Response to reviewer #1 (Jane Andersen).

We thank Jane Andersen for her detailed review and comments. We appreciate and acknowledge Jane's in-depth modelling of our data sets using a mixing depth model and the suggestions she provides based on her analyses.

Our responses to each comment are shown below in red.

Review of: Antiphase dynamics between cold-based glaciers in the Antarctic Dry Valleys region and ice extent in the Ross Sea during MIS 5

by J. Anderson et al., The Cryosphere, March 2023

General comments

This is a very well written paper; I was especially impressed with the Introduction, 'Geologic setting and study area', and Methods sections. I find the potential antiphase dynamics between glacier and ice sheet extent very interesting and important to understand better. It is also fascinating that you find vertical mixing down to a depth of 70 cm in the Pearse valley depth profile.

Thank you

However, I have the following concerns that I think needs to be addressed before publication.

1. Robustness of main result regarding antiphase dynamics. You present 10Be26Al ages from only three samples of which one clearly has a large component of inheritance. I think you need to discuss the risk of inheritance in the remaining two samples and justify better why you trust these ages.

The cobble sample PV14-CS3-P2 displays minimal edge rounding which suggests negligible erosion and is unlikely to be much older than the zero-erosion age. Given inheritance is stochastic, we infer that the two lowest consistent ages represent min inheritance and hence we take them to be our best estimate to represent the 'zero-erosion exposure ages' for the deposit of the cobbles that overlie the permafrost.

The ^{10}Be concentrations of the cobbles (5 - 12 x 10^5) are significantly lower – an order of magnitude – than the ^{10}Be concentrations of the shallow/surface permafrost sediments (~4 x 10^6). This gives us an additional piece of evidence that the cobble samples belong to a depositional event that post-dates the permafrost sediments and provides some confidence that the boulders are unlikely to have experienced a complex exposure history.

2. Cosmogenic depth profile modelling. I think this part of the manuscript is problematic for several reasons.

a. Firstly, I think you need to demonstrate that you are aware that there is a difference in the expected cosmogenic profiles between scenarios of surface mixing with and without erosion (See Fig. 8 in Granger and Riebe, 2014; also shown in Fig. 4.24 in Dunai 2010). In your fig. 6D you show a scenario that looks comparable to mixing without erosion, although you suggest that your depth profiles are a result of both mixing and erosion. Depending on how long the sediment has been exposed, the erosion may not have gone to steady state, which would lead to an intermediate scenario between the two end-member scenarios shown in Granger and Riebe, 2014. I think you should illustrate and discuss this issue.

We agree that there may be a number of different scenarios from our depth profile. We will clarify this and include an intermediate scenario in Fig. 6 and in the text. Also please see our expanded and detailed reply to reviewer #2 (Greg Balco) with respect to depth profile modelling with and without a mixing term.

b. Secondly, I am skeptical about your interpretation that the Pearse valley profile is a result of two depositional events. You seem to base this mostly on your cosmogenic profile modelling, without much evidence from the stratigraphy (except for the ice lenses).

We interpret the two depositional events from the distinct offset at 2.09 m depth in the Pearse Valley profile. This is not based on depth profile modelling. We believe this offset is real and not due to analytical uncertainties or processes because both ^{26}Al and ^{10}Be show the same positive offset as a function of depth. (i.e., they represent an age-depth inversion reminiscent of a two-step depositional history).

This is problematic because (i) your model does not include mixing (and therefore do not capture the result of combined mixing-and-erosion), (ii) modelling including mixing does a reasonably good job of matching most measured nuclide concentrations (See Figs. A and B below), and (iii) the inherited components of the measured cosmogenic nuclide concentrations have nearly the same concentrations and ratios (as shown in your Fig. 12a) –

First, we again thank Jane (reviewer #1) for doing depth profile analyses of our data using her model.

Second, we agree that our data analysis and depth profile modelling does not include an active-layer mixing component which as Jane points out, has the potential to better constrain the evolution of the permafrost core. With that said, we are not convinced it is necessary to include a mixing model (see our detailed reply to the same comment made by reviewer #2 (Greg Balco). As noted by Greg, and to a degree by Jane in her review here, the mixing model of Andersen / Knudsen does not result in a conflicting or challenging interpretation of the data from what we arrive at using a non-mixing depth profile model of Hidy et al. (2018). We note here our detailed reply to reviewer #2 on this item.

In summary, introducing a mixing model to our depth profiles we believe does not improve the age-inheritance-erosion constraints we arrive at nor resolve the complexity of these data, and hence does not alter our conclusions.

How likely is this outcome if there were two separate events? I think the simplest explanation is that there has been one depositional event leading to nearly (but not completely) homogenous initial nuclide concentrations that has subsequently undergone exposure, erosion and mixing. If you disagree, I think you need to further bolster your arguments for why this is not the case.

