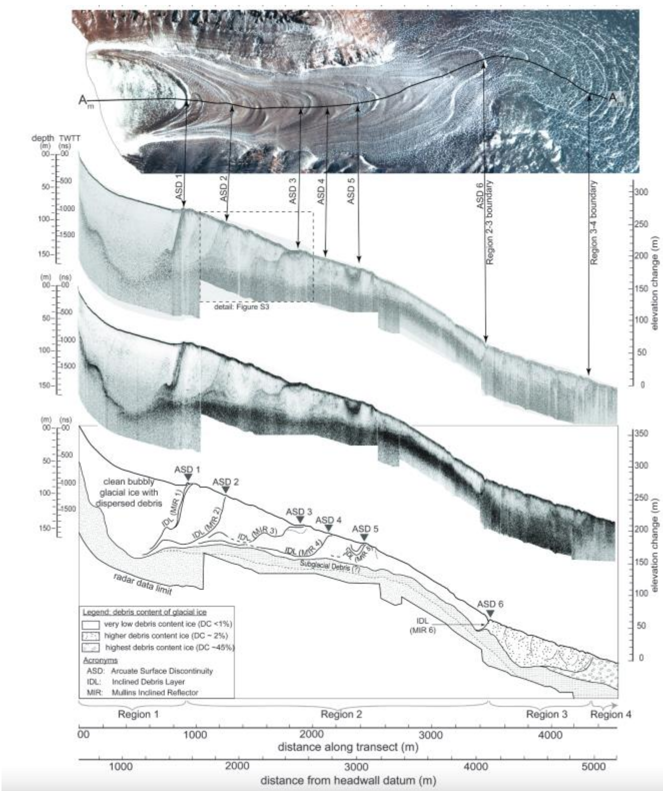
General Comments.

This manuscript presents geomorphic measurements of alcoves associated with martian glacial terrains and interprets the morphometric data and HiRISE observations of the sites to describe a sequence of glacial erosion processes. The alcove measurements are novel, interesting, and exceptionally important for understanding glacial and permafrost processes on Mars.

However, the manuscript has several critical weaknesses that need to be addressed.

1) One of the foundational assumptions presented in this paper is that glacial cirques require wet-based glaciation to form. While this is true for temperate and mid-latitude cirques, particularly for modern alpine glaciers and for older glacial remnants, it is not universally the case. Antarctic, cold-based glaciers can produce pronounced alcoves with large reliefs, over-deepened regions where bed shear stress is maximized, and downslope export of eroded or rockfall debris—all without any evidence of wet-based or polythermal conditions. See fig below from (Mackay et al., 2014) and (Mackay and Marchant, 2017):



The paper would be greatly strengthened by *evaluating the hypothesis* that morphometric observations can be used to distinguish wet-based vs. cold-based glacial cirques. If wet-based and cold-based glacial cirques can't be distinguished in the Earth-derived measurements were collected as part of this study and in prior work, then there's no basis for thinking that they can be distinguished on the basis of morphometric observations on Mars. If they can be distinguished in the Earth dataset, then it is a powerful tool applying to the Mars data in order to *test the hypothesis* that the morphology of martian cirques is more similar to the morphology of wet-based cirques than to cold-based. If it's impossible to tell the difference between cold- and wet-based glacial cirques on Earth, or to distinguish "cirque-like" versus non-cirque-like alcoves on the basis of morphometry in the Mars measurements, that's useful to know, too—it means that other tools or measurements need to be applied to figure out the thermal state of martian glacier beds.

My main concern is that from the very start of the manuscript, the wet-based model is accepted as an assumption: "Cirques are expected to form from depressions in mountainsides that fill with snow/ice and over time support active glaciers that deepen the depressions by wet based glacial erosion." If that's the only possible model, then there's nothing being tested by the hard-won measurements the team has made. More needs to be done to critically evaluate the many and interesting measurements the team has made.

