Peer Review File

Manuscript Title: Sustained wet-dry cycling on early Mars

Reviewer Comments & Author Rebuttals

Reviewer Reports on the Initial Version:

Referees' comments:

Referee #1 (Remarks to the Author):

Review of “Early Mars sub-aerial environments conducive to prebiotic evolution” by Rapin et al.

This paper describes observations of polygonal patterns at Gale crater and links these to wet/dry cycling and the possibility of evolution of life during the Noachian to Hesperian transition on Mars. This study extends previous investigations at Gale crater by providing details on the shapes of cracking patterns and associating these with evaporative processes and sulfate activity. The jump to the presence of microbes in these mudstones is speculative, but consistent with the observations. The hypothesis that prebiotic evolution could have been possible at sites containing polygonal cracking at Gale crater is a step forward from the ”habitable conditions” cited frequently in the Mars literature and is a novel perspective worthy of publication and further consideration.

I suggest clarifying the types of features observed in the smectite-bearing regions versus the sulfate-bearing regions of Gale crater in order to better document the transition from Noachian to Hesperian. This paper would also benefit from citing additional literature on related processes at Mars and on smectite-facilitated polymerization of organics. I think this study would make an excellent publication for Nature following a few minor revisions.

If the authors have questions about this review, they are welcome to contact me directly: Janice Bishop, jbishop@seti.org.

General Comments:

1) First, I would like to point out that smectites (or swelling clays) are the dominant type of clay mineral observed at Gale crater and across Mars. It is important to note that the “clays” at Gale are smectites for a few reasons. Clay minerals actually have many different formation conditions and properties, while smectites are consistent with formation under moderate temperatures and neutral waters (as opposed to kaolinite, serpentine, chlorite, mica, etc that form under other conditions, less supportive of life).

2) Further, smectites typically form in wet/dry cycling environments (e.g., Chamley, 1989) and are also associated with polygonal cracking on Mars (e.g. Bishop et al., 2008; McKeown et al., 2013).

3) Another point of interest for this study is that smectites have been associated with polymerization

of organics to form complex molecules and even RNA (see Bishop et al., 2013 and references therein – especially Ferris & Hagan,1986; Ferris, 2005; and Pinnavaia et al., 1986).

4) I suggest clarifying your points about the two kinds of surfaces: 1) the smooth polygons observed in the sulfate-poor basaltic material containing smectites, and 2) the polygonal ridges and nodules observed in the outcrops containing abundant Ca and Mg sulfates.

5) My perspective on your results is that wet/dry cycling during the Noachian likely supported formation of the smectite-bearing muds with polygonally cracked patterns in smooth surfaces and that organics may have been concentrated and polymerized during that time. Subsequently, as salts were concentrated and sulfates formed, the nodules and ridges along the polygons formed. The presence of sulfates may have provided an energy source for microbes if they arose at that time.

Specific Comments:

Suggested edits related to grammar and word choice are marked directly on the text file included with this review.

Line 51 – this would be a good place to mention that smectites are also associated with wet/dry cycling and reaction of organics

Line 97 – this would be a good place to cite the Bishop et al. (2021) suggestion of salts including Ca sulfates producing cracking and uplift of martian soils.

Something like this: "This model is consistent with a recent study documenting cracking and uplift of gypsum-bearing soils and sediments on Earth, proposed as a process shaping the martian surface through formation of small cracks, ridges, and nodules (Bishop et al., 2021)." However the small-scale surface ridges and nodules formed through brine action was hypothetical in that study - and now you have proof of this hypothesis!

Line 112 – please provide the sedimentation rate here

Line 112 – this would be a good place to cite the Bishop et al. (2018) study indicating smectites formed in warm waters during the Noachian over thousands to millions of years.

Something like this: "Wet/dry cycling is the favored environment for smectite formation (Chamley, 1989) and a time period of thousands to millions of years was proposed for smectite formation in warm waters on Mars (Bishop et al., 2018)."

Line 131 – this would be a good place to cite identification of smectite at Gale, polymerization of organics by smectites, the potential for smectites to support formation of life and transitions on Mars from smectites to sulfates.

Something like this: “Further, dioctahedral smectites similar to those present at Gale crater (Bristow et al., 2018) facilitate polymerization of organics and formation of nucleotides and even RNA (Ferris, 2005). Because smectites are ubiquitous across Mars and form in environments supportive of life (Bishop et al., 2013) and many of the aqueous outcrops transition from Fe-rich smectite to Al-rich phyllosilicates and sulfates (Carter et al., 2015; Murchie et al., 2019), then if our hypothesis that this transition supports prebiotic evolution is correct, this process could be more widespread than just Gale crater.”

References the authors may find useful in their revisions”

Bishop J. L., Noe Dobrea E. Z., McKeown N. K., Parente M., Ehlmann B. L., Michalski J. R., Milliken R. E., Poulet F., Swayze G. A., Mustard J. F., Murchie S. L. & Bibring J.-P. (2008) Phyllosilicate diversity and past aqueous activity revealed at Mawrth Vallis, Mars. Science, 321, 830-833.

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Bishop J. L., Fairén A. G., Michalski J. R., Gago-Duport L., Baker L. L., Velbel M. A., Gross C. & Rampe E. B. (2018) Surface clay formation during short-term warmer and wetter conditions on a largely cold ancient Mars. Nature Astronomy, 2, 206-213.

Bishop J. L., Yeşilbaş M., Hinman N. W., Burton Z. F. M., Englert P. A. J., Toner J. D., McEwen A. S., Gulick V. C., Gibson E. K. & Koeberl C. (2021) Martian subsurface cryosalt expansion and collapse as trigger for landslides. Science Advances, 7, abe4459.

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Chamley H. (1989) Clay Sedimentology. Springer-Verlag, New York.

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Ferris J. P. & Hagan Jr. W. J. (1986) The adsorption and reaction of adenine nucleotides on montmorillonite. Origins of Life and Evolution of the Biosphere, 17, 69-84.

McKeown N. K., Bishop J. L. & Silver E. A. (2013) Variability of rock texture and morphology correlated with the clay-bearing units at Mawrth Vallis, Mars. Journal of Geophysical Research, 118, doi: 10.1002/jgre.20096.

Murchie S. L., Bibring J. P., Arvidson R. E., Bishop J. L., Carter J., Ehlmann B. L., Langevin Y., Mustard J. F., Poulet F., Riu L., Seelos K. D. & Viviano C. E. (2019) Chapter 23. Visible to short-wave Infrared spectral analyses of Mars from orbit using CRISM and OMEGA. Pp. 453-483 in: Remote Compositional Analysis: Techniques for Understanding Spectroscopy, Mineralogy, and Geochemistry of Planetary Surfaces (J.L. Bishop, J.E. Moersch & J.F. Bell III, editors). Cambridge University Press, Cambridge, UK.

