AGU Advances

Authors' Response to Peer Review Comments on

Evidence for Deposition of Chloride on Mars from Small-Volume Surface Water Events into the Late Hesperian-Early Amazonian

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Authors' Response to Peer Review Comments on Original Version of Manuscript (2021AV000534)

Dear Francis:
We appreciate the thoughtful reviews by you and the 3 reviewers and have made revisions accordingly, as detailed point-by-point below. The most significant changes are addition of the online supplementary database at CaltechDATA (and its linking in the Acknowledgements), the tweaks to the Introduction and Discussion to satisfy the comments of reviewers about reference to prior literature and S and Cl decoupling, and shortening by moving former Figures 3 and 6 to the Supplement.
We believe the manuscript is now ready for publication.
Bethany

Dear Dr. Ehlmann:

Thank you for submitting your manuscript entitled "Evidence for deposition of chloride on Mars from small-volume surface water events into the Late Hesperian-Early Amazonian" [Paper #2021AV000534] to AGU Advances. I have now received 3 reviews of your manuscript, which are included below and/or attached.

Based on the review comments, your manuscript may be suitable for publication after minor revisions. My editorial comments are below; in their revised version of the MS the authors should respond to all comments in a point-by-point fashion unless otherwise noted.

This MS investigates the dying stages of aqueous alteration on Mars, and finds that it occurred more recently than previously thought. All three reviewers think the results are significant and suggest only minor revisions prior to acceptance, an opinion I concur with. My one real concern with the MS is that it is very long for a supposedly high-impact journal like AGU Advances. Some of the reviewers make suggestions for cuts, or moving material to SOM, and I encourage the authors to think carefully about doing so.

Rev 1 is concerned about giving credit for prior work, an easy fix.
They make some suggestions for reducing length, and also suggest making a summary database of the observations, which I think the authors should definitely implement. The main science suggestion is to give more details on the derived (young) surface ages; a suggestion I agree with, since this is one of the main results of the MS.
These suggestions are all largely implemented, and we have removed citations to prior literature when not needed for the point at hand, which should rectify some of the details of attribution concerns (see point-by-point details below). Data are available at the CaltechDATA repository linked (doi).

Rev 2 makes the interesting point that one would expect C and S to be coupled, whereas in fact they are not. They also mention perchlorates as a possible source of Cl (recall that Phoenix also detected perchlorates). This should be discussed a little.
Perchlorates are now briefly mentioned as not being present in association with chlorides, at least at a level of detectability from orbit. We have tweaked the discussion and added an additional reference to make the text that we already have on S and Cl clearer. It is actually the case that one should expect S and Cl to become decoupled in evaporative systems, due to different solubilities of their mineral products causing sequential precipitation. We have added two references with geochemical modeling that describe evaporative sequences. See detailed point response below.

Rev 3's main point was to discuss the thermodynamic effects of high salt concentrations, an easy fix.
We have added one sentence on this when describing the conceptual model in the Discussion, as detailed below.

I had one semi-quantitative question. You need enough late-stage runoff to dissolve and then transport the Cl down-stream by presumably tens of km - so there is a lower bound on the flow rate. But if the flow rate were too large, the

resulting water deposit would be too laterally extensive. Are there any bounds on flow rates that can be derived by arguments of this kind?
This is a great question, and it is likely that future work could consider the slope and extent of deposits to estimate min/max flow volumes at different locations. But it would require someone with more knowledge of physical hydrological modeling within porous media (probably also considering loss to evaporation and freezing) than we authors possess.