2) The paper would be greatly strengthened by a full assessment of the morphometric data collected for the martian alcoves and the example cirques and alcoves on Earth. Analyses are only shown for the "cirque-like" alcoves, which were determined by a combination of morphometry and image interpretation. This begins to feel like cherry-picking of the data to describe alcoves that look right. It would be great to see summary statistics like those shown in table 4 for all 7 of the morphological groupings. A box and whiskers or violin plot with each group on it for L, W, H, size, area, would be helpful to see if the classes are different. Most statistical software packages could tell if there are significant differences between the groups based on the measured properties. It would be neat if there are—but if there aren't significant differences, it just means that either the morphometric properties being measured don't capture the variation or that the resolution of the dataset wasn't up for the task. Scoping the manuscript's conclusions based on the properties of the whole dataset is very important and without it, the results could be construed to be highly cherry-picked.

3) One of the challenges in the paper is distinguishing the alcoves from which small, superposing glacier-like forms emerge versus larger alcove which appear to be associated with large LDA/LVF glaciers. Sometimes a small GLF superposes an LDA/LVF, and they may share an alcove. But without distinguishing these two very different sizes and generations of glacial activity, it's difficult to evaluate the estimates of erosion timescales. Small GLF emerging from small alcoves that superpose LDA/LVF may very well have rapidly eroded their alcoves. But LDA are much larger and much older than GLF, and likely had longer timescales to remove material from the large alcoves they occupy. These two classes of alcoves seem to be lumped together in the median and mean height/volume datasets, and might be giving the impression that small, superposing, younger glacier/flow features have done much more geomorphic work much faster than they actually have.

Specific comments.

Lines 15-17. On Earth, cirques most commonly form due to basal erosion beneath wet-based or polythermal glaciers—but why start this analysis with this assumption? There are many cirque and arete-like landforms in environments where cold-based glaciation dominates, for example, ringing Beacon Valley in the McMurdo Dry Valleys (77.88 S, 160.58 E). Presupposing that cirques form through **wet-based glaciation is an unnecessary assumption and sets out the analysis as having something** to prove, rather than examining the morphology and seeing what conclusions can be drawn from it.

Line 22-23. Is the proposal that the cirques and the lobes are a late stage of glacier evolution, or that the cirques are? The cirques could pre-date the presently occupying lobes, I'd think.

Line 35. "Presumed" is pretty editorial in tone. It has been modeled (Hecht, 2002; Kreslavsky et al., 2008; Schorghofer et al., 2019) to be difficult, but certainly not impossible, that that episodic meltwater occurrences could have happened in the Amazonian, usually during obliquity optima, or on steep slopes, sometimes with the assistance over thermal shielding from overlying CO2 ice. But widespread conditions where ice is present when surface temperatures reach the melting point is not produced in most models, especially on flat surfaces.

Line 38. (Levy et al., 2016) showed that mean erosion rates during Amazonian glaciation are several orders of magnitude higher than average Amazonian martian erosion rates and bracket erosion rates for cold-based glacial environments (which do erode their beds, just not much!)

Line 42. There's definitely evidence supporting englacial debris bands in LDA, e.g., (Butcher et al., 2023; Levy et al., 2021), but the jury is still out whether this debris is alcove-derived from rock fall or somehow entrained at the bed.

Line 57. I think the strongest evidence for the age of CCF is 300-800 Ma from (Fassett et al., 2014). The reason I thing those dates are less likely to be off from others is because they didn't count craters on the surface of glacial deposits themselves, which have been heavily reworked, instead counting on large ejecta blankets from craters.

Line 79. Coordinates are usually reported as lat/long—is there a reason to report location as long/lat here?

Line 83. What does it mean to be analogous here? Formed by wet-based glaciation? Formed by subglacial erosion? Formed by evacuation of rockfall from over-steepen cliffs? There's lots of ways these alcoves and cirques could be similar—in what ways are they being compared?