Pinnavaia T. J. & Mortland M. M. (1986) Clay Minerals and the Origin of Life. Pp. 131-135 (A.G. Cairns-Smith & H. Hartman, editors). Cambridge University Press.

Referee #2 (Remarks to the Author):

Review of: "Early Mars sub-aerial environments conducive to prebiotic evolution", by: W. Rapin et al., Submitted to Nature, Manuscript # 2022-11-17709.

Review by: Tim Goudge, The University of Texas at Austin

Date: 12/9/2022

Paper Summary:
In this manuscript, the authors present analyses of Mars Science Laboratory (MSL) observations of newly-explored sulfate-bearing outcrops identified as a key paleoenvironmental transition from orbit. The authors focus on two primary observations: (1) identification of polygonal ridges with Y-junctions that are interpreted as mud cracks from repeated wet-dry cycling; and (2) geochemical observations that the authors interpret to indicate enrichments of sulfate evaporites within the polygonal ridges (and associated nodules). The authors use these two observations to conclude that a nearly 20 m thick section of strata record wet-dry cycling in ephemeral lakes over a period of thousands to millions of years, and note in particular the potential importance of this cycling for prebiotic processes and the potential for the origin of life on Mars.

I found this paper well written, and the observations interesting and novel, with potential for broadly informing our understanding of Mars' geologic evolution, thus making this contribution potentially suitable for eventual publication in Nature. However, I do have some notable concerns about the presented science, including on the robustness of the constraints for the link to prebiotic processes, as well as the novelty of the results. These comments are laid out below, and depending on how they are addressed, this manuscript may become suitable for publication in Nature.

I hope that this review finds all those involved with this manuscript safe and healthy, and please do not hesitate to reach out for clarification if you have any questions on the comments below.

Review Comments:

(1) I think my largest concern, which has a few intertwined parts, relates to the overall conclusion that the observations indicate a record of geologically significant periods of time where the Gale crater environment experienced wet-dry cycling.

First, while the observations shown in Figure 1 are quite convincing examples of Y-junction-dominated mud cracks (although see Comment #3 below), as far as I can tell, these are all taken from the Western Section. The observations from the Eastern Section (Extended Data Figure 9) and Medium (middle) Section (Extended Figure 10) are far less convincing (not really convincing at all) as examples of polygonal mud cracks. Both of these sections look much more like nodule-dominated mudstone, which could easily be indicative of diagenetic processes, and not necessarily wet-dry cycling. This concern should be addressed head-on by the authors, showing why the most reasonable interpretation is that these sections represent mud cracks formed by wet-dry cycles.

The reason this then becomes so important is because of the estimate of stratigraphic thickness, which is ultimately used as a proxy to derive a timescale estimate. On lines 109-111 the authors indicate that the section recording wet-dry cycling is 18 m thick. It is never explicitly detailed where this number comes from (something that should be rectified), but as far as I can tell this is based on the elevation difference between the upper mud crack section (West) and the lower mud crack section (East), as shown in the stratigraphic column in Extended Data Figure 3. There are two concerns I have with this. First is the fact that I am not convinced the section used as a lower bound in the East is actually representative of mud cracks (see above). Second, there are significant sections of strata in that 18 m interval that are either covered or record only nodules, not mud cracks, so it seems a significant leap to conclude that the whole interval records wet-dry cycling? Even if the East Section does contain mud cracks, what is the evidence that the whole section between them records wet-dry cycling, and that these are not two distinct sections recording wet-dry cycling separated either by time or space? This question of how thick the section is that records mud cracks is extremely critical to the primary conclusion of the manuscript – that these strata record a geologically significant era of wet-dry cycling. So, a much better job needs to be done detailing the stratigraphy to show the reader that this truly is an 18 m thick section that exclusively/uniquely records evidence for wet-dry cycling. Presently (to this reviewer at least) there is only a convincing case of one, few m thick interval of wet-dry cycling, which presents a very different outlook.

Moving beyond stratigraphic thickness, I think the authors also need to elaborate on how they are using this as a proxy for time. Right now the logic of going from stratigraphic thickness to time is sparsely detailed, and uses only global compilation data that actually are meant to show how stratigraphic thickness correlates with sediment accumulation rate, and so is not actually a good, independent proxy for age… The approach for how section thickness is converted to time should thus be expanded somewhat (even if in the Methods section). Additionally, if the authors choose to keep their focus strongly on the implications for relevance to prebiotic processes, I think a more direct comparison is needed for these timescales. Right now the "timescale considered favorable to prebiotic conditions" (lines 114-115) is not explicitly stated.

Finally, while I understand why the authors choose to link their results to prebiotic processes, and indeed agree that this is an interesting and provocative implication of their conclusions, I might humbly suggest that this aspect could get toned down slightly in a revised version. Instead of framing the entire paper around this concept, the authors could focus equally on the hydro-climate implications of prolonged intervals of wet-dry cycling (which presently are not explored in significant detail, but are also of great interest!), as well as the astrobiology implications for prebiotic processes.

(2) I do not find the geochemical data shown in Figure 2 overly convincing. To me, there looks like significant overlap between the polygonal ridges/nodular bedrock and the host bedrock. Sure the latter has the lowest values, but there are also many datapoints of the host bedrock well-within the main data cloud for the nominally S-enriched polygonal ridges/nodular bedrock. I think the authors could spend more time robustly showing this geochemical distinction, e.g., even a simple distribution test like a Mann-Whitney U would be something.

(3) I was also disappointed to see that the critical observation for why the desiccation features record multiple wet-dry cycles, and not just a single drying event – the presence of Y-junctions, not T-junctions – has no quantitative data associated with it. This reviewer agrees with the impression of the authors that this is the case based on Figures 1b and 1e in particular; however, I have no sense of how representative this is. This seems like a relatively easy, although perhaps tedious and time-consuming, fix, where the authors can spend some time mapping out the polygons and measuring junction angles, to ultimately present a distribution (similar to the size distribution shown in Extended Data Figure 4) that (presumably) shows a modal angle near 120°.