Regarding length, I think the authors would be well-served by moving some material to SOM. The most obvious fix, I think, is to focus in the main text on the deposits which show the youngest ages (Icaria Rupes and Icaria Fossae). At a minimum I think Fig 7 could go to SOM, and also Fig 10 - it's cute but I don't think it's essential. It's also not clear to me that all three of Figs 4, 5 and 6 are needed in the main text.
We thought about how to do this without losing key evidentiary content for a main text-only reader and have consequently moved two figures, the former Figure 3 (additional examples of the thin nature of chlorides; now Figure S5) and Figure 6 (third example of perched, asymmetric deposits; now Figure S6) to the supplement. This also allowed removal of Figure 6 explanatory text and its incorporation into Figure S6's caption. We have retained Figures 7-9 (now Figures 5-7) as directly addressing young surface ages, Figure 10 (now Figure 8) as an appropriate Earth analog, and retained but beefed up the content of former Figure 11 (now Figure 9) by including a timeline.

l.495 should be "permafrost"
Fixed

The feedback provided in the reviewer assessments of your manuscript is important and should be taken into account as you complete your revision. I encourage you to submit a suitably revised version of your manuscript by September 30, 2021.

Upon submission, we will need to receive the following:

1. A response to reviewer file that lists each of the comments and describes how the manuscript has/has not been modified in response to those comments.

2. A copy of the manuscript with the changes noted (e.g., highlighted, "track changes," italics or bold changes). Please upload the article with tracked/highlighted changes as a response to reviewer file.

3. A copy of the revised manuscript with the changes incorporated which will be used for publication if the manuscript is accepted.

4. In addition to addressing the remaining important technical issues raised by reviewers, please also ensure that AGU data policy is addressed in the Acknowledgements section and that the key points report what is learned from the study.

5. All files in publication-ready formats.
***Publication-ready formats for article files are limited to Word and LaTeX (Excel is also acceptable for tables only). Figure files must be individually uploaded as .eps, .tif, .jpg, or .pdf files and all parts of the same figure need to be combined in one file.

6. AGU has officially joined with many other publishers in a commitment to include the ORCID (Open Researcher and Contributor ID) for authors of all papers published starting in 2016. Funding agencies are also asking for ORCID's.

Including the ORCID as part of published author information in papers will better enable linking of content and accurate discovery across individuals, similar to the way DOIs have enabled reference linking across journals. Given a specific scientist's permission, AGU can also add published papers to his or her ORCID record. See our statement https://eos.org/agu-news/agu-opens-its-journals-to-author-identifiers. We can also provide credit to you through ORCID when you serve as a reviewer.
If you have not already created an ORCID or linked it to your GEMS record, please do so as soon as possible. This will need to be completed for us to accept your paper. You can both create and link and ORCID from your GEMS record.

AGU requires that all data needed to understand, evaluate, and build upon the reported research must be available at the time of peer review and publication. Additionally, authors should make available software that has a significant impact on the research. Data, software, and other research objects (e.g. notebooks) should be made available in repositories that support preservation and reuse. An explicit Availability Statement in the Open Research section of the paper is required describing where readers can find and access the data (and software). Authors should include intext citations to data (and software) in the Open Research section and the full citation in the References section. Guidance on what the Availability Statement and Citation should include along with templates and examples can be found at Data & Software for Authors.

New supporting information guidelines
AGU now requires that supporting information be included in one file, except where limited by file type or size. Please see Supporting Information Guidelines in Author Resources (https://www.agu.org/Publish-with-AGU/Publish/Author-Resources/Supporting-Info-Requirements).

When you are ready to submit your revision, please login to your account (https://advances-submit.agu.org/cgi-bin/main.plex), and click "Revise 2021AV000534."

I look forward to receiving your revised manuscript. If you have any questions, please contact the editor's assistant at advances@agu.org.

Sincerely,

Francis Nimmo
Editor
AGU Advances

------------IMPORTANT INFORMATION------------------------
Additional information on text preparation, formatting, acceptable file formats, supporting information, graphics preparation, and AGU style, is here.

Sharing your work is an important part of the research process, and AGU leverages and shares published research to promote the broader importance of Earth and space science. Learn how you can promote your paper, including how your paper can be considered for additional publicity or for the issue cover if it is accepted.