Lines 85-86. See comment for line 15-17. Strong analog studies require open-mindedness to the idea that physical conditions may be different between alpine, polar, and planetary settings. I think the best planetary science emerges from framing our Earth-bound heuristics as a question: on Earth, many (though not all) cirques form through wet-based erosion at the point of maximum basal shear stress; how does the morphology of martian alcoves from which glaciers emanate compare to the morphology of terrestrial cirques in cold-based and wet-based environments?

Line 87. I think the cirques/alcoves you show in Fig 2, and the many glacial valleys/cirques in the Antarctic argue against the idea that cold based glaciers don't carve alcoves/cirques. It's true that most alpine glaciers are wet-based or polythermal and that the erosion rates produced by these glaciers are higher, but the slower rate does not necessarily mean cold-based glaciers don't carve cirques. Put another way, can you show based on your terrestrial analog examples that there's a substantiative difference between the alcoves that cold-based glaciers flow from versus the cirques that are carved by wet-based glaciers? That would make for a really interesting basis for comparison for understanding what attributes of the martian alcoves are more like wet-based cirques or more like cold-based non-cirque alcoves.

Fig. 2. The DEM served by Google Earth is coarse spatial resolution and is also smoothed in places to improve image draping. It's not the most reliable dataset for geomorphic measurements. For many of your global sites, SRTM (Shuttle Radar Topography Mission) data are available and are certainly not coarser in spatial resolution, but have the advantage of being traceable to a particular

dataset with known positioning uncertainties. For alcoves in the US, the National Elevation Dataset (NED) would be a more traceable and higher resolution dataset for measuring cirques. It is served up for easy access by the USDA Geospatial Gateway (https://datagateway.nrcs.usda.gov/). For the Antarctic examples, the REMA reference elevation model for Antarctica might be a good bet for higher spatial resolution topography (served up by the Polar Geospatial Center at http://pgc.umn.edu). The best way to measure cirque properties in places like Beacon Valley or the high Asgards/Olympus range in the Antarctica would be to use the lidar data for those sites, distributed in DEM format by OpenTopography.

Lines 91-94. Given that cirques are most commonly recognized when they are empty of ice or nearly empty of ice, what measurement challenges exist for mapping martian alcoves that still have ice and/or debris in them? Are there ice-free cirques on Mars in which an over-deepened depression (a Mars tarn?) could be identified?

Lines 100-101. Is it also possible that the alcoves formed slowly during the ~500 My of large-scale (LDA and CCF-producing glaciation) described by (Fassett et al., 2014)? We don't usually have multi-million-year glacial erosion events on Earth—so the timescales on Mars may be different from our terrestrial expectations.

Fig. 3. What projection was used for data analysis in the Deuteronilus Mensae region? The craters in Fig. 3 look foreshortened, like they are being plotted in a geographic coordinate system. For length measurements, locally projected coordinates will produce less projection error, which can be large at middle to high latitudes.

Fig 1 and Fig 3. I'm a little confused by the selection criteria for branching cirques. There's several features that look like they could be mapped as branching cirques in Fig. 1, but they are not tagged as cirques because they have GLF in them. But many of the features marked as candidate cirques in Fig. 1 have LDA or remnant LDA or debris aprons of some kind in them, too. So none of these candidate cirques are empty in the sense that deglaciated cirques in places like the Uintas or the English Lake District are empty. Is it that the GLF-filled alcoves are too small/narrow to map with HRSC DTMs? That's a different issue from what's reported, which is that GLF-filled cirques were not mapped.

Line 149. The classification of the cirques seems very subjective—especially distinguishing crater-like cirques from other rounded alcoves. Do crater-like cirques show ejecta? Is it possible that all the cirques initiated a craters where impact damage broke up rock, allowing for easier down-slope export by the glacier? Craters commonly have internal landslides—could that explain some of the stepped cirques or the features interpreted later to be active layer detachments?