(4) As stated on line 55, there are already multiple publications (three cited) that interpret seasonal drying of a Gale crater lake. I understand that these new observations are from a different stratigraphic interval, but I was unclear from reading this paper how the current manuscript otherwise stands apart from these previous contributions? If we already knew there were wet-dry cycles at some point in the record of Gale stratigraphy, what is the new and exciting thing here that makes the paper broadly appealing for the Nature audience (aside from the link to prebiotic processes, which seems an argument that could otherwise be applied to the previous publications, no?)? I am not trying to be dismissive with these questions, but rather would encourage the authors to spend some time expanding on the novelty of this set of contributions, and how they are distinct from previous interpretations of seasonal drying recorded at Gale crater.

(5) Two additional, more minor comments:
--On lines 103-104 it is suggested that desiccation cracks affect only the upper most few cm, yet on lines 65-66 it is suggested that the polygons extend to tens of cm depth based on their "stepped

appearance". For the latter observations, is it possible that this stepped appearance doesn't represent differential erosion of one 10s of cm thick interval, but rather pre-desiccation topography with 10s of cm of relief? That might reconcile the two points mentioned above?
--On line 105, replace "in consistence" with "consistent".

Author Rebuttals to Initial Comments:

Both referee 1 and 2 found the findings novel, with broad implications for the geological evolution of Mars, and potentially suitable for publication in Nature. Referee 1 suggested only a few minor revisions before publication, while Referee 2 expressed concern about the robustness of the interpretations and about the novelty of the results that need to be addressed.

Inline answers to referees' comments:

Referees' comments:

Referee #1 (Remarks to the Author):

Review of "Early Mars sub-aerial environments conducive to prebiotic evolution" by Rapin et al.

This paper describes observations of polygonal patterns at Gale crater and links these to wet/dry cycling and the possibility of evolution of life during the Noachian to Hesperian transition on Mars. This study extends previous investigations at Gale crater by providing details on the shapes of cracking patterns and associating these with evaporative processes and sulfate activity. The jump to the presence of microbes in these mudstones is speculative, but consistent with the observations.

We have refocused the introduction and toned down the implications on prebiotic evolution (also per editor recommendation) in order to avoid confusion with that regard. We never meant to convey that these mudstones are supposed to contain microbes, our findings only highlight that these polygonal features are indicative of ancient surface environments/processes that favor early polymerization of organic molecules. This represents a new step in understanding Mars' potential for prebiotic record but not at all evidence for microorganisms.

The hypothesis that prebiotic evolution could have been possible at sites containing polygonal cracking at Gale crater is a step forward from the "habitable conditions" cited frequently in the Mars literature and is a novel perspective worthy of publication and further consideration.

We thank the referee for this appreciation of the findings that gives a full sense/value to previous evidence of "habitable conditions". These habitable conditions are needed to ensure past sustainability of life, but pending on the fact that life had already emerged, which first starts by searching and examining environments that could have been conducive to prebiotic evolution.

I suggest clarifying the types of features observed in the smectite-bearing regions versus the sulfate-bearing regions of Gale crater in order to better document the transition from Noachian to Hesperian. This paper would also benefit from citing additional literature on related processes at Mars and on smectite-facilitated polymerization of organics. I think this study would make an excellent publication for Nature following a few minor revisions.

We considered adding a discussion with other polygonal features identified orbitally and thank the referee for pointing out that perspective. We decided to focus on the *in situ* evidence of this smaller polygonal pattern only here for reasons mentioned in answer to general comment 5 below.

If the authors have questions about this review, they are welcome to contact me directly: Janice Bishop, jbishop@seti.org.

We would like to thank the referee for the suggestions and constructive comments regarding the text which helped improve the paper.

General Comments:

1) First, I would like to point out that smectites (or swelling clays) are the dominant type of clay mineral observed at Gale crater and across Mars. It is important to note that the "clays" at Gale are smectites for a few reasons. Clay minerals actually have many different formation conditions and properties, while smectites are consistent with formation under moderate temperatures and neutral waters (as opposed to kaolinite, serpentine, chlorite, mica, etc that form under other conditions, less supportive of life).

> We thank the referee for the insightful comment regarding clays that are indeed part of the mineralogy at Gale. We updated the text to smectites or smectite clays where appropriate, this is indeed valid for both Gale crater, where smectites were observed using XRD (Bristow et al. 2018), and more globally for Mars from orbital mapping of Fe/Mg smectites near infrared spectroscopy as highlighted in figure 4 (Elhmann and Edwards 2014).

2) Further, smectites typically form in wet/dry cycling environments (e.g., Chamley, 1989) and are also associated with polygonal cracking on Mars (e.g. Bishop et al., 2008; McKeown et al., 2013).

> We thank the referee for pointing out other perspectives and potential links to consider. See answer to specific comment below.

3) Another point of interest for this study is that smectites have been associated with polymerization of organics to form complex molecules and even RNA (see Bishop et al., 2013 and references therein – especially Ferris & Hagan,1986; Ferris, 2005; and Pinnavaia et al., 1986).

> We incorporated this reference into the text, see specific answer to comment below. We also added a reference to a most recent paper that more specifically refers to Fe-rich smectites -nontronites-, similar to what has been found out in Gale.

Pedreira-Segade, U., Feuillie, C., Pelletier, M., Michot, L. J., & Daniel, I. Adsorption of nucleotides onto ferromagnesian phyllosilicates: Significance for the origin of life. Geochimica et Cosmochimica Acta 176, 81-95 (2016).

4) I suggest clarifying your points about the two kinds of surfaces: 1) the smooth polygons observed in the sulfate-poor basaltic material containing smectites, and 2) the polygonal ridges and nodules observed in the outcrops containing abundant Ca and Mg sulfates.

> We have reviewed and slightly updated the text describing chemical composition to indicate nodular bedrock, and smooth host bedrock more clearly. Sulfates are traceable in bedrock using

sulfur and related cations Ca+Mg (Fig. 2) more easily than smectites that are silicates and more difficult to identify by chemical analyses when mixed within a siliciclastics matrix. Enrichment in smectites, whether it could be specifically in the smooth host bedrock or in the ridges and other nodular features, cannot be identified using our analysis, but enrichment in Ca and Mg-sulfates is clearly observed. Unfortunately no drilling to acquire samples for XRD was performed in the polygon-bearing interval. Due to this issue we prefer not to comment on the smectite content of the bedrock.

5) My perspective on your results is that wet/dry cycling during the Noachian likely supported formation of the smectite-bearing muds with polygonally cracked patterns in smooth surfaces and that organics may have been concentrated and polymerized during that time. Subsequently, as salts were concentrated and sulfates formed, the nodules and ridges along the polygons formed. The presence of sulfates may have provided an energy source for microbes if they arose at that time.

