Final Response : A Global High-Resolution Hydrological Model to Simulate the Dynamics of Surface Liquid Reservoirs: Application on Mars

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Referee comments on "A Global High-Resolution Hydrological Model to Simulate the Dynamics of Surface Liquid Reservoirs: Application on Mars" by A. Gauvain et al., Geoscientific Model Development Discussions., https://egusphere.copernicus.org/preprints/2025/egusphere-2025-4992/#discussions.

Referee comments are shown in black. *Our responses are in blue italic.*

1 RC1: "Comment on egusphere-2025-4992", Kerry Callaghan, 08 Nov 2025

This paper presents a series of simulations of possible past water distribution on Mars, making use of a depression hierarchy and a hydrological model to distribute different possible amounts of water across the planet. It is great to see this work being done and I enjoyed reading the paper, especially the fantastic figures showing several theoretical water distributions for Mars. However, I do have several questions about the implementation of the hydrological model, pre-processing of the data, and conclusions around the final water distribution. Some of the questions that I had while reading the paper are listed as limitations of the model or discussed as directions for future work, but I felt as though more may be needed in the present paper. I hope that the authors will be able to address these questions and produce a stronger paper ready for publication.

Thank you for the positive feedback. We address the comments below in the revised version of the manuscript. We provide detailed responses to each of your comments below.

Major comments:

The hydrology model: The authors use the Depression Hierarchy (DH) by Barnes et al to analyse the topography prior to routing water using a hydrology model. I have two major questions about their hydrology model:

1. Especially considering that DH was already used, why not continue and use Fill-Spill-Merge (FSM) by the same authors, which was constructed to route water through the DH? The authors refer to FSM and the methods used therein. Possibly there is some specific function that was not covered by FSM, but if so, it is not clear to me what this is. Especially given that the hydrology model was not explicitly coupled with any climate/GCM model, but was run iteratively with a calculation of E and P, the same thing could have been done with FSM with remarkably less effort on the part of the authors. It seems likely that I am just missing the reason here, in which case, it needs to be more clearly stated and in general, the differences between this and other similar algorithms/the specific need for this new algorithm needs to be stated (e.g. Noel et al 2021, Gailleton et al 2024, Shook et al 2021).

Thank you for this comment. Indeed, the overflow algorithm implemented in our model is similar to the FSM approach. A detailed response on this point is provided in the following comment.

As mentioned in line 168 of the manuscript, the primary objective of this work is to develop a global hydrological model that is ultimately intended to be coupled with a Global Climate Model (GCM) within a Planetary Evolution Model (PEM). Implementing our own hydrological model in Fortran provides substantially greater flexibility for future developments. In particular, it enables a tight and efficient coupling with a GCM and facilitates the implementation of additional physical processes, such as the evaporation of surface water bodies, which is a key process in planetary-scale hydrology. We agree that our long-term objectives regarding the coupling between the hydrological model and a GCM were not sufficiently explicit in the original manuscript. These objectives will be clarified in the revised version.

We also acknowledge the need to better position our approach relative to existing and related algorithms. In the revised manuscript, we propose to add the following paragraph to the introduction:

*"This paper presents a new global high-resolution hydrological model designed to simulate the spatial distribution of surface water reservoirs and their connectivity for a given global topography map.*This paper presents a new global high-resolution

hydrological model based on the depression hierarchy and flow-routing concepts introduced by Barnes et al. (2021). The model is designed to simulate the spatial distribution, storage, and connectivity of surface water reservoirs at the planetary scale for a given global topography. This focus on global organization and long-term equilibrium behaviour distinguishes our approach from most existing terrestrial hydrological models, which are generally developed for local to regional applications. Sequential depression-filling methods (e.g. Noel et al. (2021)) focus on event-based hydrological connectivity within individual catchments. Wetland ponding models such as WDPM Shook et al. (2021) primarily target low-relief environments and local storage dynamics. Hydrodynamic models like GraphFlood Gailleton et al. (2024) resolve two-dimensional surface flow at high spatial and temporal resolution. In contrast, our framework explicitly conserves surface water mass at the scale of the entire planet and redistributes water volumes through a pre-computed hierarchical network of topographic depressions. The model efficiently captures the large-scale structure and long-term equilibrium states of lakes, seas, and potential oceans. This makes it particularly well suited for investigating planetary surface hydrology, such as that of early Mars, where global organization and long-term water redistribution are more relevant than short-term, process-based hydrodynamics."