Line 149. Having the classification scheme in the Methods feels a little bit misplaced. Classification is interpretation, so it really could be in the Discussion section. Are there natural breaks in the measured morphometry of the cirques that leads to their classification in certain ways? Are the morphological classes of the cirques that are based on inspection distinguishable from one another in the morphometric measurements?

Line 163. If the description in Table 2 is what is used to define simple alcoves on Mars based on simple cirques on Earth, those are not morphometrics (i.e., things with lengths, volumes, slopes, etc. that are measured)—"having an armchair shape with a defined headwall, two sidewalls, and are open downslope" is a qualitative description of the landform. I think it is not accurate then to say "simple alcoves have morphometrics consistent with simple cirques on Earth." It would be more accurate to say, "Like simple cirques on Earth, simple alcoves on Mars have an armchair shape with an identifiable headwall, two sidewalls, and an opening downslope."

Line 163-164. This is the methods section—no data have been presented or analyzed—but a conclusion is being reported: "Herein, we use the term "cirque like alcove" for these martian alcoves that are the most likely candidate cirques." That's a clear indication that this should be in the discussion section, after measurements and results have been presented. This is especially concerning because the introduction implies that "cirque-like" in the context used in this paper has a genetic meaning, i.e., carved by wet-based glaciation. At this point in the manuscript, there is not enough evidence to support that claim. If "cirque-like" just means "an eroded alcove that hosts the accumulation zone of a glacier," it's less problematic to use it here, since these are alcoves, many of which host landforms interpreted by debris-covered glaciers. But if an origin is implied by "cirque-like," more evidence needs to be marshalled to show that these shapes are exclusively consistent with wet-based glaciation.

Fig. 4. It looks like the spatial resolution of the DEM profiles is very different between these alcoves. Is it a different dataset for each of them?

Fig. 4. What is the purpose of plotting the alcove profiles? To show that they open downslope? Put another way, if someone presented you with a profile of a random alcove in the collection, would you be able to use just the elevation profile to figure out its classification?

Lines 192-193. The use of the morphological classifications suggests that the morphometry does not discriminate well between the observational morphological classes. How many simple alcoves wouldn't have been classified as cirque-like if you just used the morphometrics? How many other types of alcoves would have been classified as "cirque-like" if you hadn't excluded them based on the classification?

Line 192. How many cirques from cold-based glacier sites are in the Barr & Spagnolo (2015) database? Put another way, could these morphometric means and ranges distinguish alcoves formed in cold-based sites from warm-based sites? Are they diagnostic of one process or another?

Lines 193-195. This seems like it's a result, not a method section sentence.

Line 223. It shows a tremendous eagerness to find similarities between the martian alcoves and the terrestrial cirques by grouping results of the measurements in with comparison to their counterparts on Earth. It might be more clear to report the novel measurements for Mars in the results and then compare them to the properties of cirques on Earth in the Discussion section.

Line 224. Again, no results shown yet, and more conclusions: "Focusing on cirque-like alcoves only because they are the most likely candidate cirques." Inferences, interpretations, and classifications belong in the discussion section, and should be framed as inferences based on the data analysis, not based on assumptions made going into the measurements.

Line 229. It would be best to show all the DEM-based measurements, not just the measurements for the alcoves that were assumed to be most Earth-like. It would be great to see summary statistics like these for all 7 of your morphological groupings. A box and whiskers or violin plot with each group on it for L, W, H, size, area, would be helpful to see if the classes are different. Most statistical software packages could tell you if there are significant differences between the groups based on the measured properties. It would be neat if there are—but if there aren't significant differences, it just means that either the morphometric properties being measured don't capture the variation or that the resolution of the dataset wasn't up for the task. Scoping your conclusions based on the properties of the whole dataset is very important and without it, the results could be construed to be highly cherry-picked.

Line 236. "Size" is confusing here—it only gets defined later in the paper. Please define it in the main text and not just in the caption.