> We considered the possibility to link our findings to other polygonal patterns observed orbitally, if we understood the referee's comment correctly, but we find it rises multiple issues: the scale of the patterns is significantly different with meters to decameter scale orbitally observed cracks covering large exposed areas, versus centimeter scale polygons buried within specific strata. The orbitally observed patterns are identified as "potential desiccation polygons", indeed often associated with orbitally identified smectite, and their formation may be related to desiccation of smectite rich sediments upon their exposure to the late-stage hyper arid climate (El-Maarry et al. 2014). No evidence for fossil desiccation-related polygonal cracks have been documented in the smectite-bearing regions of Gale, e.g. in the Sheepbed Mudstone Member for which "cement-filled synaeresis cracks, rather than sediment desiccation cracks suggest that it was perennially wet for the stratigraphic interval" (Grotzinger et al, Science, 2013). Due to the difference of scale and the lack of *in situ* observations that could favor an evidence for ancient desiccation process or clearly discriminate between other potential formation processes we prefer to not compare our results with these orbital polygonal patterns.

Specific Comments:

Suggested edits related to grammar and word choice are marked directly on the text file included with this review.

Line 51 – this would be a good place to mention that smectites are also associated with wet/dry cycling and reaction of organics

> We agree with the referee for smectites also favoring reaction with organics, see changes to text further below.

Line 97 – this would be a good place to cite the Bishop et al. (2021) suggestion of salts including Ca sulfates producing cracking and uplift of martian soils.
Something like this: "This model is consistent with a recent study documenting cracking and uplift of gypsum-bearing soils and sediments on Earth, proposed as a process shaping the martian surface through formation of small cracks, ridges, and nodules (Bishop et al., 2021)." However the small-scale surface ridges and nodules formed through brine action was hypothetical in that study - and now you have proof of this hypothesis!

> The paper (Bishop et al., 2021) is focused on "interactions of sulfates and chlorine salts in fine-grained soils on Mars could absorb water, expand, deliquesce, cause subsidence, form crusts, disrupt surfaces, and ultimately produce landslides after dust loading on these unstable surfaces". Although salt deposition is part of our model, the polygonal pattern in our finding can only result from desiccation. As mentioned in our text (line 74-78) ridges due the formation of salts also form polygonal patterns in extremely saline evaporitic settings but of different scale, lithology and composition.

Line 112 – please provide the sedimentation rate here

> We have added the range of existing sedimentation rates from strata of lacustrine settings of similar thickness "0.01 - 10 mm/yr ", and also checked that aggregation rates of floodplain strata are included within this range. We added the corresponding references to the text.

Line 112 – this would be a good place to cite the Bishop et al. (2018) study indicating smectites formed in warm waters during the Noachian over thousands to millions of years.
Something like this: "Wet/dry cycling is the favored environment for smectite formation (Chamley, 1989) and a time period of thousands to millions of years was proposed for smectite formation in warm waters on Mars (Bishop et al., 2018)."

> We prefer to not comment in this part of the text about smectite formation as smectites may have a large variety of origins (e.g. Deconink & Chamley, Diversity of smectite origins in Late Cretaceous Sediments, Clay Mineralogy, 1995). Also it would not be the purpose of our paper to make a full comparative review of processes/materials conducive to polymerization of organics, yet we added a reference to smectites with that regard further below, see answer to comment below.

Line 131 – this would be a good place to cite identification of smectite at Gale, polymerization of organics by smectites, the potential for smectites to support formation of life and transitions on Mars from smectites to sulfates.
Something like this: "Further, dioctahedral smectites similar to those present at Gale crater (Bristow et al., 2018) facilitate polymerization of organics and formation of nucleotides and even RNA (Ferris, 2005). Because smectites are ubiquitous across Mars and form in environments supportive of life (Bishop et al., 2013) and many of the aqueous outcrops transition from Fe-rich smectite to Al-rich phyllosilicates and sulfates (Carter et al., 2015; Murchie et al., 2019), then if our hypothesis that this transition supports prebiotic evolution is correct, this process could be more widespread than just Gale crater."

> We thank the referee for pointing out to us the contribution of smectites in prebiotic processes, and we have reformulated this addition to the text based on the referee suggestion (line 150-153).

References the authors may find useful in their revisions"

Bishop J. L., Noe Dobrea E. Z., McKeown N. K., Parente M., Ehlmann B. L., Michalski J. R., Milliken R. E., Poulet F., Swayze G. A., Mustard J. F., Murchie S. L. & Bibring J.-P. (2008) Phyllosilicate diversity and past aqueous activity revealed at Mawrth Vallis, Mars. Science, 321, 830-833.

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McKeown N. K., Bishop J. L. & Silver E. A. (2013) Variability of rock texture and morphology correlated with the clay-bearing units at Mawrth Vallis, Mars. Journal of Geophysical Research, 118, doi: 10.1002/jgre.20096.

Murchie S. L., Bibring J. P., Arvidson R. E., Bishop J. L., Carter J., Ehlmann B. L., Langevin Y., Mustard J. F., Poulet F., Riu L., Seelos K. D. & Viviano C. E. (2019) Chapter 23. Visible to short-wave Infrared spectral analyses of Mars from orbit using CRISM and OMEGA. Pp. 453-483 in: Remote Compositional Analysis: Techniques for Understanding Spectroscopy, Mineralogy, and Geochemistry of Planetary Surfaces (J.L. Bishop, J.E. Moersch & J.F. Bell III, editors). Cambridge University Press, Cambridge, UK.

Pinnavaia T. J. & Mortland M. M. (1986) Clay Minerals and the Origin of Life. Pp. 131-135 (A.G. Cairns-Smith & H. Hartman, editors). Cambridge University Press.

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Paper Summary:
In this manuscript, the authors present analyses of Mars Science Laboratory (MSL) observations of newly-explored sulfate-bearing outcrops identified as a key paleoenvironmental transition from orbit. The authors focus on two primary observations: (1) identification of polygonal ridges with Y-junctions that are interpreted as mud cracks from repeated wet-dry cycling; and (2) geochemical observations that the authors interpret to indicate enrichments of sulfate evaporites within the polygonal ridges (and associated nodules). The authors use these two observations to conclude that a nearly 20 m thick section of strata record wet-dry cycling in ephemeral lakes over a period of thousands to millions of years, and note in particular the potential importance of this cycling for prebiotic processes and the potential for the origin of life on Mars.

