AGU Advances

Peer Review History of

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

E. K. Leask[1] and B. L. Ehlmann[1]

[1] Division of Geological and Planetary Sciences, California Institute of Technology, Pasadena, CA, USA

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Author Response to Peer Review Comments

Peer Review Comments on 2021AV000534

Reviewer #1

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.
- 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.
- 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.

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)

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.

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).

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.

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).

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.

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.

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.

Ube 459-463. Same as above.

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.

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.

Reviewer #2

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.
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.
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.

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.

Figure 6 caption: typo in ejecta blanket.

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.

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.

Line 495: typo- soilts

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?

Line 549 Calculates should be calculations

Line 556 Not clearer what you mean by "our update of the statistics of distribution". More specifics/explanation would be helpful here.

Line 559 crater counting "of" local units

[Please see attachment that begins on the following page.]