1. Line 213: the overflow algorithm described cites Barnes et al 2021 (FSM), and indeed seems to be identical to the FSM algorithm.

Yes, it is indeed similar to FSM. We directly implemented in Fortran the algorithm described in Barnes et al. (2021), specifically Pseudo-code 6.2 (OverflowInto). Subsection 2.3.2 describes this algorithm as proposed by Barnes et al. (2021). This subsection was added to provide a complete description of the hydrological model. Although we refer to Barnes et al. (2021) (line 213), we have clarified that part of the FSM algorithm is implemented within our hydrological model.

2. Line 235: the bypass mechanism described herein already exists in FSM as well.

We were not aware of this bypass mechanism in FSM, and we thank the reviewer for pointing this out. We have refered to Barnes et al. (2021) in the revised version of the manuscript.

2. I am concerned about the pre-computed hydrological lake functions. I understand that the intention here was to increase computational efficiency, but I have not been convinced that it is worth it. Either a discussion of how much faster this is than FSM (or than the current model with a full calculation of lake levels), or actually calculating the real lake levels, would help here.

After attempting a comparison, we found it difficult to provide a precise one between our algorithm and the FSM algorithm, mainly because FSM includes the construction of a depression hierarchy (DH). However, we incorporated Pseudo-code 6.3 from Barnes et al. (2021) (FillDepressions) into our algorithm to compute lake areas and levels. This comparison between the Interpolation of Pre-computed lake Functions (IPF) and the Fill–Spill–Merge (FSM) algorithm illustrates the efficiency benefits of our approach. We propose to add the following figure to the appendix of the revised manuscript.