I found this paper well written, and the observations interesting and novel, with potential for broadly informing our understanding of Mars’ geologic evolution, thus making this contribution potentially suitable for eventual publication in Nature. However, I do have some notable concerns about the presented science, including on the robustness of the constraints for the link to prebiotic processes, as well as the novelty of the results. These comments are laid out below, and depending on how they are addressed, this manuscript may become suitable for publication in Nature.

I hope that this review finds all those involved with this manuscript safe and healthy, and please do not hesitate to reach out for clarification if you have any questions on the comments below.

We would like to thank the referee for the detailed and constructive comments regarding the text and figures leading to significant improvements.

Review Comments:
(1) I think my largest concern, which has a few intertwined parts, relates to the overall conclusion that the observations indicate a record of geologically significant periods of time where the Gale crater environment experienced wet-dry cycling.
First, while the observations shown in Figure 1 are quite convincing examples of Y-junction-dominated mud cracks (although see Comment #3 below), as far as I can tell, these are all taken from the Western Section. The observations from the Eastern Section (Extended Data Figure 9) and

Medium (middle) Section (Extended Figure 10) are far less convincing (not really convincing at all) as examples of polygonal mud cracks. Both of these sections look much more like nodule-dominated mudstone, which could easily be indicative of diagenetic processes, and not necessarily wet-dry cycling. This concern should be addressed head-on by the authors, showing why the most reasonable interpretation is that these sections represent mud cracks formed by wet-dry cycles.

The reason this then becomes so important is because of the estimate of stratigraphic thickness, which is ultimately used as a proxy to derive a timescale estimate. On lines 109-111 the authors indicate that the section recording wet-dry cycling is 18 m thick. It is never explicitly detailed where this number comes from (something that should be rectified), but as far as I can tell this is based on the elevation difference between the upper mud crack section (West) and the lower mud crack section (East), as shown in the stratigraphic column in Extended Data Figure 3. There are two concerns I have with this. First is the fact that I am not convinced the section used as a lower bound in the East is actually representative of mud cracks (see above). Second, there are significant sections of strata in that 18 m interval that are either covered or record only nodules, not mud cracks, so it seems a significant leap to conclude that the whole interval records wet-dry cycling? Even if the East Section does contain mud cracks, what is the evidence that the whole section between them records wet-dry cycling, and that these are not two distinct sections recording wet-dry cycling separated either by time or space? This question of how thick the section is that records mud cracks is extremely critical to the primary conclusion of the manuscript – that these strata record a geologically significant era of wet-dry cycling. So, a much better job needs to be done detailing the stratigraphy to show the reader that this truly is an 18 m thick section that exclusively/uniquely records evidence for wet-dry cycling. Presently (to this reviewer at least) there is only a convincing case of one, few m thick interval of wet-dry cycling, which presents a very different outlook.

Moving beyond stratigraphic thickness, I think the authors also need to elaborate on how they are using this as a proxy for time. Right now the logic of going from stratigraphic thickness to time is sparsely detailed, and uses only global compilation data that actually are meant to show how stratigraphic thickness correlates with sediment accumulation rate, and so is not actually a good, independent proxy for age… The approach for how section thickness is converted to time should thus be expanded somewhat (even if in the Methods section). Additionally, if the authors choose to keep their focus strongly on the implications for relevance to prebiotic processes, I think a more direct comparison is needed for these timescales. Right now the "timescale considered favorable to prebiotic conditions" (lines 114-115) is not explicitly stated.

Finally, while I understand why the authors choose to link their results to prebiotic processes, and indeed agree that this is an interesting and provocative implication of their conclusions, I might humbly suggest that this aspect could get toned down slightly in a revised version. Instead of framing the entire paper around this concept, the authors could focus equally on the hydro-climate implications of prolonged intervals of wet-dry cycling (which presently are not explored in significant detail, but are also of great interest!), as well as the astrobiology implications for prebiotic processes.

> We thank the referee for pointing out the issues regarding statements on stratigraphy and we proceeded to multiple changes to this section and updated figures:

Thicknesses and intervals: In the initial statement we indicated "intermittent signature" to refer to the polygons being not continuously present over the entire interval where they are found, we now changed to "Polygonal ridges are identified at multiple locations within an 18 m thick stratigraphic interval" to more clearly reflect the observations. This is now also mentioned in the descriptive paragraph above with reference to updated Extended Data Fig. 3 (lines 61-63). The Extended Data figure now indicates the thickness of the interval where polygons are identified at multiple locations. The text also now clearly differentiates with the 2 m elevation of the subinterval where preserved polygonal pattern is observed to have propagated as seen in the distance in updated Extended Data Fig. 2 (as far as camera resolution allows) and illustrated range with updated Extended Data Fig. 4b.

Clarity about other locations with polygonal ridges: We updated the accompanying Extended Data Fig. 9, 10 and 11 to expand and improve the detailed views on other locations with polygonal ridges. We also created Extended Data Fig. 12 to highlight these variably altered lithologies with and without markings. Although these locations are less prominent, their lithology has evidence for "incipient and altered variants of these patterns" as mentioned in the text (line 63) and now traced in the figures using dotted red lines to highlight nodular ridged patterns. The degree of incipience and preservation on these ridges where they are identified varies significantly across the section which is not surprising given their early pedogenic formation process. They can be altered and removed by various later diagenetic and exhumation processes as evidenced by cross cutting relationships and removal as decimeter large halos around large fracture-filled veins on the location of their most prominent observation (Fig. 1f white overlay highlights the presence of veins and ridges fading away near the fractures). Importantly, polygonal ridges at other locations, although less prominent, show similar characteristics in terms of size and angles.

Thickness and timescales: We thank the referee for pointing out the unclear statements on duration. The text now acknowledges that the timespan involved is "uncertain" and for an indicative duration the text now has sedimentation rates pointed out from the literature of the corresponding analogs for the deposits, either "floodplain and lake margin" obtained from Sadler 1981 for lacustrine settings and encompassed by floodplain aggradation rates obtained from Kraus 1999 and Daniels 2003 added references, which both cover the plausible depositional models for the strata. We removed the "timescale considered favorable to prebiotic conditions" which was based on the geological duration of the ponds or surface macrobionts (years to centuries) used in the Clark et al., (2020) reference to investigate their statistical potential for prebiotic evolution. We have updated the text to tone down the implications on prebiotic evolution and now only refer to the estimates as an initial indication to appreciate the possible scale of the process recorded from the observations.