We agree that the initial state is a well-mixed sediment profile – the corollary is a constant inheritance component at all depths. On the assumption that all samples have maintained their relative depths to each other, a single depositional event is the most likely and simplest outcome. However, given a distinct increase in nuclide concentration at 2.09 m depth for both ^{10}Be and ^{26}Al, relative to the samples between 1.09 – 1.65 m depth, and the presence of ice lenses between 1.57 – 1.87 m depth, we chose to interpret the deeper core section to signal an earlier depositional event. We will provide further clarification of this in the text.

3. Annual and inter-annual variability of active layer depth. You assess the present-day active layer thickness based on the depth of ice-cemented sediments in your excavation (37 cm in Pearse Valley; a snapshot in time) and state that the ice-lenses (73-86 cm in PV) represent a paleo-sublimation unconformity. I think you need to argue (based on literature) that this difference cannot be explained by annual/interannual variability of the active layer thickness, to justify why your interpreted 'paleosublimation unconformity' is not just a result of this variability.

We cannot rule out that the paleosublimation unconformity is a result of a warm summer. However, most of the coastal thaw zone rarely exceeds >50 cm depth of the active layer in Taylor Valley. As Pearse Valley is further inland and at higher elevation this seems less likely. We will acknowledge the possibility of this in the text.

Figure A. Inverse modelling of the Pearse valley profile using a revised version of the model used in Andersen et al., 2018 and described in more detail in Knudsen et al., 2019. The top eight panels show the six model parameters (grey dots show burn-in phase and rejected models, colored dots show accepted models colored by walker number) and the pairwise tradeoff between four of the sample parameters colored by the residual to the measured 10Be and 26Al concentrations (lower residual = better fit). Note that there is a trade-off between mixing depth (parameter 'dm' in eq. 4 in Knudsen et al. 2019 supplement) and mixing rate (m2/yr), with higher rates/lower depths leading to slightly better fits/lower residuals and correspondingly to a more abrupt 'step' in the profile between the mixed and unmixed zone. The three bottom panels show the measured 10Be and 26Al concentrations and the 26Al/10Be ratio as a function of depth with 1 sigma error bars (light grey samples with only one nuclide measured were not used for inversion). The grey patch/lines in background show accepted models and the red line the model with lowest residual/best fit to observations. No model matches all measured nuclide concentrations, but that is perhaps reasonable given the assumption about completely uniform inherited nuclide concentrations at t=0? This is not a perfect inversion and the production parameters are slightly different than in your manuscript, but based on this experiment I would encourage you to i) include mixing in your model and ii) reconsider why you think two-stage deposition is necessary to explain your nuclide concentrations.

Figure B. Same inversion as above, but here the top panels show the distributions of parameter values for accepted models and the trade-offs between surface exposure duration and inheritance while the lower panel shows the range in exhumation histories compatible with the measured nuclide concentrations.

See our detailed replies above and to reviewer #2.

Specific comments

Section 4.2 and figures 8-10: I found the presentation of the modelling results in this section hard to follow. Firstly, if you decide to model several different scenarios, I think you need to make it much clearer in the text and figures when you are presenting and discussing what scenario. At present, the figure captions for example do not explain what model/scenario you are using, and there is no clear break in the text before you introduce your second scenario/model (the mid of line 450). It may be helpful to give the different scenarios descriptive names such as 'Scenario 1: Single depositional event, no vertical mixing' etc.

We will label and explain scenarios more clearly in the figures and text.

Secondly, I don't think the link between ice lenses in the stratigraphy and your proposed breaks in the sedimentary sequence is convincingly presented. It would be helpful to have the sedimentary 'logs' (Fig. 5) repeated next to the cosmogenic depth profiles to make it easier to compare, or alternatively indicate the key features (ice-lenses, transition between cemented and loose sediments) on/below/adjacent to the cosmogenic depth-profile.

We will show the logs and key features next to the depth profile data.

Thirdly, the top icelenses (~73-86 cm) are associated with the bottom of the (paleo-)active layer. If the lower set of ice-lenses (157-187 cm) indicate the bottom of a former active layer of comparable depth this would presumably imply ~0.5-1 m of erosion prior to deposition of the upper sediment package. This is not clear from the current description.

In the text we do not suggest the lower set of ice lenses are necessarily a paleosublimation unconformity like the top ice lenses. We will include this possible scenario in the text and discuss such an erosion scenario.

Finally, I think you need to be clearer about how you assess the model performance, especially since you don't include mixing which inherently lead to a poor fit to the top (vertically mixed) samples.

We will clarify this in the text. We will provide further detail assessing the model performance.

