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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.

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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 re-

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sults 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.

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

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. 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.

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.

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. 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.

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.

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.

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.

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: - 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

- Chmeleff, J., Blanckenburg, F., Blanckenburg, F., Kossert, K., Jakob, D. (2010). Determination of the 10Be half-life 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

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.

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.

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

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.

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

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.

Line 179: Suggest adding "any" before "have meteoric"

Line 197: Delete "which"

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

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.

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.

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.

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?

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.

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

Line 276: Unclear if you are referring to your ages or in situ exposure ages. Please clarify.

Line 280: Need to insert "to" after "first"

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

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. 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?

Line 313: Missing "was"

Line 327: The referencing choices are confusing. Are you referring to the start timing of the last glacial cycle and thus referring the Blunier and Brook and the other refs?

Line 340: Unclear as to which exposure age you are referring to. Bennett Platform?

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.

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.

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

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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.

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.

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

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.

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

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.

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.

Table 2: I suggest presenting uncertainties using the same exponent for the measured

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value and uncertainty.

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