Hydro-climate implications: We also thank the referee for the constructive comment on the implications that we used as inspiration to refocus the introduction (lines 40-49) and improve the discussion on the implications. We now first comment on the new perspectives the discovery offers with regard to the current understanding of climate regimes of early Mars (lines 129-135), and now only after, as a second step, our text comments on the implications for prebiotic evolution. We do not engage in quantitative implications on paleoclimates as it would require a numerical modeling of the regional hydrology for which ongoing research is being developed beyond our group dedicated to this geological finding. We now highlight first and foremost the significance of the discovery: the first tangible evidence for sustained wet-dry cycling in early Mars sub-aerial environments, and shortly discuss its implications that point to future research and exploration.

(2) I do not find the geochemical data shown in Figure 2 overly convincing. To me, there looks like significant overlap between the polygonal ridges/nodular bedrock and the host bedrock. Sure the latter has the lowest values, but there are also many datapoints of the host bedrock well-within the main data cloud for the nominally S-enriched polygonal ridges/nodular bedrock. I think the authors could spend more time robustly showing this geochemical distinction, e.g., even a simple distribution test like a Mann-Whitney U would be something.

> We agree with the referee that the initial Fig. 2 is confusing, as effects of horst rock dilution between the two types of sulfate cations (Ca and Mg) found in the bedrock can produce groups that seem to overlap. We have updated the figure to improve readability of the data. With only one horizontal axis now showing CaO+MgO content the new figure highlights that the groups of points selected on the basis of their lithology are compositionally distinct due to the addition of either Ca or Mg-sulfate. Although some points in bedrock show Ca-sulfate enrichments (now acknowledged in the text line 70-72) they are sporadic detections and most points are sulfate-poor. We also ran a Mann-Whitney U test to check the distribution and found an extremely low p-value indicating clear distinction of the two groups.

(3) I was also disappointed to see that the critical observation for why the desiccation features record multiple wet-dry cycles, and not just a single drying event – the presence of Y-junctions, not T-junctions – has no quantitative data associated with it. This reviewer agrees with the impression of the authors that this is the case based on Figures 1b and 1e in particular; however, I have no sense of how representative this is. This seems like a relatively easy, although perhaps tedious and time-consuming, fix, where the authors can spend some time mapping out the polygons and measuring junction angles, to ultimately present a distribution (similar to the size distribution shown in Extended Data Figure 4) that (presumably) shows a modal angle near 120°.

> We now have mapped the polygonal pattern junctions from the 3D model of the bedrock (ortho projected top view) to measure the statistical distribution of their angles and show the result in an updated and much larger version of Extended Data Fig. 4. Whether the well-expressed (solid lines) or more altered potential ridges (dashed line) are considered the angles distribution is well-fitted by a gaussian curve centered at 119.1°. The polygons were mapped within a short enough distance from the rover so that resolution allows for identification of ridges and their orthoprojection to estimate actual angle. Methods now describe the new measurements.

(4) As stated on line 55, there are already multiple publications (three cited) that interpret seasonal drying of a Gale crater lake. I understand that these new observations are from a different stratigraphic interval, but I was unclear from reading this paper how the current manuscript otherwise stands apart from these previous contributions? If we already knew there were wet-dry cycles at some point in the record of Gale stratigraphy, what is the new and exciting thing here that makes the paper broadly appealing for the Nature audience (aside from the link to prebiotic processes, which seems an argument that could otherwise be applied to the previous publications, no?)? I am not trying to be dismissive with these questions, but rather would encourage the authors to spend some time expanding on the novelty of this set of contributions, and how they are distinct from previous interpretations of seasonal drying recorded at Gale crater.

> We thank the referee for pointing out the contradiction in the initial statements. The text mentioned initially "secular" not only seasonal, and among the three references cited : Hurowitz et al. (2017) describe a gradual change of Fe oxidation within sediment mineralogy of decameters of strata interpreted as a secular change of depositional depth in a redox stratified lake. Rapin et al. (2019) describe the presence of intermittent sulfate enrichments within 150 m thick strata interpreted as deposits from episodes of lake higher salinity that involve climatic fluctuations, possibly seasonal but also secular. Schieber et al. (2022) is a more comprehensive geochemical and sedimentary analysis of a 15 m thick stratigraphic interval with an interpreted parasequence that involves seasonal dryings. While Rapin et al. (2019) and Schieber et al. (2022) models may involve seasonal dryings, the papers mention that "no features were observed that clearly indicate primary salts, their dissolution or the presence of disruptive fabrics common to terrestrial evaporitic settings" for Rapin et al. (2019) and "absence of primary desiccation cracks" for Schieber et al. (2022). Indeed the absence of such clear features to support evaporite deposits and/or repeated sub-aerial dryings has led to completely different explanations of the same strata, such as Grotzinger et al. (2015), Stack et al. (2019) interpretation of a deep lake sedimentary origin, or as mentioned in Rampe et al. (2017) : "The absence of physical sedimentary evidence for subaerial exposure in the lacustrine succession suggests a perennial lake environment (Grotzinger et al., 2015).".

With that regard, we present our findings as "the first tangible evidence for sustained wet-dry cycling in early Martian sub-aerial environments" (lines 129-130). We have removed the reference to prior modeled seasonal or secular drying at Gale as introducing the dispute over prior strata that lack clear evidence for desiccation would require a lot of context that is out-of-scope of the paper. Instead, we now refer more broadly in the introduction (lines 44-46) to the disagreements of global models of the hydro-climate that are poorly constrained due to the lack of evidence from prior in situ observations, which we believe would be of interest to the broader community. We have kept the comparison to the only prior published evidence for polygonal cracks mapped and interpreted as desiccation cracks. These formed T-junctions within a single horizon instead (Stein et al. 2018), highlighting that the new findings are distinct, with much broader implications.

Grotzinger, John P., et al. "Deposition, exhumation, and paleoclimate of an ancient lake deposit, Gale crater, Mars." *Science* 350.6257 (2015): aac7575.

Stack, Kathryn M., et al. "Evidence for plunging river plume deposits in the Pahrump Hills member of the Murray formation, Gale crater, Mars." *Sedimentology* 66.5 (2019): 1768-1802.

Rampe, E. B., et al. "Mineralogy of an ancient lacustrine mudstone succession from the Murray formation, Gale crater, Mars." *Earth and Planetary Science Letters* 471 (2017): 172-185.

(5) Two additional, more minor comments:
--On lines 103-104 it is suggested that desiccation cracks affect only the upper most few cm, yet on lines 65-66 it is suggested that the polygons extend to tens of cm depth based on their "stepped appearance". For the latter observations, is it possible that this stepped appearance doesn't represent differential erosion of one 10s of cm thick interval, but rather pre-desiccation topography with 10s of cm of relief? That might reconcile the two points mentioned above?
--On line 105, replace "in consistence" with "consistent".

