The Cryosphere Discuss.,
https://doi.org/10.5194/tc-2020-241-RC2, 2021

The Cryosphere
Discussions

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.

Anonymous Referee #2

Received and published: 5 January 2021

My first impression upon reading this paper was, wow, what a novel way to approach this problem! People have been trying to track Dry Valleys lake levels since Scott in the Heroic Age. None of the existing datasets is complete. Landforms (mostly deltas) dated by radiocarbon tell you when the lake was at a certain level, but geomorphic records are fragmentary and incomplete. Lake bottom sediments from beneath the present lake can give more continuous records, but their interpretation in terms of specific water level is complicated. Here, the authors give a third way of reconstructing former lake levels, which I'm sure has its own associated problems, but which presents a third perspective that may help us circle ever closer to the actual lake history.

That said, I cannot judge the actual methods used in the remote imaging or in interpretation of the resistivity data. It is outside of my field of expertise. I am assuming here that it is correct, and more reliance should be placed on other reviewers on this aspect.

Although my overall impression of the paper is quite favorable, I do think there are some areas that could use reframing and a critical assumption or two that should be revisited.

Major Points 1) The analysis seems to operate on the assumption that there was a continuous drop in lake level from the ~80 m sill level to the present lake over the second half of the Holocene (and indeed, perhaps from the highstand of GLW). However, this is unlikely to have been the case. This would have been a closed-basin lake and highly sensitive to changes in inputs (almost entirely glacial meltwater) and outputs (evaporation/sublimation). This hydrology places it among a category of lakes that typically shows outsized reactions to small changes in water balance. Pre-Holocene (and even Holocene) lakes in the Dry Valleys region are suggested to have undergone large-scale fluctuations during the last glaciation and termination of the ice age (i.e., Hall et al., 2010, PNAS - a reference which probably should be included). Lake Vanda showed large fluctuations at ~3 and ~6 ka. So, it is likely that Lake Fryxell did as well - and possibly other changes not yet documented. To what degree does this lake-level volatility affect your modelling results and error bars? How long must permafrost be covered up to completely melt under a lake (I assume this results from permafrost thickness and length of lake cover)? In short, it seems as if this assumption may be quite critical to the outcome of the model.

2) The paper attempts to compare model results with 14C and OSL data. However, this is mixing of apples and oranges (and may stem from the assumption in point 1 above). Both the 14C and OSL dates refer to the position of the lake at a specific time. They do not preclude the lake from reaching that same level (or higher) at another, later time. It is highly likely that most if not all of the dated deltas were under water at multiple times, not necessarily reflected by the dates (this is to some degree acknowledged in the

discussion of OSL, but the same applies to radiocarbon). Some deltas at critical levels (i.e., the sill level) may have been occupied and active at several times. Thus, in my opinion, you cannot make a direct comparison of the three different dating methods, simply on elevation. These lakes fluctuated many times and there are going to be deposits from earlier times mixed in at the same elevations occupied by later lakes. Recognition/resolution of this assumption may help resolve some other issues raised below.

3) I am having some difficulty with Fig. 14. If I am understanding it correctly, parts of the modelled curve become problematic and require some explanation. I have already mentioned the 14C and OSL data - I don't think you can compare them directly for reasons mentioned above and recommend taking them off this diagram. My concern here is about the model results themselves.

My understanding is that this figure shows the mean of the model (solid line), a dark shaded zone (1 sigma), and a light shaded zone (raw data). The underlying assumption here is that the permafrost age is directly linked to the lake presence (let's revisit this in a minute). Thus, the older permafrost ages at higher elevations are attributable to the lake dropping from there earlier. Those at >80 m must be from a time when there was still an ice dam at the valley mouth, because they are higher than the sill. However, model ages for >80 m seem far too young. Measurements by multiple methods place deglaciation at the mouth of Taylor Valley by $\sim$8 ka if not a bit earlier. On Fig. 14, 8 ka intersects the model results (solid line) at $\sim$160 m elevation, something that is not possible, because there would have been no ice to dam the lake. Even the 1 sigma error bars are well above the 80 m sill. The lake cannot exist above 80 m after 8 ka, so how is this result explained? Only the low end of the raw data fit in the plausible zone (>80 m = >8ka). Is there something about the model or in the picking of 100 Ωm that is producing results skewed toward young ages? How does this affect the reliability of the conclusions in this paper? In other words, if we know the model is producing erroneous ages above 80 m, why should we have confidence in results below 80 m

elevation? Greater discussion would be useful here.

4) While it seems plausible, is changing lake level the only variable that can affect permafrost age? How old is that brine?