I was intrigued to see that the cosmogenic profiles indicate rapid mixing down to ~70 cm depth in Pearse Valley. I would like to see a short discussion of what process can cause sediment mixing in these coarse-grained (not very frost-susceptible?) sediments in this climate?

The active-layer is susceptible to cryoturbation during summer months. Gravimetric water content is relatively high in near-surface permafrost in the Dry Valleys. See Fig 2e in Lacelle et al. (2022). We will include a short discussion about these processes.

L. 532-534: Could the 'offset' in 10Be concentrations be a result of mixing? Why do you not see an offset in 26Al-concentrations? Does this indicate that the measurement errors are underestimated?

The offset is a result of mixing. The paleoactive-layer above this depth was vertically mixed. We will clarify this in the text.

Regarding the offset in ^{26}Al concentrations, the averaging of the first five data points (the average mixed layer concentration) in both profiles up to and including 0.65 m, is distinctly different for the next lower data point below the 0.65 m depth.

Technical corrections

L. 82: Indicate location of Allan Hills BIA on map? Agree

L. 92: Are the glacier advances in phase with ocean warming rather than out-of-phase? We will correct this

L. 99-100: Can you add Arena and Kennar Valley on map? Agree

L. 207-208: Add '(blue circle)' after first mention of drill site PV14-A and delete the second mention Agree

L. 259-260: Did you also drill through the loose sand/gravel section at the top or did you dig? The top of the section was collected via a mixture of digging and coring. The initial coring was collected in whirl-pak bags as the coring quality was poor, and largely came out as loose material. We will clarify this.

L. 289-290 compared to L. 295-296. How can the sediments be ice-cemented but also loose? And would it be worthwhile indicating this (and the maybe also the laminae) on the sketch (Fig. 5)? The active-layer (0 – 28 cm) above the ice cemented permafrost consists of a thin armoured surface layer (2 cm) and a layer of loose sand and pebbles (26 cm). Below 28 cm is ice-cemented. We will clarify the sentence so it is easier to follow. This is already outlined from the key in Fig. 5. We will include laminae in Fig. 5.

L. 293: You give an interpretation of the sediment deposition environment in Lower Wright valley, but not for Pearse Valley above. We will include our interpretation for Pearse Valley.

L. 319: Check spelling of 'concentration' Agree

L. 327-333: It is nice that you describe the error propagation in detail! Thank you

L. 373-374: What is the effect of ice lenses and the mix of open and ice-cemented porosity on this estimate? The difference between bulk density for loose sediment, and ice cemented-permafrost is largely within the +/- 0.1 uncertainty. All ice lenses were less than 10 cm thick and, in most cases, less than 5 cm thick. We will acknowledge the small difference this assumption could have on the overall models.

Line 404: I would suggest that you illustrate the effect of mixing with/without erosion and add a panel F to this figure showing the resulting cosmogenic profiles corresponding to scenario E. We will add a panel F showing erosion.

L. 419-420: Explain what you mean by 'simple' and 'complex' exposure history, this is not clear. Also, one sample indicate up to ~900 ka burial according to the diagram in Fig. 7, do you still consider this simple? Simple assumes a simple constant exposure. Complex suggests at least one episode of burial. We will clarify this in the text. The sample showing up to ~900 ka burial has a higher probability of being a non-simple exospore history. We will distinguish between that sample and the other two in the revised manuscript.

L. 433: Are the 10Be concentrations normalized by production rate (x-axes on Figs. 7 and 12)?

No, our concentrations are not normalised. We will clarify this in the text.

L. 443-444: How do you assess the model performance here? Do you calculate a residual?

We didn't calculate residuals, but from the model outputs the model fits within the 1-sigma measurement error bars for each sample within the deeper depth range, and they do not fit within 1-sigma for the shallow depth range. We will include a calculation of the residuals in the revised version over the two depth ranges to support this statement more quantitatively.

L. 469: 'depositional age of the permafrost': permafrost is not deposited, sediments are? Agree. This will be corrected to 'depositional age of the sediment'

L. 501: I don't think the lower panel is useful. Specify that you are talking about 'exposure age' on axis label. Exposure age will be shown on both plots

L. 526-527. Here I think you need to discuss how certain you are about the present-day active layer thickness/on what time-scales it is expected to vary. The present-day active layer fluctuates throughout summer months. We will include further discussion regarding active layer migration over summer months.

L. 533-534: Does an unconformity ‘occur’? No we will change this to ‘formed’

L. 538-539: “The higher nuclide concentrations in these samples” is a bit misleading – the concentrations are lower than in the upper part of the section. We will clarify this sentence to state: The increase in nuclide concentration >2.09 m depth relative to the samples between 1.09 - 1.65 m depth.

L. 590: spell out Acc. in caption and axes label. Agree

L. 698: Check spelling of ‘inheritance’ in reference. Agree