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The Cryosphere
Discussions
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Interactive comment on "Thermal legacy of a large paleolake in Taylor Valley, East Antarctica as evidenced by an airborne electromagnetic survey" *by* Krista F. Myers et al.

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The authors first want to thank the reviewer for their comments which will greatly approve the manuscript. We appreciate it!!

Reviewer Comment (RC) 1) This manuscript presents a novel approach based on airborne transient EM resistivity surveys and permafrost refreeze modeling to reconstruct the recent (<8 ka) paleohydrological history of the Lake Fryxell basin in the McMurdo Dry Valleys. The resistivity data collected within the Lake Fryxell basin show a clear signal of subsurface brine and permafrost distribution, which is analyzed to provide a maximum age for the last lacustrine draw down event of 1-1.5 ka. Permafrost depth

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Discussion paper

and refreeze modeling suggest that following the ice sheet retreat at 8 ka, lake levels likely fluctuated to up to 81 m above sea level until 1.5 ka. These results provide new insight and place new constraints on recent groundwater and lake level variability that were not detected by other techniques. I have only one major comment regarding the assumptions made in permafrost modeling. As acknowledged in the discussion section, "this model assumes a constant rate of lake level drop and constant Tps for simplification." I strongly recommend including a dedicated paragraph to discuss the ramifications of these assumptions and how results may be affected. For example, how much would the maximum permafrost age change if Tps was allowed to vary by an extra 2, 3, 5 K? What would be the effect on permafrost growth at depth if the ice dam partially collapsed in one or more episodes instead of allowing for a more gradual draw down?

Author Comment (AC) 1) We appreciate this comment from the reviewer, and agree that this topic deserves more discussion. We have decided to include a second approach to calculating permafrost ages using a 1D numerical (finite-difference) model solving the classical Stefan problem of vertical heat diffusion coupled with latent heat release during freezing (see attached Figure A). The upper boundary condition is a prescribed temperature that is deltaT lower than the freezing point of the sub-permafrost brines (deltaT = surface temperature - brine freezing temperature). This deltaT can be either held constant during numerical experiments or can be prescribed to vary with time. The bottom boundary condition of the numerical model is a constant heat flux, set to the geothermal flux of 0.080 W/m2 consistent with two borehole-based estimates proximal to the study area (boreholes DVDP-6 and CIROS-1 in table 1, Morin et al., 2010). Other model parameters are based on permafrost properties listed in Table 1 in our original manuscript.

Existing observational constraints indicate that under modern conditions in the study area, the temperature at the bottom of the permafrost is ca. -9C (e.g., Figure 7 in Foley et al., 2015) while ground surface temperature is ca. -19C (Table 1 in

Obryk et al., 2020), yielding deltaT of about 10C. When we assume a constant deltaT of 10C, our numerical experiment still yields fairly young ages for elevations below the 81 masl sill level (<4ka). We then applied a linear cooling rate of 1, 2, 3, and 4C over the last 10,000 yr to model the cooling trend observed in the Holocene Taylor Dome paleotemperature reconstructions (Steig et al., 2000, Monnin et al., 2004). These ice core constraints are best approximated by a linear cooling trend of 3C per 10,000 years. In the revised version of the manuscript we plan to use these results to create an additional figure (similar to Figure 13) which shows the permafrost age distribution using the 1D vertical diffusion model. This will better address the reviewer's concern of assuming a constant Tps throughout time.

Minor comments below, indexed by line and figure number

RC 2) General comment on acronyms: the manuscript contains a lot of acronyms, which affect the readability for readers who are less familiar with the region and/or techniques in this study. Some acronyms are only used a handful of times, such as RIS, GLW, AEM, DOI, and DVDP, and thus I suggest spelling out the entire words instead. "LGM" appears to be used only four times but is a well-known acronym and I feel it can be left as it is. Also, there are two acronyms that are not spelled out: 49: TV - Taylor Valley? 118: DVDP - Dry Valley Drilling Project?

AC 2) We agree that it would be useful to spell out some acronyms that are only used a few times. We made these changes to the manuscript.

RC 3) Line 102: What type and parameters of kriging interpolation was used? Also, why was kriging preferred over other interpolation techniques? Kriging is a predictive algorithm and may diverge or create artifact under certain conditions. I believe the authors need to provide some information regarding the configuration of the kriging interpolation and motivate the choice over other algorithms.