Lines 235-236. Is this trend in all alcoves or just the narrowed down subset?

Line 243. When doing orientation analyses, it's important to normalize to the availability of host slopes. Are S and SE-facing alcoves abundant because they really form preferentially in that direction, or are there just a lot of E-W slopes in the study area and alcoves will form normal to the slope they are seeded on. Can the rose diagram be normalized based on a sampling of slopes in the study area?

Fig. 8. Binning the data seems like losing resolution. Could these plots be rendered as scatterplots to show the trend more clearly? If the inference is that there's a correlation between these geographic attributes like latitude and elevation, it would be possible to compute a correlation coefficient using the unbinned data.

Fig. 9b. This seems like another case where it's important to check whether the study site increases in elevation towards the north. A random sampling of elevation points in the study area should help normalize to the available elevations for alcoves to see if the alcoves are over- or under-represented at high or low elevations.

9c. Likewise, is the mesa heigh limiting cirque height in the study area? All those fretted terrain blocks are about the same elevation above the surrounding plains. Do alcoves cut all the way down through them to plains level? Is there something else that's limiting incision of the alcove into the mesas like internal layering? The change in alcove height with latitude is neat, but it's important to know if the mesas get shorter/taller with latitude, or if it really is something to do with the alcoves.

Line 269. If preserved GLF are pointed north, but alcoves are pointed south, doesn't that argue that whatever produced alcoves is driven by being equator-facing (warm at low obliquity or cooler at high) and whatever drives GLF formation is driven by being pole-facing (cold at low obliquity and warmer at high)? So, maybe thermal cycling matters more for alcoves and ice accumulation matters more for GLF? It just seems like so many of the alcoves mapped in this study are associated with much larger, much older LDA and LVF, that the connection to GLF seems very tenuous. It's like looking at the McMurdo Dry Valleys and inferring a causal link between the Miocene fjords cut by ice sheet draining outlet glaciers and the small alpine glaciers that occupy the valleys today—except the time scale may be more directly comparable to linking LGM glaciation to Proterozoic snowball Earth glaciation (which is what the age difference between the alcoves and the GLF may be).

Line 307. In most places where gullies and glacier-related alcoves are present and have been mapped (Dickson et al., 2023; Dickson and Head, 2009), they postdate the LDM which postdates the alcoves (based on superposition relationships). The hypothesis that gullying could provide seed points for alcove formation is intriguing and implies cyclicity to ice accumulation and melt, but there's not evidence to support that interpretation that has been presented here.

Line 322. One interesting thing we saw in (Levy et al., 2021) was that the number of boulder bands on LDA (inferred to be internal debris bands) increases with latitude—so further north sites seem to meet threshold conditions to both start and stop glaciation more often than low-latitude sites. So it's interesting that the alcoves get smaller closer to the pole. That suggests that glaciation on Mars is very inefficient at moving rock, especially at higher latitudes, where even though glaciation spins up more over the course of the Amazonian, it doesn't move more rock than low latitude sites.

Line 332-333. This does not follow. Not all alcoves have GLF emanating from them, and not all GLF emerge from mapped alcoves. So how can a correlation be drawn between them?

Line 342. Again, this assumes that GLF and alcoves are contemporary processes, which is an assumption not supported by the measurements here. All these alcoves fall within the zone over which large debris-covered glaciers (LDA, LVF) are found, so there's not a need to expand the area over which glacial activity occurred. There is not a causal or time link shown between alcoves and GLF, so I don't understand the basis for inferring that there is a much larger area over which GLF were active but are somehow vanished now.

All Figures. It would be helpful to show the alcoves that were digitized using hollow polygons and/or dashed lines. In Fig. 5, the opaque polygons cover up details that would help a reader understand the extent and morphological features in the alcove zone. In Fig. 4 the mapped alcoves are absent from the figures. It would be very helpful to see what was being considered.

