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. 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 ^{10}Be-^{26}Al 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.
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.
    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). 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) – 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.
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/inter-annual variability of the active layer thickness, to justify why your interpreted 'paleosublimation unconformity' is not just a result of this variability.

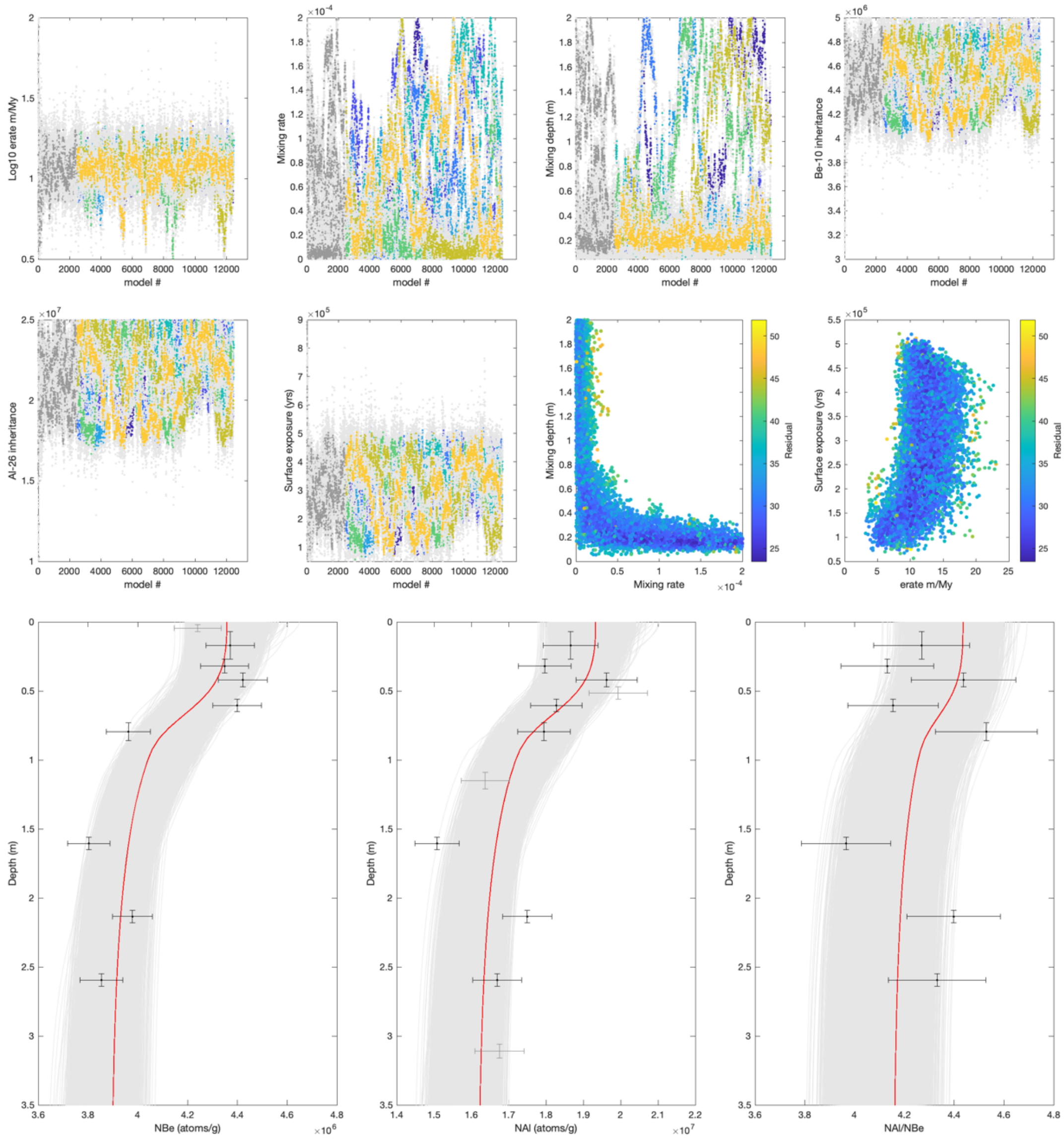

Figure A. Inverse modelling of the Pearse valley profile using a revised version of the model used in Andersen et al., 2018 and described in more detail in Knudsen et al., 2019. The top eight panels show the six model parameters (grey dots show burn-in phase and rejected models, colored dots show accepted models colored by walker number) and the pairwise trade-off between four of the sample parameters colored by the residual to the measured ^{10}Be and ^{26}Al concentrations (lower residual = better fit). Note that there is a trade-off between mixing depth (parameter 'd_m' 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 ^{10}Be and ^{26}Al concentrations and the ^{26}Al/^{10}Be 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.