AC 3) The software used for data processing, Aarhus Workbench, currently has the option of inverse distance or kriging interpolation, with actual calculations being carried out by gstat (Pebesma, E. J. 2004 https://www.sciencedirect.com/science/article/pii/S0098300404000676#aep-section-id12). Kriging involves the use of a semi-variogram to determine weights during the interpolation, which makes this method well-suited to capturing spatial correlations, both methods result in similar images in the main area of interest. The variogram model we use in the kriging is a simple exponential function with log-transformed resistivity values, a sill value of 0.16 and a range of 1520 m. We will add a segment about why we chose kriging to the manuscript in Section 2.1 (Resistivity Surveys).

We also added a sentence to further highlight some of the artifacts of the interpolation that are not real, especially around the edges of the mean resistivity map (Figure 6). We added model node locations to our location map to provide an easy visualization of data density, which will give insight into which features may be anomalous artifacts and which are model based. When making Figure 6, we had to make a tough decision defining interpolation search radius (here we chose 1,000 m) in order to allow enough overlap between surrounding model nodes and avoid gaps in the spatial mapping. However, a larger search radius does produce some artifacts around the edges that need to be explained better.

RC 4) Line 113: I suggest adding some information on the DEM employed in this study.

AC 4) We agree with the reviewer, and added the following information in Section 2.1: "The digital elevation model (DEM) used for this study was generated from a 2015 LiDAR campaign flown over the McMurdo Dry Valleys in 2015. The DEM has 1 m spatial resolution and covers all of Taylor Valley (Fountain et al., 2017)"

RC 5) Line 134: Here the authors use the -20 C average air temperature of Lake Fryxell from Obryk et al. (2020) to calculate the age of permafrost. However, this temperature was calculated over a timespan of 30 years, and thus may not be representative of the air temperature since the permafrost refreeze initiation. I understand that the Monte Carlo analysis takes the uncertainty of each parameter into account, but I think there should be a discussion on the reliability of a recent temperature measurement in the context of a much longer time scale.

AC 5) See response to first comment. We have included a variable Tps through time (3C linear cooling trend over the last 10ka). We appreciate the reviewer's comment and believe this will greatly improve the paper.

RC 6) Line 145: Is a geometric mean appropriate to calculate the bulk thermal conductivity of sediment, fluid, and air mixtures in this scenario? I recommend motivating the usage of a geometric mean over other mixing formulas. For example, Fuchs et al. (2013; https://doi.org/10.1016/j.geothermics.2013.02.002) explore a few different mixing formulas and find that some are better than others for specific sediment mixtures.

AC 6) Even Fuchs et al. (2013) concludes "From the studied models, the geometric mean displays the best, however not satisfying correspondence between calculated and measured BTC" (where BTC stands for Bulk Thermal Conductivity). It is important to note here that the value of BTC for ice-saturated sediments that we calculated using the geometric mean method (2.57 W/m/K in our Table 1) is very close to the BTC (2.55 W/m/K) that one can calculate for the DVDP-6 borehole based on geothermal flux and geothermal gradient given in Table 1 of Morin et al. 2010. The closeness of the two values may be coincidental but it is the only observational point of reference from this region that we can use to calibrate the performance of our BTC calculations. It is also useful to remember that a choice of the exact BTC model is less important in ice-saturated sediments than in water- or air-saturated sediments, for which the existing models have been developed and calibrated (e.g., Fuchs et al., 2013). This is because the difference between the thermal conductivity of ice and sediment matrix (e.g., 2.2 vs. 2.8 W/m/K in our Table 1) is much smaller than the difference between the thermal conductivity of water and sediment matrix (e.g., 0.57 vs. 2.8 W/m/K). Although we have included the possibility of air saturation in our Monte Carlo model for completeness, the permafrost ages we plot up and discuss in the manuscript are based on assumption of full ice saturation in the bulk of the permafrost layer in our study area. It is our judgement that we cannot substantially improve our model by choosing a different BTC model than the geometric-mean model that we are using right now. Particularly since the calculation of permafrost thickness is only weakly sensitive to BTC of the permafrost layer. For instance, in the analytical solution (Equation 1 in our manuscript), permafrost thickness depends on the square-root of BTC. Hence, even if BTC were to range between 2 and 3 W/m/K, the permafrost thickness would only vary by less than +-10% as compared to the permafrost thicknesses we are now calculating when assuming 2.57 W/m/K.

RC 7) Line 152: I recommend writing either "variance" or "standard deviation." As it is written in the manuscript, it seems like the two are the same thing.

AC 7) Agreed, we removed the word "variance" from line 152 and line 610, and now only use "standard deviation".

RC 8) General comment on the results section and related figures: I suggest moving or at least copy some of the text in the result section over to the caption of relevant figures. Currently, the captions are on the minimalistic side, and I believe that adding further explanations would greatly improve the readability of the paper when readers glance through it quickly.