Reviewer #1 Evaluations:
Recommendation: Return to author for minor revisions
Significant: Yes, the paper is a significant contribution and worthy of prompt publication.
Supported: Mostly yes, but some further information and/or data are needed.
Referencing: Mostly yes, but some additions are necessary.
Quality: Yes, it is well-written, logically organized, and the figures and tables are appropriate.
Data: Yes
Accurate Key Points: Yes

Reviewer #1 (Formal Review for Authors (shown to authors)):

Title: Evidence for deposition of chloride on Mars from small volume surface water events into the late Hesperian-early Amazonian
Authors: E.K.Leask and B.L. Ehlmann

Reviewer: Mikki Osterloo

General comments: The article details an update to the statistics of the global chloride survey offering new insights into possible formation mechanisms of these unique and somewhat perplexing deposits. Of importance is updated crater age-dating of the units upon which the chloride materials occur and detailed geomorphological assessments using higher resolution CTX and HiRISE imagery as well DEMs. Additionally, the work presented includes a comprehensive spectral investigation using CRISM to search for associated minerals (e.g., sulfates, carbonates, clays) to better understand the potential geochemical/geological relationships. The work adds to the previous research and advances the community's understanding of chloride material formation. The proposed conceptual model invoking a process that occurs in the dry valleys of antarctica is new and the authors have collected data to support this new hypothesis. Additionally, updated crater-age dates suggest the mechanism that formed the chlorides could have operated, in select locations, into the Amazonian further supporting past results that suggest chlorides are among the youngest deposits to have recorded relatively large scale hydrologic activity on the surface. I have a few general comments as well as specific comments that I hope the authors will consider in order to improve the impact of the manuscript.

- In general, the manuscript somewhat overstates that previous work proposed that the chlorides were formed similarly to a playa lake environment on earth. From my perspective there have been a few studies (e.g., Hynek et al. 2015) that did stress the "deep lake, long lived water", however my own research (Osterloo et al. 2008, 2010, 2015) did not strongly hypothesize that the environment was exactly like one would find on earth. Additionally, some of my later work specifically stated that the chlorides formed via evaporitic processes from surface runoff given the correlation of chloride deposits with channels and incised valleys. (e.g., Osterloo and Hynek, 46th LPSC abstract #1054). Of course, this is an LPSC abstract from 2015, so the authors may not be aware of it. In my opinion there have been a plethora of plausible formation mechanisms with some being favored over others. I think being specific on which previous work strongly hypothesized a mechanism over another and adding citations would be helpful. I have made some suggestions below.
We have refined the attributions of different formation mechanisms to particular authors, as suggested in the detailed suggestions provided below.

- The work presented has some really interesting new findings both in terms of ages of the deposits as well as a plausible formation mechanism. Stressing these over the previous formation mechanisms that are unlikely (e.g., volcanic outgassing) and perhaps even removing some of that text would help streamline the manuscript and aid in the readers overall understanding of "what's new about the chlorides" instead of revisiting old or out of date knowledge.
This is a good suggestion, and we have pared back the discussion of these mechanisms to a minimum.

- It may help to provide in the supplemental text a list of which deposits had CRISM coverage, which were used for age-dating, how many had channels, etc.

A summary database of the work would be very powerful for add on studies and would strength some of the statistics stated throughout.

We have added this supplemental data to CaltechDATA and it is referenceable via DOI.

Specific comments:

Line 68-70. Although it is somewhat true that chlorides and chloride mixtures were inferred due to the lack of spectral signature (e.g., rather spectrally featureless), there has also been modeling work that further constrains the most likely mineral(s) and abundances present in the chloride deposits. I would suggest modifying this to include the work of Jensen and Glotch 2011 (Jensen, H.B. and T.D. Glotch, (2011) Investigation of the near-infrared spectral character of putative Martian chloride deposits, JGR, doi:10.1029/2011JE003887)

This reference has been added

Line 183-188: Although Osterloo et al. 2010 (and 2008) used the map units to constrain the oldest ages of the chloride units, several studies have also undertaken crater age-dating to better understand the geologic ages of these materials. E.g., Osterloo et al. (2010) and Osterloo and Hynek (2015), Hynek et al. (2015) and potentially others. The authors do not discuss how their crater age dates compare with those completed previously. Although the LPSC abstract does not discuss specific areas that were used in the study, the general results are available for discussion. Several sites were presented in the 2010 study and Hynek (2015) also used crater age-dating for the site near Meridiani (and also inferred "late stage" formation). It would be useful if some of these previous sites were discussed in light of new crater age date estimates presented here.