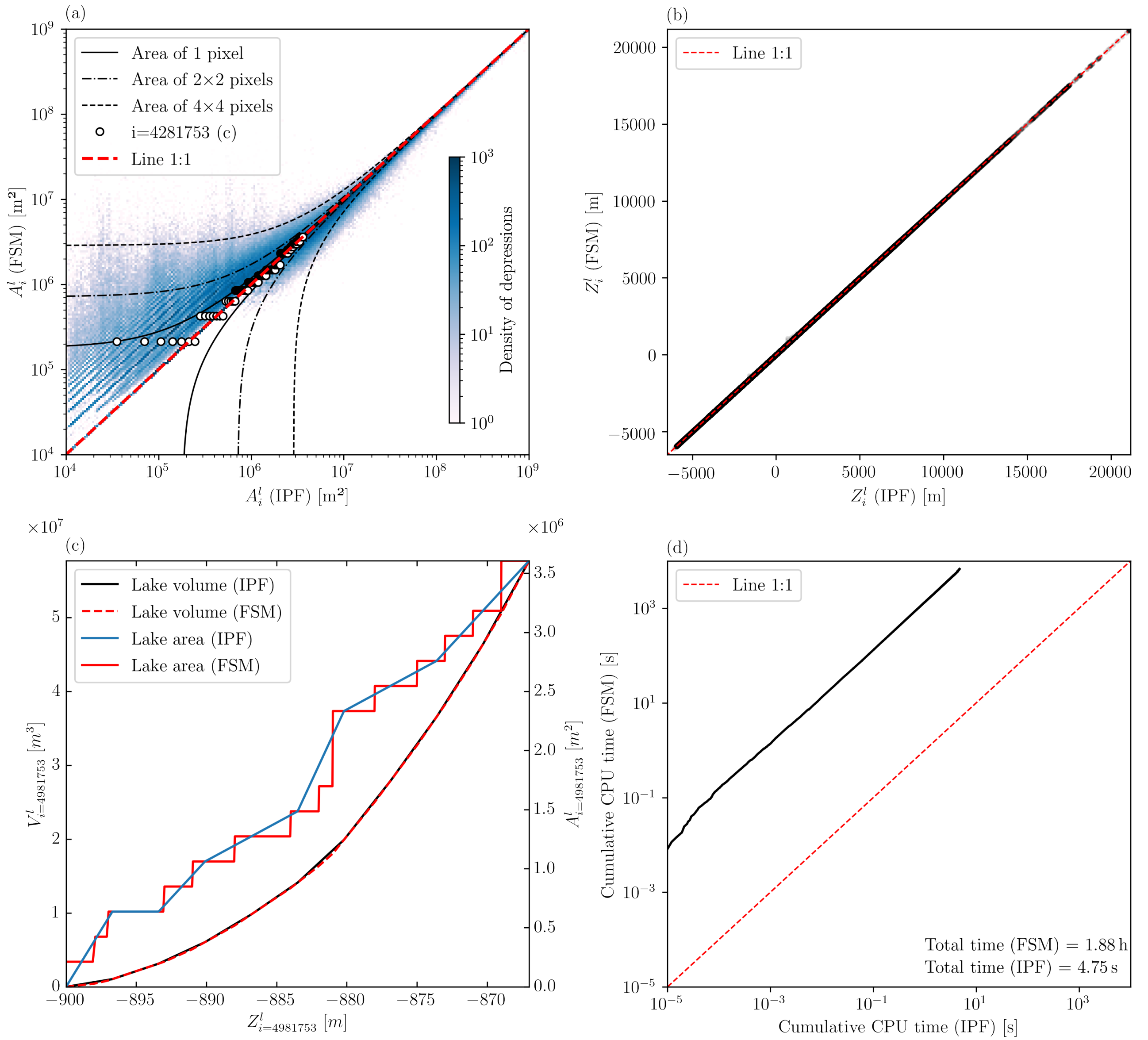

Figure 1. *(a) Comparison of the lake area A_i^l computed using the interpolation of pre-computed lake functions (IPF) and the Fill–Spill–Merge (FSM) algorithm. The comparison is performed for all active depressions (2,059,551 depressions) shown in Figure 5 (100 m GEL simulation). The color bar indicates the density of depressions. The three black lines (solid, dashed, and dash-dotted) represent errors of 1 pixel (423×423 m), 4 pixels, and 16 pixels, respectively. The white dots indicate the comparison between the blue and red solid curves in panel (c) for depression $i = 4{,}281{,}753$. (b) Comparison of the lake elevation Z_i^l computed using the interpolation of pre-computed lake functions (IPF) and the Fill–Spill–Merge (FSM) algorithm. (c) Pre-computed hydrological functions (IPF) and functions computed with the Fill–Spill–Merge (FSM) algorithm for a single depression ($i = 4{,}281{,}753$) shown in Figure 3. (d) Comparison of computation times for the two algorithms. The total CPU time for the 2,059,551 depressions is 1.88 hours for the Fill–Spill–Merge algorithm, compared with 4.75 seconds for the interpolation of pre-computed functions (IPF).*

For this comparison, we recomputed the lake area and elevation at steady state for the 100 m GEL presented in Figure 6 of the manuscript. In this final state, there are 2,059,551 active depressions. We calculated the lake areas and elevations of these depressions using both the IPF and FSM algorithms. The results show very good agreement between the two methods (Figure 1), with a mean error of 1.44 pixels (using the largest cell size of the DEM, $423 \times 423, \mathrm{m}$ *at the equator) for the lake area. This discrepancy arises from the difference between the step function computed with FSM and the pre-computed function, which is based on a linear interpolation of the lake area (Figure 1c). Comparison of the lake elevations also shows good agreement (Figure 1b), as the functions are very similar (Figure 1b). The total CPU time required to process the 2,059,551 depressions is 1.88 hours for the FSM algorithm (recall that the DEM contains 1,061,683,200 pixels), compared with only 4.75 seconds for the IPF approach. This comparison clearly demonstrates that our method is significantly more efficient than directly computing lake levels with FSM, while still providing accurate estimates of lake areas and elevations. Additionally, we want to mention that the computation of lake area and elevation is performed at every time step of the simulation, which further emphasizes the importance of computational efficiency in our approach.*

The topography: The authors mention in the text that it will be crucial for future work to exclude craters that formed after the Noachian period, 3.7 billion years ago. I think that this issue is being rather undersold. The post-Noachian period includes most of Mars's history. Many, many craters could post-date this period. The inclusion of these craters in the current work is likely to create a significant bias in the results, particularly in the case of the low-GEL simulations, in which a significant portion of the water is likely stored in younger craters. I do not have enough knowledge of Martian geomorphology to speak to whether the larger craters also post-date this time period, but it is a concern. How meaningful are the results if the topography used is significantly different from the time period intended to be simulated?