AC 8) Thank you for this suggestion. We have added additional detail to select figure captions to highlight key results of the figures.

RC 9) Line 247: Inherent -> inherit ?

AC 9) The reviewer is correct. We have replaced inherent with inherit.

RC 10) Fig. 6 and 7: The usage of this rainbow color scale is problematic for a couple of reasons. (1) It is visually non-linear, with sudden jumps in hue that may result in apparent variability of the dataset that does not actually exist. For example, there is a large jump in light blue-green-yellow that conveniently coincides with the proposed boundary between brine and permafrost resistivities; although this helps locating such putative boundary, I find it potentially misleading. (2) It is very hard to read by colorblind people. To the most kind of color blindness cases, this color scale looks symmetrically identical below and above 200 ohm*m, thus making it very difficult to distinguish which areas are low and high resistivity. Fig. 8 and 13 also employ a non-linear color bar with a large jump mid-range.

AC 10) We understand the concern of the reviewer (particularly with respect to color blind readers). This exact rainbow color scale is very commonly used for airborne geophysics, and is close to being the de facto standard. A linear color would produce images where the structure is visible, but the values on the figure would be completely unreadable. Also a linear scale would be extremely difficult to capture the variation across three orders of magnitude. The log scale balances seeing contrasts in both the low and high resistivity limits (which is needed in this region). We believe it would be far more difficult to find a good looking linear scale that can capture the variations in our resistivity maps that the log scale currently shows well. For these reasons, we propose keeping the color scale as is, but will defer to the editor to make the final call or provide guidance.

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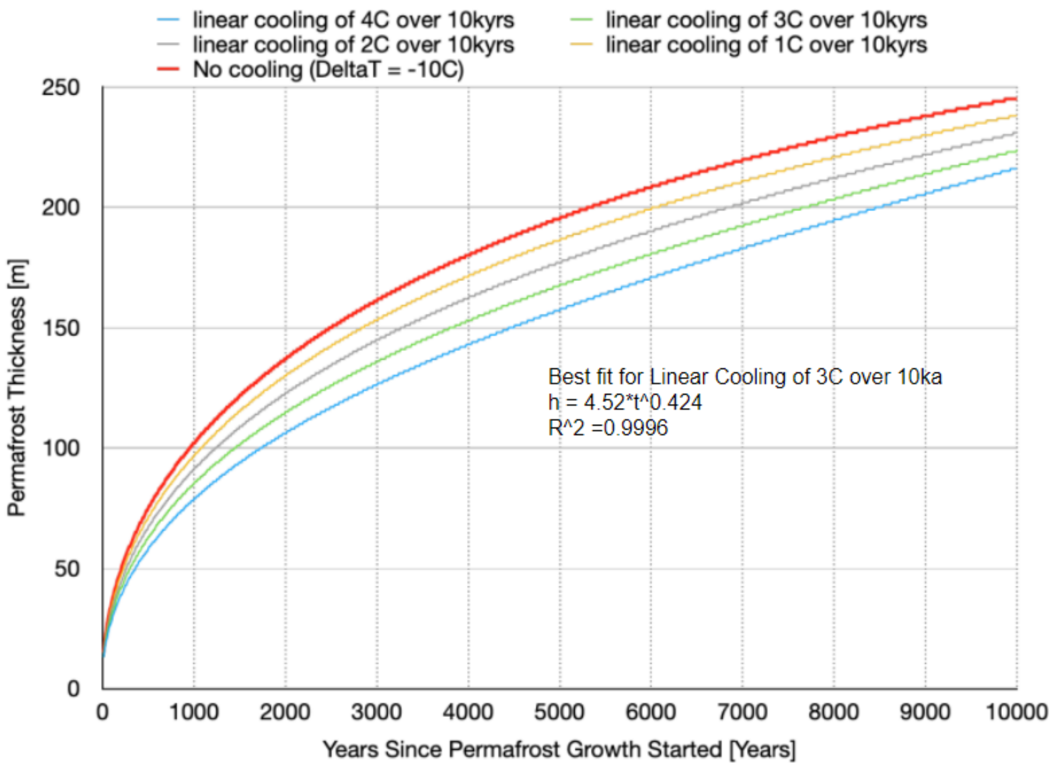
linear cooling of 4C over 10kyrs
linear cooling of 3C over 10kyrs
linear cooling of 2C over 10kyrs
linear cooling of 1C over 10kyrs
No cooling (DeltaT = -10C)
Permafrost Thickness [m]
Years Since Permafrost Growth Started [Years]
Best fit for Linear Cooling of 3C over 10ka
h = 4.52*t^0.424
R^2 =0.9996

Fig. 1.