Agree this is a good idea. We have added "Hynek et al. (2015) performed detailed mapping at one site in Merdiani and found an oldest bound of 3.6 Ga; Osterloo et al. (2010) find most chlorides within terrains of middle to late Noachian or early Hesperian age on global geologic maps but suggest further detailed site-specific age estimate determinations.**"**

Line 275. Note- the updated work of Osterloo and Hynek (2015) found a much higher percentage of fluvial channels and sinuous networks associated with the chloride materials as well (using higher resolution imagery).

Noted, though we have not changed the manuscript text (no statistics within that LPSC abstract to update our text)

Lines 297- Osterloo et al. 2010 never stated that the most likely formation mechanism was similar to a playa lake on earth. In fact, the stated hypothesis is evaporation from ponded water either from surface runoff or ground water upwelling. In that work, the lack of accessory minerals such as sulfates was noted as inconsistent with this type of environment. Furthermore, Glotch et al. (2010) concludes that the chlorides likely formed during the late Noachian to early Hesperian as a result of groundwater discharge and evaporation. They go on to say that a later event, involving groundwater or perhaps surface water, led to the formation of the chloride-bearing unit. The main point here is that there has been a plethora of previous work on the chloride materials, few have invoked a mechanism similar to playa lake formation on Mars so the authors need to take care when lumping previous works together. If the other works cited do specifically state that all of the chloride materials were formed in a similar way to playas on earth, then make the case for differences observed here and those typical of playas on earth. Another way to go about this if none of the cited works concluded playa formation, then simply discuss that playa formation is one potential mechanism and go on to provide the reasons why these materials would not be consistent given the slopes, etc.

We agree this was worded in a way that was a bit squishy, perhaps lumping when splitting would have been desirable. Because this was already covered in the Introduction, the references to prior formation mechanisms from particular papers were not strictly needed in this sentence, so we have simply removed the parenthetical citations. This also has the benefit of making the text more succinct. Now the sentence simply reads that the data are inconsistent with the characteristics of terrestrial playas.

Lines 381-397. These sections are quite short. Combining to a single paragraph or summarizing with a table would be more useful to the reader. Additionally when lumping deposits into distinct regions, it would be helpful for the reader to understand the quantity of deposits (or total volume, etc).

We have left as separate sections to allow description of each locality where we performed age dating, albeit very brief. We think this is important, simply to describe in words the nature of the inferred stratigraphic relationships. The volumetric quantity is not something that we consider particularly crucial for each individual deposit. The figure provided for each deposit allows an area calculation, if it is important for a future reader to create a new estimate beyond that already provided in the Osterloo et al (2010) database, which we did not attempt to update.

Line 400. Again, remind the reader here of what "all lines of evidence" are. And again, Osterloo and Hynek (2015) using higher resolution data also concluded that chloride formation via evaporitic processes from surface runoff was the dominant mechanism of formation.

The lines of evidence are indeed given the collection of sentences that immediately follow this topic sentence of the paragraph. We believe the context is clear so have not made a change.

Line 406. Be specific and cite sources instead of referring to "prior literature" given that the literature here is varied. Specifically, here, Hynek et al. 2015 would likely be the correct citation given that the work relied heavily on "thick chloride" deposits.

We agree this was vague, and we've removed references to prior literature here in the sentence as the specifics are given earlier in the manuscript and need not be repeated again here.

Line 443-448. Unless the authors have evidence that deliquescence is active on the martian surface, I think the discussion detracts from what is being proposed as the new idea of formation mechanism. The manuscript would probably be strengthen if the discussion simply focused on the new idea and not rehashing old ideas which the data do not support.
We had noted already in the 2 sentences of text that deliquescence is unlikely to alone be responsible but its contribution cannot be ruled out. We've retained this text because such deliquescence is observed at our Antarctic paternoster lake site, thus it is appropriate to acknowledge -- briefly -- a potential minor role but its inability to explain all characteristics.

