Response to reviewer #2 (Greg Balco).

We thank Greg Balco for his detailed review and comments. Our responses to each comment are shown below in red.

The summary of this review is that this is a straightforward paper that reports a collection of useful cosmogenic-nuclide data from the Antarctic Dry Valleys. From this perspective the paper is perfectly fine and communicates these data in a clear way. Thank you

There is one thing that is surprising about this paper, though, which is that the clear description of the cosmogenic-nuclide data is bookended by an extraordinarily large amount of discussion of the relationship of glacier advances to climate in the DV. The proposed relationships are very likely to be true, but are almost entirely unrelated to the new observations in this paper. To put this in perspective, this paper reports cosmogenic-nuclide measurements on 27 samples -- three surface samples and the remainder from cores in frozen sediment. Of the 3 surface samples, only two have the same age. These two ages are consistent with the generally accepted and well-described-in-the-literature concept that Dry Valleys glaciers are moisture-starved and retreat during cold periods. Thus, 2/27 (7.5%) of the data described in this paper can be used to support this assertion. However, 100% of the title, about 2/3 of the abstract, most of the introduction, a very long discussion in section 5.4, and about 2/3 of the conclusions are devoted to the antiphase-with-climate behaviour of Dry Valleys glaciers. Thus, nearly all of the introduction, discussion, and conclusions of the paper is all about only 7.5% of the data.
We appreciate the remark of the imbalance in the direction of the paper with respect to sample inventory and accept the need to address it. See below.

The data from the other 92.5% of the samples, which are depth profile measurements, are quite interesting, but complicated and not easily interpretable as an age of any particular event. They are certainly consistent with glaciers retreating during glacial maxima, but it is clear that the measured nuclide concentrations record a complex history of exposure, burial, sediment recycling, and permafrost processes, so they would be equally well consistent with all kinds of other glacier change scenarios. These data are not really about glacier change, they are more about the provenance and process environment of frozen sediments in the DV. It appears that the authors thought that these data were too complicated, and permafrost dynamics too esoteric, for anyone to care about, so they wrote the entire paper about the two surface samples and then included the depth profiles because they were from about the same place.

In our response, we note that considerable effort was invested in modelling the permafrost depth profiles and the relevancy of the data to glacial and sedimentary processes in the Dry Valleys. We will expand on our interpretation and significance of the depth profiles.

This is not to say that there's not a good description of the depth profile data in this paper -- there is -- but it is just kind of abandoned among all the discussion of antiphase glacier dynamics, which, as noted, is certainly correct, but already pretty well established by other research and only marginally relevant to the majority of the data reported here.

Overall, we agree with the reviewers' comments with respect to the majority of data presented not being directly related to glacier advance / retreat. We will adjust the title, abstract, introduction, discussion, and conclusions to include greater emphasis on permafrost processes, and sediment recycling.

Of course if I were writing this I would have titled it "Timescale of active layer mixing in Antarctic permafrost inferred from cosmogenic-nuclide depth profiles," spent most of the paper talking about that, and then just tossed in a sentence at the end that, oh yeah, we measured three surface clasts, of which two ages agree and are consistent with the generally accepted thinking that DV glaciers retreated during the LGM.

We will modify the title to reflect both permafrost processes and antiphase dynamics of Taylor Glacier.

New proposed title: *Antarctic permafrost processes and antiphase dynamics of cold-based glaciers in the McMurdo Dry Valleys inferred from ^{10}Be and ^{26}Al cosmogenic-nuclides.*

But, I'm not writing this, so the authors can do whatever they want. All the data are clearly reported here, so readers can take from it what they choose.

However, even though the authors don't seem very interested in highlighting the depth-profile results, I am going to only talk about them in the rest of the review. Basically, what happens here is that the data clearly show that the concentration-depth relationship does not show the exponential-like decrease expected for a sediment/soil unit that is either stable or eroding without vertical mixing. A surface mixed layer is clearly evident in both profiles. However, despite this observation, the authors begin by trying to fit the data with a model that does not include a mixed layer (Hidy, various refs.). Frankly, this does not make a ton of sense. Why is this even included? Eventually at the end of this section, the authors come to the conclusion that "the depth profile model does not work well for non-attenuating profiles." Of course it doesn't! It's not supposed to. Thus, I would remove this entire initial fitting exercise.

We presented the initial model to demonstrate, as pointed out by the reviewer, that it does not fit our data – as one would expect - given there is a surface mixed layer. However, it does provide a reasonable or perhaps a sufficient fit at mid to lower depths and as such gives some direction in interpreting the evolution of the permafrost deposit.