Minor Points (by line) Line 49 - use the calibrated value, not the raw 14C age.

Line 54 - The Taylor Dome Holocene chronology has been revised (Monnin et al., 2004), which actually is fairly substantial in the mid-Holocene. Use of any specific times should be checked as, if my memory is right, parts of the chronology have shifted as much as $\sim$1500 years.

Line 61 - "atmospheric correction" is an odd way to put it. I would simply call them calibrated ages. Hall et al., 2010 in PNAS is a key paper here with an updated dataset (see the SI with that paper).

Line 67 - I'm not sure the lack of samples led to an assumption that lakes remained at or below modern levels over the last 5000 years but only that there were no data and that Lake Fryxell had to be below the sill.

Line 106 - says <1000 Ωm but line 115 and elsewhere uses 100 Ωm as the cutoff. Is this a typo or something else? If not a typo, why was 100 chosen?

Line 196 - see comment above about these deltas being covered and recovered by water and thus direct comparison is impossible.

Line 205 - this distance would be made more useful for lake level if an elevation were associated with it.

Line 213 - this assumption is critical and might be key.

Line 225 - this paragraph goes with the assumption of gradually lowering lake, which was not the case. See Hall et al., 2010. It also makes the assumption of a close link between air temperature at Taylor Dome and lake level, which may be problematic. Also, see comment above about revised Taylor Dome Holocene timescale.

Line 238 - I may have missed it, but I didn't see anywhere in Hall and Denton (2000) where they stated that lake levels had remained at or below present since 5 ka. They didn't have data to address this.

Line 243 - the sentence about OSL dates from depth and not applying to most recent episode of lake level change applies equally to radiocarbon.

Line 249 - application of a large reservoir correction to shallow water lake sediments seems ad hoc, given prior references. Most of these deltas formed at the same stream mouths as today, fed locally by the same glaciers as today, not the RIS. As the data should not be compared directly (for reasons already given), such an attempt at correction is not warranted.

Line 259 Whittaker here and later in paragraph.

Par. starting with 265. While I don't necessarily disagree with the conclusion of the last drop in lake level being post 1.5 ka, I find these stated reasons unconvincing. The link with the Taylor Dome ice core here implies, without specifically stating, that high temperatures should lead to high lake levels. But, this may not be the case. If you want to make this argument, this needs to be stated explicitly (and you'll want to confirm that the chronology didn't shift too much for this to still apply). In addition, there are many, large, well-preserved deltas and while the Crescent Stream one is a nice one, weathering rates are so low that you cannot resolve 1000 yrs vs 10,000 years based on delta appearance.

Be careful with the 6 degrees warmer comments about Taylor Dome. Temperature is only one of several variables that affect stable isotope data. In addition, the 100 yr running average in the Steig paper (probably more applicable to lake-level changes than any single year) suggests only about $\sim$1 degree change and is probably a better representation of actual temperature changes.

I think these reasons don't help your argument any. Why not just say your data suggest

the lake might have last been at the level of the sill at 1.5-1 ka and leave it at that? To my knowledge, there is nothing that says it couldn't have been. The OSL and 14C data are older, but they may be dating earlier events.

Line 287 and elsewhere. GLW refers only to the ice-dammed lake. Once the ice sheet retreated, this became Lake Fryxell.

Please double check references. I didn't check them all, but the one I looked up to make sure it was spelled correctly (Whittaker) wasn't there.

Figures 1) Please add latitude/longitude 2) You may want to switch to the Monnin et al., 2004 timescale. 3) May want to mark the location of this figure on Fig. 1 4) suggest changing "atm corrected" to "calibrated" 6) I found the color on these maps (this and the ones that follow) hard to follow, I'm afraid. Is there a better way to present this? The caption could be more informative for those (like me) who do not deal in resistivity commonly. Perhaps you could add a sentence about what is meant by constant elevation. 13) see comment above about using GLW. This should be "lowering of Lake Fryxell" rather than draining of GLW, which would have happened when the ice sheet retreated. Draining really isn't the right word for something that evaporated. 14) was covered in comments above. Suggest removing other chronologic data as the comparison doesn't really mean anything and is potentially misleading.

Overall Impression Although I have pointed out several potential issues with the paper, I think the approach is really intriguing and the study is important to publish. I think most of these comments can be dealt with fairly quickly. Assessing the impact of the assumption about constant lake-level drop (which is critical), may take more effort. I recommend publication after moderate revision.

Interactive comment on The Cryosphere Discuss., https://doi.org/10.5194/tc-2020-241, 2020.