Ube 459-463. Same as above.
We have removed. We agree that this sentence on non-plausibility of local volcanism was similar to a sentence in the introduction that already dismissed this hypothesis.

Line 505-507. Being specific here is key. How many are several? Adding a chart, table, or maybe graph showing the distribution of chloride units across the geologic units with age dates (from crater age-dating) overtop. Finding a single deposit on an Amazonian terrain is unique rather than the norm it would seem based on the data here. Certainly, it is plausible that these materials are much younger and they have simply formed on older terrains due to the geomorphology (and latitude range). However, as the authors state, it is not provable with current mechanism for age dating the materials. Nonetheless, we are constrained by what we have available so being as detailed as possible is important.
We changed "we show" to "we have shown" to make it clearer that this sentence summarizes the findings of this paper previously detailed earlier in the manuscript and in the supplement. This sentence refers to the several deposits discussed and reported in this paper with their age dates provided in the figures and text. As noted previously and also later in this paragraph, only some deposits have surfaces from which age estimates can be derived, so many/most only have broad no-younger-than bounds.

Supporting information:

I would suggest adding a table or chart detailing the results of the updated geologic age survey (using updated maps) as well as a table of how many deposits had crater age-dating analysis on them (and the results). Furthermore, it is not sufficiently clear if the crater age dates presented in the manuscript are examples of the analysis or the entirety of the analysis.

We include a summary table at the CaltechDATA doi that records the Tanaka et al. (2014) global map unit the chloride polygons are within. However, as noted in the text, when examined at the smaller regional scale of the deposits, there smaller geologic units not identified in the global map that underlie chloride deposits. Where underlying units could be clearly identified, we did CTX-scale crater counting, as described in the Methods. We have tweaked the language to try to make this clearer. "For all of the sites, we also searched for cross-cutting relationships that establish whether the chloride pre-dated, post-dated, or was contemporaneous with nearby units, *[sometimes smaller than those identified in global maps],* to improve understanding relative to simple reporting of occurrence in global-scale map units."

The results of the age dating are presented in their entirety, as described in former lines 346-356 and shown on the performed manuscript figures. Only certain deposits have clear superposition relationships that permit confident CTX/HiRISE scale analyses at scales smaller than the global maps.

Reviewer #2 Evaluations:
Recommendation: Return to author for minor revisions
Significant: The paper has some unclear or incomplete reasoning but will likely be a significant contribution with revision and clarification.
Supported: Mostly yes, but some further information and/or data are needed.
Referencing: Yes
Quality: Yes, it is well-written, logically organized, and the figures and tables are appropriate.
Data: Yes
Accurate Key Points: Yes

Reviewer #2 (Formal Review for Authors (shown to authors)):

Review of "Evidence for deposition of chloride on Mars from small-volume surface water events into the Late Hesperian-Early Amazonian" by Leask and Ehlmann
Review by Tim Glotch
This paper presents an exciting new analysis of chloride-bearing deposits on Mars and presents substantial evidence that these deposits were formed as a result of evaporation of shallow ponds in the late Hesperian/early Amazonian. These deposits may represent the last gasp of aqueous mineralization on Mars. The paper presents several important observations to support the main conclusions:
1) Substantially higher proportion of channels associated with chloride-bearing deposits than previously recognized.
2) Thin, draping textures of chloride deposits and absence of chlorides on slightly higher topographic rises.
3) Chlorides appear at a range of elevations in many areas, and in basins, occur

at higher elevations close to the inlet channels.
4) Crater counting reveals late-Hesperian to early-Amazonian ages, substantially younger than Tanaka et al. (2014) global map units on which they occur.
Overall, the paper is well written and organized and the figures are clear and legible. I recommend publication of the manuscript after minor revisions. I have several comments, all related to the geochemistry/mineralogy of the deposits and the materials that they were derived from that should be addressed by the authors and numerous small suggestions in the appended annotated pdf.