We will reduce the text in this section and highlight the limitations in depth models for complex data sets as an instructive exercise for those less familiar in this field.

In the next fitting exercise, the authors average all the data from the apparent mixed layer into one mixed pseudo-sample, and then try to fit the same model. As expected, this works better, but this is also a surprising thing to do, because, basically, you still have a data set that shows clearly that the process of mixing is happening, but you are trying to fit it with a model that doesn't include that process. Of course you can kind of fit it with this approach, but it's not right because it doesn't capture the effect that the production rate in the entire mixed layer is not the same as the production rate at the average depth in the mixed layer, or the effect that mixing and erosion are happening at the same time, if that makes any sense. So, this is a little better in terms of fitting the data, but kind of what has happened here is the authors have tortured the data to try to fit them with a model they know to be wrong. This could be done better.

The other review (Jane Lund Anderson) discusses this at length and applies a much more sophisticated model to fit the data. As expected, this does a much better job of fitting the data and also highlights the tradeoff between various parameters (like exposure age and inheritance) that makes it basically impossible to assign a definitive age to this deposit based on the depth-profile data. It should also be noted, however, that the results of this model simulation don't actually account for the fact that the site in Pearse Valley is episodically covered by ice, and, presumably, vertical mixing also stops during these periods. So even the large range of exposure ages permitted by these results might not cover all possible scenarios. As an aside that's not really related to this paper, the Andersen/Knudsen model doesn't do a good job of reproducing the sharp bottom of a mixed layer that is often observed in data (although it's not designed to).

As we outlined in the response to reviewer #1 - We are not convinced it is necessary to include a mixing model, nor do we believe that in this case, it solves the problem uniquely, or even sufficiently, for age, inheritance, and surface erosion. As reviewer #1's (JA's) model demonstrates, and as repeated by this reviewer, it appears not to result in a more restricted constraint on age or erosion rate.

We acknowledge a mixing model can provide an improved interpretation among many possible scenarios, and we will expand upon this in our discussion – but importantly, in this case, not one contradictory or as a challenge to the one we have concluded for the Pearse Valley permafrost evolution in the absence of a mixing model.

In continuously vertically mixed soils where radionuclide decay is negligible (such as those in the Dry Valleys), the average production rate in the mixed layer is constant with depth (Granger and Riebe, 2014). Under these conditions, the mixed layer in our permafrost soils will reflect the spatially averaged cosmogenic nuclide concentration. Surface erosion or changes in active layer thickness will modify this outcome, however the Antarctic environment supports extremely low erosion rates and hence the age-erosion trade off as mentioned by reviewer #2 may not be severe. In summary assuming the quartz grains in a given deposit are well mixed, on a time scale shorter than the ^{26}Al half-life, the steady state final nuclide concentration should be constant at all depths which is what we observe in the surface mixed layer at Pearse Valley. Thus, a depth model fit, without a mixing component, commencing with a calculated mean concentration at mid-depth within the upper 65 cm i.e., the surface mixed layer provides a reasonable attempt at data analysis.

In addition, we note that reviewer #2 (GB) highlighted the fact that the mixing model output of reviewer #1's (JA) model does not include the effect of episodic ice-cover, which could vary age, and erosion results more than the wide-range of reviewer #1's model output.

Overall, however, the important thing is that even though the more sophisticated model provides an age estimate, it's also dependent on assumptions about what happened re. ice cover, etc. So the fact is that these data are not going to provide an accurate age estimate for any particular event.

We agree with this, though the data can still provide useful age information.

One thing that would have intermediate complexity and possibly be helpful in interpreting these data would be to try to fit the data with the mixed layer model of Lal and Chen (2005). This is fairly simplified (the parameters are exposure time, erosion rate, and mixed layer depth), but it would be helpful to see what range of erosion rates and exposure times could be consistent with the data. Of course this doesn't include episodic burial by ice either. If the authors try this they should note that there is a typo in Equation 12 of Lal and Chen -- it is missing a factor of rho in one of the terms.

We agree there are many different scenarios. As this doesn't include the episodic burial by ice it also does not solve the complexity of this data and is unlikely to change our results or conclusions.

I am also skeptical of the "paleosublimation unconformity." Certainly this could be true, but there is no strong evidence that it is. We will provide additional discussion on this.