> The depth of desiccation and the depth of propagation by soil aggradation represent two different depths that don't necessarily match. The text now more clearly defines the different interpreted depths (lines 100-109). On the possibility that the exhumed surface may represent a paleosurface, this seems unlikely. The stepped lithology shows that polygonal ridges are actually exhumed as a series of vertical walls when exposed on a sloped block. It is well-observed on this block (Fig. 1b) and can also be seen in the bedrock with more gentle slopes where ridges are also observed as walls forming a stepped appearance, whereas flat lying bedrock displays clearly a pattern of positive ridges. It indicates the vertical persistence of the pattern within the sediment in a specific vertical direction instead of a direction fluctuating with the slope of former topography.

Reviewer Reports on the First Revision:

Referees' comments:

Referee #1 (Remarks to the Author):

I like the new title for this paper.

The authors have largely addressed my comments in their revision of this paper; however, there are 2 places in the manuscript that I feel would still benefit from revision.

1)Smooth versus polygonal features

Lines 70-72 describe a comparison of sulfates at smooth host bedrock versus polygonal features and cite Figure 2 and Extended Data Table 2. I spent some time looking at these data but some information is still lacking. I'm guessing that the "Avg. bedrock" data listed in Extended Data Table 2 is the "smooth host bedrock" and maybe also the APXS Ribagnac sample. The caption for this refers to Fig. 2c, but the paper does not include a Fig. 2c. From the caption of Fig. 2 it appears that "host" bedrock is also "smooth" and "nodular" bedrock includes "polygonal" features. Would both the Hautefort and Montignac features listed in Extended Data Table 2 be considered "nodular"? And for Fig. 2 there are different symbols for "polygonal ridges" and "nodular bedrock".

Please review this text at lines 70-72, Fig. 2 and Extended Data Table 2. Additional description of the sample categories would help me better follow your arguments here.

2)Reasoning needed for statement about warm temperatures

Lines 139-140 mentions "our observations at Gale instead suggests relatively warm temperatures associated with sulfate formation". While I am in support of this idea, I think this statement needs some reasoning and references.

The Bishop et al. 2018 paper in Nature Astronomy presents arguments in favor of warm conditions for smectite formation. One way to argue for warm temperatures for the sulfates at Gale would be to use the points raised in that paper for smectite formation since Gale has abundance smectites.

Bishop J. L., Fairén A. G., Michalski J. R., Gago-Duport L., Baker L. L., Velbel M. A., Gross C. & Rampe E. B. (2018) Surface clay formation during short-term warmer and wetter conditions on a largely cold ancient Mars. Nature Astronomy, 2, 206-213.

Referee #2 (Remarks to the Author):

Review of: "Sustained wet-dry cycling on early Mars", by: W. Rapin et al., Submitted to Nature, Manuscript # 2022-11-17709A.

Review by: Tim Goudge, The University of Texas at Austin

Date: 3/21/2023

Paper Summary and Overview Comments:
In this manuscript, the authors present analyses of Mars Science Laboratory (MSL) observations of sulfate-bearing outcrops identified as a key paleoenvironmental transition from orbit. The authors identify polygonal ridges enriched in sulfate and with Y-junctions. The authors interpret these as indicative of repeated wet-dry cycles that occur in multiple locations within an 18 m section of strata. These observations are used to drive discussion on implications for the early martian hydro-climate, and the potential for prebiotic processes on early Mars.

This is a revised draft of a manuscript I previously reviewed, and I have looked over both the response to reviewer comments (my own and those of the other reviewer), and the revised manuscript. The authors have gone to significant lengths to address all of the reviewer comments, including conducting/presenting new results that bolster their arguments. I commend the author team on their efforts to provide such a thorough and detailed response. I am sure these revisions were no small feat, and I think the current submission is a much more succinct, defensible, and an even more compelling manuscript.

I have only a handful of minor comments (detailed below). Once these edits are implemented/addressed, I think this manuscript will be suitable for publication in Nature, and will draw significant community attention when it is ultimately published. I do not anticipate needing to see another draft of this manuscript, and leave it to the editor to assess adequate incorporation of my comments.

I again applaud the authors on a job well done, and please do not hesitate to reach out if there is any clarification needed on the comments below. I hope that this review continues to find all those involved with this manuscript safe and healthy.

Remaining Comments (Lxx refers to line xx in the submitted PDF):
1. L22: Replace “modifications” with “structures. Add “are” after “yet”. Replace “missing” with “incomplete”.

2. L25: Add “the” after “on. Replace “sub-aerial” with “surface” (technically some parts of the landscape might have been shallowly submerged at some points).

3. L31: Add “the” before “Gale”.

4. L32: Add “observed” before “polygonal”.

5. L35: Add “may have” before “sustained” (I think this is a touch speculative, so good to qualify). Delete “that have been”.

6. L39: Change “date” to “dates”.

7. L43: Optional, but could add “(e.g., mud cracks)” after “imprints” to telegraph the story a bit more.

8. L46: Could add the following paper to the “11-15” citation block: https://doi.org/10.1029/2021GL093523.

9. L71: Change “polygons” to “polygon”.

10. L72: Add “is” after “but”.

11. L111-117: I appreciate the authors’ changes here, and think how this is now presented is largely consistent with the observations. I remain somewhat skeptical of the East Section as true desiccation cracks; however, the way things are presented in this revised version does not rely on this observation alone, and indeed leaves room for this ambiguity. My one suggestion to really make this section align with all the presented observations in the best possible way is to add the following text after “indicated” on L117: “by this sequence, with perhaps punctuated or spatially variable episodes of persistent wet-dry cycling”.

12. L117: Optional, but I think it would be fair to replace “may have” with “likely”.

13. L121: Replace “flood deposits” with “input”.

14. L124-124: Replace “persistence with” with “existence across”, and add “at least periodically” after “maintained”.

15. L129: Optional, but could merge this paragraph and the preceding one, in which case you could also add “therefore” after “are”. I think this would help the flow.

16. L135-136: Delete “Besides” and add “also” before “adds”.

17. L137-140: Why are warmer temperatures indicated here? I presume because of the requirement for desiccation of liquid water at the surface, but the authors should just state this explicitly at the end of this paragraph.