Major comments:
1) Lines 440-442: The authors cite volcanic outgassing as a potential source of Cl for the deposits. It is probably likely that precipitation due to volcanic outgassing would also be rich in sulfur, and that the sulfur and Cl would be present as aerosols vapor deposit coatings on dust/soil rather than well crystalline oxychlorine or sulfate phases. It would be helpful to provide some idea of how chloride and sulfate are segregated in such a scenario.
We have removed the sentence in question as it was confusing to provide this Cl enrichment reference in the "source of water" section. Now, the discussion is solely in the third paragraph of section 4.3, which describes the preferred scenario for source of Cl and chloride enrichment at the surface without sulfate enrichment.

2) Lines 489-492: It would be really helpful to be more quantitative here. Are there models in the literature that discuss the relative solubilities of chlorides and sulfates/carbonates that you can point to as supporting evidence? Could you run some relevant models in Geochemist's Workbench or a similar geochemical modeling software? In addition, if the Martian dust is a major source for leaching the Cl, I don't think it is clear what the Cl- and S-bearing phases are (aerosols/vapor deposits or crystalline phases?) or what their solubilities would be.
We have modified the discussion and added two additional references to make this clearer. While they may not be decoupled in deposition (if volcanic), we actually do expect Cl and S to be decoupled in many evaporative systems due to vastly different solubilities of their mineral precipitates as observed commonly on Earth. This has been observed by the Spirit rover, as referenced already in our text (Haskin et al., 2005). Gypsum is low solubility; halite is high solubility. Of course, the fluid chemical details matter as Mg sulfates have only slightly less solubility as halite and they are found together in a small section of the Curiosity deposits, whereas most of the deposits only have Ca sulfates with the Cl having been transported and precipitated into a mineral elsewhere (Rapin et al., 2019, Nat. Geosci). The ratios of cations and anions in a fluid dictate how they evolve with evaporation to generate "chemical divides" that lead to different evaporite sequences, as promulgated in the classic Hardie-Eugster paper series that various researchers are starting to extend to Mars (e.g., Toner et al., 2014; Tosca & McLennan, 2006); we have added

these two references. Determining the permitted range of fluid chemistries for the chloride-forming fluids and their evolution is a good subject for follow-on geochemical modeling work outside the scope of this paper (we are actually working on this with Tom McCollom now). Similarly, we agree that the Cl- and S-bearing phases in dusts and soils are undetermined – indeed, Ehlmann et al., 2017 JGR showed the x-ray amorphous phase(s) are the principal carrier of S and Cl in Bagnold dunes soils -- but such is also outside the scope of this paper. These uncertainties in starting phase are one key reason we promulgate a conceptual model here in this manuscript rather than a quantitative model as the geochemical modeling and underpinning assumptions are sufficiently multi-faceted to be a paper in their own right.

3) The manuscript does not address the known presence of other oxychlorine salts in the Martian regolith (perchlorate and likely chlorate; see Sutter et al., 2017 and references therein). What are the possible relationships, if any, between the known Cl-bearing salts in the Martian soil and the chloride deposits? In addition, what does the apparent presence of only anhydrous chloride salt say about the environmental conditions at the time of deposition? Hygroscopic and hydrated Mg-, Ca-, and Fe-bearing chlorides would likely be detectable by CRISM due to their hydration features, but are not apparent. Overall, the manuscript would be substantially strengthened by addressing some of the relevant geochemistry more quantitatively.

Reference: Sutter, B., R. C. Quinn, P. D. Archer, D. P. Glavin, T. D. Glotch, S. Kounaves, M. M. Osterloo, E. Rampe, and D. W. Ming (2017), Measurements of oxychlorine species on Mars, Int. J. Astrobio., 16, 203-217.