What these data do provide, however, is some interesting information about soil/sediment mixing in these areas, which is something that is not widely studied or understood. Thank you. We agree that this data set provides an interesting perspective on permafrost sediments and processes that are not well understood. We will emphasize this in the updated manuscript.

Like the first reviewer, I am really interested to see very thorough mixing in the upper 70 cm in the Pearse Valley core. That's not necessarily expected. So in my view these data are extremely unhelpful as to age, but quite interesting as to process. If I were writing this, I'd probably do this as follows:

As we noted above, we agree with the reviewers' comments with respect to the majority of data presented not being directly related to glacier advance / retreat and will expand on permafrost processes, and sediment recycling. We agree that the entire profile is not useful for age, but we stand by our observation that depth concentration offsets do demonstrate several deposition / mixing events, which can be associated with relative age, and therefore permafrost stability.

1. Note the main features of the Pearse Valley depth profile data, which are (i) there is a surface mixed layer; (ii) there is a high inherited concentration at depth, and (iii) the 26/10 ratio indicates a long history of exposure and burial. Noted

2. Note the main feature of the Wright Valley profile, which is that it has the same concentration at all depths. Agree

3. Apply a simple calculation to determine the "age" of the mixed layer at Pearse by (i) computing excess Be-10 and Al-26 with respect to the inheritance at the bottom of the cores, and (ii) dividing by the production rate to get an age. For Pearse Valley, there is about 5e5 atoms/g excess Be-10 in the mixed layer, and the production rate in the mixed layer looks to be about 5 atoms/g/yr, which suggests that order 100 ka is needed to produce these data. Looks like you get a similar order of magnitude from the Al-26 data.

We will include reviewer #2's suggestion using a simple 'back-of-the-envelope' calculation (ie (min-max)/(average production rate)) for the upper 65 cm mixed zone.

4. Apply another simple calculation to estimate the length of the minimum total exposure plus burial history recorded by the inherited 26-10 concentrations. This will come out to be 1 Ma++ and shows that these sediments have been sitting here for a long time.

We will include this.

5. Apply another simple calculation at Wright Valley to show that only a very short period of exposure can have taken place after the sediments were mixed/emplaced, which is already in line 264 (< LGM). Noted. We will include this.

The overall conclusions being that the Pearse Valley sediments have been there for a while, but are subject to fairly rapid active-layer mixing, at least some of the time, whereas th Wright Valley sediments don't seem to have been there for very long.

The text currently notes the >1 Ma scale of pre-depositional history of both the Pearse Valley and Wright Valley sediments in their respective cores. We will re-structure our discussion on this topic following the 1-5 steps provided by reviewer #2.

I am not sure you can get much else out of these data. They are interesting from the process perspective, but I don't think they do much from the chronology perspective. In this paper, the emphasis on trying to come up with an age in order to fit these data into the overall discussion of glacier change isn't really a good fit and is not highly informative.

To summarize, I don't really have any strong recommendations for this paper. It reports a lot of data that I for one think are interesting, but, in my view, have a weaker chronological significance then portrayed here. If this paper were published in its present form it would be fine -- readers can clearly obtain and understand the context of the data, even if the data are surrounded by a lot of discussion that is not strongly related to most of the observations. However, I think the paper could be improved by focusing the discussion of the depth-profile data on the process significance and not on the chronology.

In conclusion from the above comments and responses, we will provide additional discussion of the depth profile data and permafrost processes in our discussion.

A couple of minor items:

-- The 26/10 diagrams need to say what production rate was used to draw the simple exposure region and isochrons. I take it this is the surface production rate at the core site? Yes

Of course these diagrams look very different if you normalize each sample to the production rate at its respective depth.

We used the surface production rates derived from the CRONUS-Earth calculator for the surface exposure samples, and from core sites. The production rates will be outlined in the new manuscript.

-- It would be helpful in 3.2.1 to get a little more information about the ice-cemented soil. The use of 'ice-cemented' tends to indicate that there is only ice in the pore space and it is still clast-supported. Correct? Yes, we will provide more information.

Also, are the ice wedges in Figure 5 accurately to scale? Yes

As this is a pretty obscure place that is unlikely to be revisited often, it would be great to get a little more detailed description of the sediments.

Additional description will be provided in the revised manuscript.

-- Line 360-ish. I am confused by the 'any postdepositional...is unknown...' sentence. Clarify? Maybe what you want to say here is what you know, what you are assuming, and what you are trying to solve for by model fitting.

This will be clarified in the revised manuscript.