18. L162: Replace “definite” with “also”.

19. L164: Replace “stratigraphy” with “stratigraphic section”.

20. L165: Reference 38 should not be to the whole “Sedimentary Geology on Mars” book, but rather the first chapter in that book – “The Sedimentary Rock Record of Mars: Distribution, Origins, and

Global Stratigraphy", https://doi.org/10.2110/pec.12.102.0001. Also, you could consider adding the following reference to this block: https://doi.org/10.1029/2009GL041870.

21. L167: Replace "sub-aerial" with "surface" (see Comment #2 above).

Author Rebuttals to First Revision:

See below point-by-point response to referees' comments.

Referees' comments:

Referee #1 (Remarks to the Author):

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The authors have largely addressed my comments in their revision of this paper; however, there are 2 places in the manuscript that I feel would still benefit from revision.

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Please review this text at lines 70-72, Fig. 2 and Extended Data Table 2. Additional description of the sample categories would help me better follow your arguments here.

> We thank the referee for pointing out the missing reference, and confusion due to Extended Data Table 2 miss-referencing Fig. 2 instead of Fig. 1 for context on targets. We have corrected the reference and now clearly indicate where context can be found in the figure. Hautefort Fig. 1d, Montignac Extended Data Fig. Ribagnac Fig. 9b, and APXS Ribagnac Fig. 1c. We more clearly indicate where table show average smooth host bedrock composition.

2) Reasoning needed for statement about warm temperatures

Lines 139-140 mentions "our observations at Gale instead suggests relatively warm temperatures associated with sulfate formation". While I am in support of this idea, I think this statement needs some reasoning and references.

The Bishop et al. 2018 paper in Nature Astronomy presents arguments in favor of warm conditions for smectite formation. One way to argue for warm temperatures for the sulfates at Gale would be to use the points raised in that paper for smectite formation since Gale has abundance smectites.

Bishop J. L., Fairén A. G., Michalski J. R., Gago-Duport L., Baker L. L., Velbel M. A., Gross C. & Rampe

E. B. (2018) Surface clay formation during short-term warmer and wetter conditions on a largely cold ancient Mars. Nature Astronomy, 2, 206-213.

> We agree with the author here and appreciate the suggestion but additional statements on smectite formation seem out-of-scope of the paper as our chemical data mostly support the presence of sulfates and does not discriminate the absence or presence of clays in these specific strata. We added a minor change to the statement in response to referee 2 however.

Referee #2 (Remarks to the Author):

Review of: “Sustained wet-dry cycling on early Mars”, by: W. Rapin et al., Submitted to Nature, Manuscript # 2022-11-17709A.

Review by: Tim Goudge, The University of Texas at Austin

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This is a revised draft of a manuscript I previously reviewed, and I have looked over both the response to reviewer comments (my own and those of the other reviewer), and the revised manuscript. The authors have gone to significant lengths to address all of the reviewer comments, including conducting/presenting new results that bolster their arguments. I commend the author team on their efforts to provide such a thorough and detailed response. I am sure these revisions were no small feat, and I think the current submission is a much more succinct, defensible, and an even more compelling manuscript.

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I again applaud the authors on a job well done, and please do not hesitate to reach out if there is any clarification needed on the comments below. I hope that this review continues to find all those involved with this manuscript safe and healthy.

> We are very glad the referee is satisfied with how his original concerns are tackled in this new version.

Remaining Comments (Lxx refers to line xx in the submitted PDF):
1. L22: Replace “modifications” with “structures. Add “are” after “yet”. Replace “missing” with “incomplete”.

> We incorporated those changes along with the editor suggestions in this part of the manuscript.

2. L25: Add “the” after “on. Replace “sub-aerial” with “surface” (technically some parts of the landscape might have been shallowly submerged at some points).

> This sentence of the summary of the abstract was replaced and no longer refer to sub-aerial modifications.

3. L31: Add “the” before “Gale”.

> We added this to the text.

4. L32: Add “observed” before “polygonal”.

> We added this to the text.

5. L35: Add “may have” before “sustained” (I think this is a touch speculative, so good to qualify). Delete “that have been”.

> We made this change. See also other changes proposed to the summary.

6. L39: Change “date” to “dates”.

> We added this to the text.

7. L43: Optional, but could add “(e.g., mud cracks)” after “imprints” to telegraph the story a bit more.

> This is a good idea, we added this to the text.

8. L46: Could add the following paper to the “11-15” citation block: https://doi.org/10.1029/2021GL093523.

> This is indeed a good reference on the limits of current climate models to understand surface hydrology. We added this reference.

9. L71: Change “polygons” to “polygon”.

> We made this change.

10. L72: Add “is” after “but”.

> We added this to the text.

11. L111-117: I appreciate the authors’ changes here, and think how this is now presented is largely consistent with the observations. I remain somewhat skeptical of the East Section as true desiccation cracks; however, the way things are presented in this revised version does not rely on this observation alone, and indeed leaves room for this ambiguity. My one suggestion to really make this section align with all the presented observations in the best possible way is to add the following text after “indicated” on L117: “by this sequence, with perhaps punctuated or spatially variable episodes of persistent wet-dry cycling”.

> We find this is a great addition and incorporated this change.

12. L117: Optional, but I think it would be fair to replace “may have” with “likely”.

> We added this change.

13. L121: Replace “flood deposits” with “input”.

> We added this change.

14. L124-124: Replace “persistence with” with “existence across”, and add “at least periodically” after “maintained”.

> We added this change.

15. L129: Optional, but could merge this paragraph and the preceding one, in which case you could also add “therefore” after “are”. I think this would help the flow.

> We merged the paragraphs and added this change.

16. L135-136: Delete “Besides” and add “also” before “adds”.

> We added this change.

17. L137-140: Why are warmer temperatures indicated here? I presume because of the requirement for desiccation of liquid water at the surface, but the authors should just state this explicitly at the end of this paragraph.

> We have explicitly mentioned that in this sentence to avoid confusion on the temperature here.

18. L162: Replace “definite” with “also”.

> We added this change.

19. L164: Replace “stratigraphy” with “stratigraphic section”.

> We added this change.

20. L165: Reference 38 should not be to the whole “Sedimentary Geology on Mars” book, but rather the first chapter in that book – “The Sedimentary Rock Record of Mars: Distribution, Origins, and Global Stratigraphy”, https://doi.org/10.2110/pec.12.102.0001. Also, you could consider adding the following reference to this block: https://doi.org/10.1029/2009GL041870.

> We added this change and added the reference here (already in our reference list).

21. L167: Replace “sub-aerial” with “surface” (see Comment #2 above).

> We added this change.