profiles in this paper satisfy the criteria, so it is not possible to say that the background concentration has been measured for any of these profiles (also see discussion of this below). 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.

To summarize, in my opinion these sections of the paper dealing with relating Be-10 concentrations to age need to be thoroughly revised to make these five points, clearly, in order.

IV. Data analysis.

This section of the review focuses mostly on section 5 and highlights three areas that I thought were incomplete or oversimplified and need improvement.

The first one of these is the section in lines 200-209 that deals with the regressions in Figure 5. I did not understand what the purpose of these regressions is. It seems that the basic sample collection design was to go to each ice-free area and collect a pair of samples, one from a site thought to have been covered by LGM ice, and a second from a more ice-distal area that was probably not covered. Existing exposure-age data show that many areas in the TAM that are outside the LGM limit have been ice-free for hundreds of thousands to millions of years. As Be-10 concentrations should be proportional to exposure age, this implies that we should expect to see order-of-magnitude differences in surface exposure ages between pairs of samples collected from the same area – potentially within meters of each other if a pair was collected just inside and outside the LGM ice limit. In this context, I don't understand why one would want to regress Be-10 concentrations against elevation and distance from the ice shelf. Is the goal here to identify differences in fallout flux with elevation? If so, that effect would be expected to be orders of magnitude smaller than the localized variation in concentration attributable to soil age, and asking this question would not make any sense unless one could identify soils at a range of elevations that were independently known to have exactly the same exposure age.

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 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. As discussed above, none of the depth profiles collected in this paper allow a background concentration to be estimated. One profile decreases, but does not at any time stop decreasing and become constant with depth. The others start out at a high concentration and do not decrease in the interval sampled. None of these data meet criteria for identifying a background concentration. Thus, it is not possible to use the depth profile method for estimating postdepositional Be-10 inventories for any of the sites in this paper (although of course one could assume zero background and add up all the measurements in the depth profile to compute a minimum limit on the total inventory, which may be useful to compare with the empirical N-I transfer function). The implication of this is that section 5.2, as well as any age estimates based on a background subtraction, are not valid and should be removed.

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-correlation-between-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.

The third area that I think needs additional discussion in this section is the discussion of the relation between Be-10 and nitrate concentrations. Both are atmospheric constituents that are deposited in soils and accumulate over time. Thus, at first order one would expect them to be positively correlated. However, in one depth profile, they are inversely correlated (Fig. 7, lower right). Of course the reason for this is that one doesn't necessarily expect the two measurements to be correlated within a depth profile because the mobility of the two species might be different, but without further discussion here, a statement such as "we used the relationship between Be-10 and NO3 to estimate Be-10 concentrations..." (line 229) cannot make any sense. Does the relationship that is referred to have a positive or a negative slope? 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.

V. Discussion and interpretation areas.

As I read through the discussion and conclusions sections of this paper, my overall impression was that they contain a lot of statements that may well be true, but are not clearly related to the observations in the paper. This is a serious problem, because to the reader this makes it look like the discussion section is focused on a series of unsupported claims. This distracts attention from the important observational data and makes the paper look weaker than it should. I strongly encourage the authors to significantly revise this section to clearly link the observations to the conclusions, and make this section as long as it needs to be so that there is a clear chain of reasoning behind each of the conclusions. As it is, too many steps are skipped and it is not possible for readers to understand how the authors got to their conclusions.

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. As discussed earlier in this review, an initial problem here is that the authors have not clearly explained that at most of the study sites, they attempted to sample LGM and non-LGM-aged surfaces. This needs to be clearly explained earlier in the sampling section. Regardless, the discussion section should lead off with a clear explanation of whether these basic predictions are or are not satisfied. 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. I think the reader first needs to know if this basic concept works at all if they are to believe any of the additional conclusions later.

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. Because there are only three sites where both types of data exist, this shouldn't be too hard, and without this, there is really no way for the reader to figure out whether or not the claim that the data are consistent is at all justified. 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.

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. This is misleading, because in both situations different glacial deposits have been dated at different locations along the glacier. At Beardmore, LGM-age glacial deposits have been dated at several places along the glacier, but pre-LGM deposits have only been mapped or dated at one site at the top of the glacier. If site selection had been the opposite, i.e., past researchers had targeted pre-LGM deposits near the ice shelf and post-LGM deposits at the top of the glacier, the age relationship would also be the opposite. In other words, the distribution of ages reflects selection bias by researchers and cannot be used by itself to establish the existence of deposits of different ages, or any variation in the ages of particular deposits. Thus, this section of the present paper gives the impression that the authors have an overly simplistic understanding of this situation. 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.

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 don't understand the connection between these two statements and others in this paragraph. I suggest starting again with this paragraph and trying to lead more clearly from observations to conclusions.

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. This observation is interesting, but unfortunately doesn't help very much with the age of the Sirius as a whole because it appears to be a loose clast of the Sirius that overlies the soil. Although these data show that the clast cannot have been dropped at this site until after 14 Ma, that doesn't directly constrain the age of the source material.

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). This is not correct. First of all, nowhere does the paper define what "synchronous" means and what the expected results of this study would be for "synchronous" and "asynchronous" options. Second, to me, the fact that old ages were not observed close to the ice margin at coastal locations simply indicates that the glacier thickens more nearer the grounding line, which is expected from basic glaciological principles and is not in conflict with a model in which thickening occurs at the same time everywhere. 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. As noted above, there is no reason that there should be any relation between location and Be-10 concentration unless we know we are sampling a deposit of the same age at all sites. Therefore, a sample that diverges from this relationship also has unclear significance. The simplest explanation for this sample is just that you sampled a younger deposit. Whether that has any significance depends on the distribution of the deposits – are multiple deposits present at all sites? The other important point here is that relating the Be-10 concentration in surface samples to exposure ages relies on an empirical concentration-inventory relationship, which is quite scattered and not expected to be exact. Note that the relevant figure in Graly (2010) is on a log-log plot and displays quite a lot of scatter. Thus, even given a number of sites in the same deposit that were deposited at the same time and have the same vertically integrated inventory, significant variations in the surface Be-10 concentration are expected to occur. 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.

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. As discussed above, I got the very strong sense in reading this paper that the authors were unsure whether or not their data set would be perceived as a significant contribution by itself, so they felt like they had to add discussion of larger-scale issues about Antarctic ice sheet change to increase the perceived interest of the paper. I think this does the paper a disservice. The observations in this paper are, in fact, relevant and of interest by themselves. There are not enough data in this paper to solve any major problems having to do with glacial chronology that have not been solved by the much larger data set of pre-existing exposure-age data. However, the observations here are very valuable in generally understanding the residence time and disturbance frequency of Antarctic soils, which is necessary both for the original ecological-succession aspect of this study and also to make exposure-dating and soil development studies better in the future. The observations in this paper can stand by themselves without the need to bring in broader, but largely unrelated motivations.

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.

Near Line 100 . The authors should not mix up evidence for sustained aridity in ice-free 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.

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

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.

Line 122-3. This discussion gives the impression of not being well founded in glacial-geological observations. The critical difference between moraines deposited by frozen-based 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.

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."

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.

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.

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.

Interactive comment on Earth Surf. Dynam. Discuss., https://doi.org/10.5194/esurf-2020-50, 2020.