Line 348. Presenting new geomorphic observations seems like results. Can this be moved from the discussion section to the results section?

Line 382. Frost heave (needle ice formation via cryosuction) is a mechanism to produce sorted patterned ground on Earth, but it is not a mechanism that is needed to create thermal contraction crack polygons of any type.

Lines 367-375 and Line 428. It seems unlikely that the downslope surface lineations shown here atop the mantling terrain that superposes the bedrock of the cirque resulted from subglacial erosion beneath the glacier that carved the alcove. The ridges superpose a younger mantle unit that in turn superposes the alcove. Neither the stratigraphic relationships, nor the relative ages of the deposits support the interpretation that the ridges formed subglacially.

Line 402 and Fig 14. These ridges are really interesting, but also very mysterious. I think we don't know what those ridges, which appear within LDA, near the toes of LDA, and sometimes along the edges really are. They could be drop moraines or medial moraines (Baker and Carter, 2019). They could also be some kind of internal thrusting feature (Stuurman, 2017). They could be eskers (Butcher et al., 2021), or internal debris bands outcropping (Levy et al., 2021). So it's best practice to not give them a genetic name when a descriptive one will do. Unless there's new evidence here suggesting they are moraines, it would be best to call them transverse or flow-parallel ridges and then to use the observations to help evaluate their origin. What seems most important here is that these ridges are on or in the top-most mantling unit that superposes the bedrock alcoves. So it's stratigraphically some of the most recent material present. Inferring moraine deposition based primarily on ridge sinuosity seems like insufficient evidence. Is there a different in lithology, color, grain size—any other evidence that might help shape the interpretation?

Lines 417-419. Is the area/ridge size relationship something seen in this study? The alcove measurements made in this paper seem very well suited to evaluate this previously reported trend.

Line 483. What does it mean for these blobs to be glacial remnants? Does that imply shrinking of a previously expansive glacier that accumulated and flowed? Or is it possible they are dead ice fields that never grew large enough to deform and flow? How could the two be distinguished?

Lines 492-493. Many assumptions go into using Earth-based erosion rates to estimate the duration of processes on Mars, many of which could be orders of magnitude off in scale. Is it possible to use evidence from Mars to constrain the timescales through crater counting, or published age estimates for mantles, LDA, etc.?

Line 504. At Mars temperatures and ice grain sizes and dust contents, ice can be quite brittle and resistant to flow, see (Milliken et al., 2003). How important is the A parameter in this Earth-derived model?

Line 508. In the mapping, h represents the relief of the alcove—it's maximum minus its minimum elevation. Why is it used here to stand in for ice thickness? In many modern alcoves or cirques, ice thickness is quite a bit thinner than the relief of the headwall. See radar cross sections from (Mackay and Marchant, 2017; Petersen et al., 2020). h is a kind of "bank full" flow, but it's not the maximum possible ice thickness, nor the minimum.

Line 513. K_g seems like an important parameter, too. Are there reported K_g values for basalt? How much do Kg and l values vary? Could the erosion amounts be framed in terms of that range of possible inputs?

Line 538. This ~500 My timescale is pretty consistent with the duration of LDA and CCF-forming glaciation based on the (Fassett et al., 2014) crater counts. That's very neat!

Lines 546-547. Is it possible that erosion of the supra-glacial debris is what carved or evacuated the alcove?

Lines 573-577. Taking a look at the steep, concave shape, and over-deepened basin and threshold in the (Mackay et al., 2014) radargrams, there is not evidence that those formed through wet-based glaciation. I think the heuristic that steepness, over-deepening, and concave shape only form in wet-based glaciers is incorrect and inappropriate to apply here.

Line 600. Active layers are difficult to produce on Mars during the past 10 Ma. While not impossible, especially in steep settings (Kreslavsky et al., 2008), generating saturated conditions at the base of a detachment surface might be expected to produce downslope spring features, channelized erosion, etc. Are these accessory features observed as well?

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