We thought about this comment, but we do not detect -ClO_4 or -$Cl*nH_2O$ species with the chlorides. We have added a sentence at the beginning of the Results section to state this explicitly. No large-scale perchlorate deposits have been detected on Mars, though it has been reported at ~<1% levels in soils. Why ClO_4 might exist in a solution in contact with Martian rock (with its reducing power) is actually a rather open question. Why it *and* Cl might exist together (if they do) is even more open. A paper we now cite, Toner et al. (2014), assumes these might co-exist and models brine evolution accordingly, but Toner et al give no explanation for why such solutions might come to be. Absent a plausible geochemical scenario in the literature for Cl/ClO_4 mixed redox state brines, it is beyond the scope for us to generate one here, so we have not changed our conceptual model in response to the comment, though it is an interesting one and highlights an open area in Martian aqueous geochemistry.

Reviewer #3 Evaluations:
Recommendation: Return to author for minor revisions

Significant: Yes, the paper is a significant contribution and worthy of prompt publication.
Supported: Yes
Referencing: Yes
Quality: Yes, it is well-written, logically organized, and the figures and tables are appropriate.
Data: Please Select
Accurate Key Points: Yes

Reviewer #3 (Formal Review for Authors (shown to authors)):

This paper provides a careful study of the context of chloride deposits on Mars, investigating both the non-chloride minerals associated with the deposits, as well as the topographic context and age of the underlying terrain. The work expands the range of likely standing water on Mars into the early Amazonian and presents a model for chloride deposit formation which can be tested by future work. Overall, I found the paper to be clearly written and well referenced. The arguments made in the paper are clearly supported by the data provided and the interpretations are well-reasoned. From my perspective, this is high quality research that is worthy of publication. Below, I've listed a few issues/questions that could be addressed to further strengthen the manuscript.

Lines 237-240 Chloride salts significantly decrease the freezing temperature, vapor pressure, and evaporation rate of liquid water. As ions become more concentrated, these effects are even greater. Therefore, the end-stage fluids responsible for precipitating these chloride deposits have very different thermodynamic properties that pure water. A discussion of the relative stability of near-saturated chloride brine would be a good addition here, as the addition of salts significantly expands the stability field of liquid water.
We agree. Rather than add to the Results section of phases accompanying chlorides, we have added to our conceptual model in section 4.3
"Additionally, the freezing point depression from halides may inhibit freezing of the briny water (e.g., Clark and van Hart, 1981), facilitating its downstream transport."

Figure 6 caption: typo in ejecta blanket.
Fixed

Line 406-407: this sentence is very awkward - consider rephrasing to read : Our thickness estimates (<3m and typically <1m) are lower than those reported in the prior literature, but are also consistent with smaller volumes of water.
We have simplified the sentence

Lines 476-477. There is a disconnect in the math here, as a circle with a diameter of 10 km has an area of 78 km^2, which is <10% of the area needed in the mass balance calculations. This needs to be clarified or the argument reframed.

Our methods and Figure 1 describe the buffer as 10km around the Osterloo et al., 2010 chloride deposit delineation. Thus, the minimum size would be 314 km^2 (circle of radius >=10km), but as none of the Osterloo deposits are a mere point and many are multiple tens to hundreds of square kilometers, our statement that the typical deposit is ~1000 km^2 is correct. We have changed the location of the reference to Figure 1 (which has an example of a buffer zone) in the sentence to hopefully provide a clearer pointer to readers to understand how the 1000km2 area looks relative to the chlorides.

Line 495: typo- soilts
Fixed

Figure 11. This is an interesting model which led me to wonder- do you observe more chloride deposits on the pole-facing slopes where snow/ice is more likely to accumulate?
This is an interesting question. In general, the chlorides themselves are not on a slope as they are in a local, relatively flat, topographic low. However, the reviewer asks an interesting question as to whether the feeder channels preferentially feed from pole-facing slopes instead of equator-facing. We did not keep the statistics to answer this question comprehensively. In our quick glance at the channels in figures in our paper, most (~2/3rd) of feeder channels are E-W or W-E rather than S-N, so it is not obvious that there is a preference for poleward facing slopes. This could be an area for a future study.

Line 549 Calculates should be calculations
Fixed

Line 556 Not clearer what you mean by "our update of the statistics of distribution". More specifics/explanation would be helpful here.
We have changed to simply “our update of statistics”

Line 559 crater counting "of" local units
Fixed