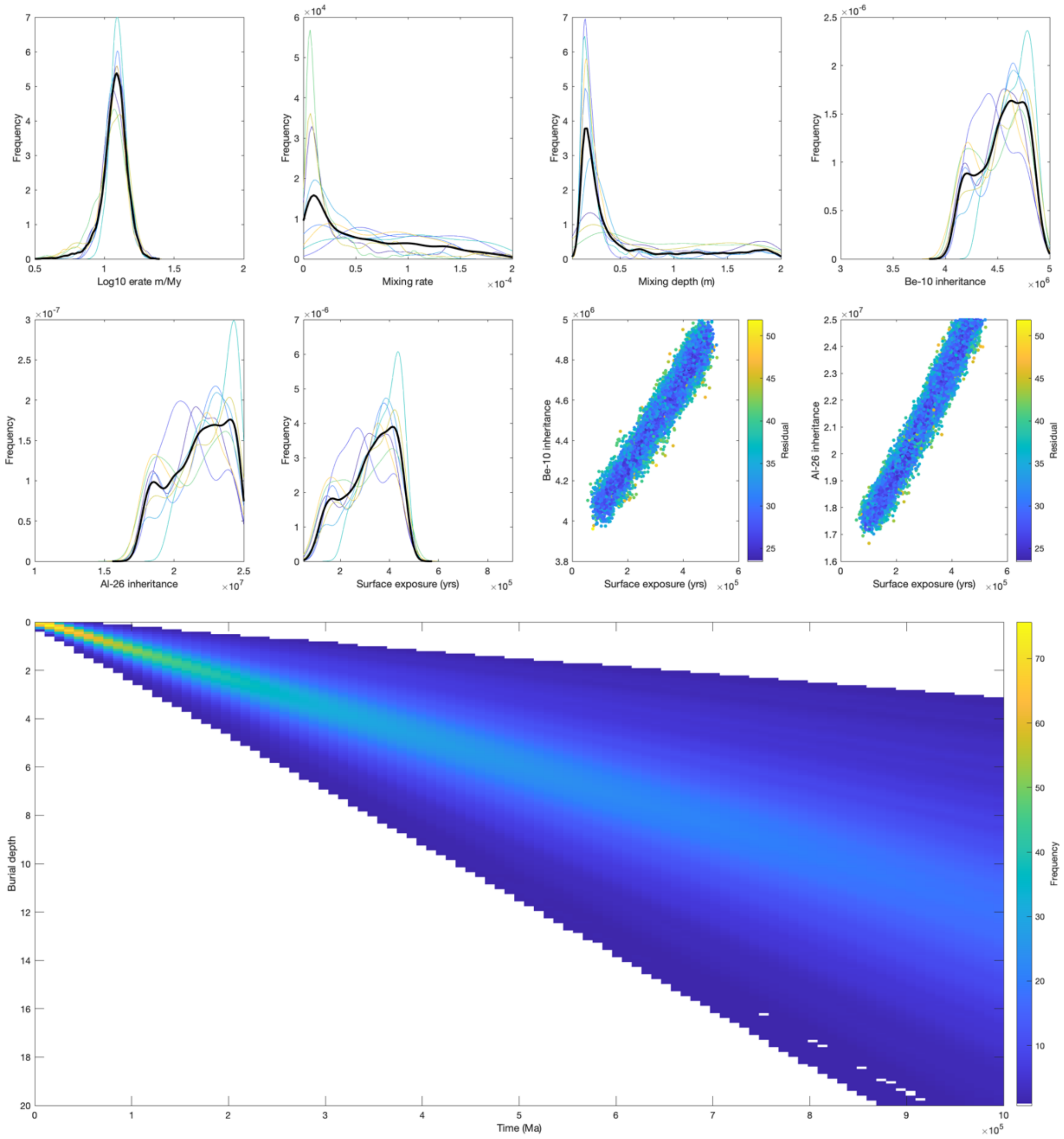

Figure B. Same inversion as above, but here the top panels show the distributions of parameter values for accepted models and the trade-offs between surface exposure duration and inheritance while the lower panel shows the range in exhumation histories compatible with the measured nuclide concentrations.

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. 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. Thirdly, the top ice-lenses (~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. 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.

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?

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?

Technical corrections

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

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

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

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

L. 259-260: Did you also drill through the loose sand/gravel section at the top or did you dig?

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)?

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

L. 319: Check spelling of ‘concentration’

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

L. 373-374: What is the effect of ice lenses and the mix of open and ice-cemented porosity on this estimate?

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.

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?

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

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

L. 469: ‘depositional age of the permafrost’: permafrost is not deposited, sediments are?

L. 501: I don’t think the lower panel is useful. Specify that you are talking about ‘exposure age’ on axis label.

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.

L. 533-534: Does an unconformity ‘occur’?

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.

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

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

References

Andersen, J. L., Egholm, D. L., Knudsen, M. F., Linge, H., Jansen, J. D., Goodfellow, B. W., ... & Fredin, O. (2018). Pleistocene evolution of a Scandinavian plateau landscape. Journal of Geophysical Research: Earth Surface, 123(12), 3370-3387.

Dunai, T. J. (2010). *Cosmogenic nuclides: principles, concepts and applications in the earth surface sciences*. Cambridge University Press.

Granger, D. E., & Riebe, C. S. (2014). Cosmogenic nuclides in weathering and erosion. *Treatise on geochemistry 2nd edition*, 401-436.

Knudsen, M. F., Egholm, D. L., & Jansen, J. D. (2019). Time-integrating cosmogenic nuclide inventories under the influence of variable erosion, exposure, and sediment mixing. *Quaternary Geochronology*, *51*, 110-119.