Thank you for this comment. As you noted in your online comment, much remains to be done to better understand the past hydrology of Mars. We agree that excluding craters formed after the Noachian period would be an important step to improve the realism of our simulations. However, this is a complex task that requires a detailed analysis of the Martian crater population and the development of methods to identify and exclude post-Noachian craters from the topographic data. We are currently working on this aspect for future studies, but it is beyond the scope of the present work, which focuses on presenting a global hydrological model that can be applied to any global topography. Moreover, this point is already discussed in the last paragraph of Section 4.1.

Validation: I recognise that a comprehensive validation or evaluation of the actual distribution of water on Mars may be beyond the scope of this work, however, I felt that at minimum some comparison between the results and known locations of water on Mars was starkly missing. Figure 2 shows known data around lakes and deltas, and the paper computes stored water and overflow volumes. This seems like a perfect opportunity to, at minimum, compare the results between the different GEL values and the known data and at least determine which simulation comes the closest to matching these. At a glance, the 100 m GEL simulation seems to come closest to the Deuteronilus and Arabia shorelines, and a deeper look into this would be helpful.

Thank you for this comment. We agree that a comparison between our results and the known locations of water on Mars would be valuable. However, we chose not to pursue this in the present study, in line with the comments of Reviewer 2, who noted that the assumption of spatially uniform precipitation is a strong constraint and may not be realistic compared to the

GCM simulations of early Mars available in the literature. We plan to explore more realistic precipitation patterns in future work, which will allow for more meaningful comparisons with the geomorphological observations on Mars. We adde the following paragraph to the section 4.4 :

"A quantitative comparison between simulated water distributions and observed geomorphological features on Mars (Figure 2) would strengthen the validation of our model. However, such a comparison is constrained by two key limitations in the current work. First, the assumption of spatially uniform precipitation is a strong constraint that may not reflect the realistic precipitation patterns predicted by Global Climate Models for early Mars (Turbet and Forget, 2021; Wordsworth et al., 2015, 2013). Second, the use of present-day MOLA topography, rather than a reconstructed ancient surface, introduces systematic biases in the spatial distribution of depressions and their connectivity. Despite these limitations, our results provide useful insights: the 100 m GEL simulation broadly reproduces the northern ocean extent between the Arabia and Deuteronilus shoreline levels, and the model successfully identifies the major topographic basins (Hellas, Argyre, northern lowlands) as primary water repositories."

Line comments:

Line 65: I recommend citing DH at its first mention here. Similarly, this paragraph mentions the Mars topography, which should be cited here.

Thank you for the suggestion. We have included the citations in the revised manuscript.

Line 114: the ocean elevation – the ocean elevation setting in DH does not fix the elevation of the oceans. Rather, it defines which cells should be considered as 'ocean' in order to start the process of locating pit cells to build the DH. The setting has no bearing on whether or not water redistributed through depressions can create a planetary scale ocean. I think, in your case, that choosing the highest cell on the topography should be okay in the workflow, but what is happening algorithmically then is that DH construction will initialise from the location of that highest cell and no more.

Thank you for this comment. We have clarified this point in the revised manuscript. As you noted, the ocean elevation setting in DH is used to identify pit cells and does not fix the actual elevation of the oceans. We made this distinction clearer in the revised version.

Line 119: I believe 'identify the depressions' should be 'identify the pit cells'.

Thank you for pointing this out. We made the correction in the revised manuscript.

Lines 180-185: My understanding is that a GCM was not actually used in this work, but some of the writing reads as though it was. Please be clearer that the use of a GCM is only a possibility for future work.

Thank you for this comment. We have removed any ambiguity regarding the use of a GCM in the revised manuscript. We have clarified that coupling with a GCM is intended for future work and was not implemented in the current study.

Equation 5: It is unclear to me why the total evaporated volume is being divided only by the areas of currently active watersheds. Shouldn't it be divided by the area of the planet, since precipitation is spread over the entire planet? In equation 6, similarly, the area of active depressions is used, and the text specifies active depressions. I know that you must be adding P into inactive depressions given that water distributions from all three starting points given in figure 4 converge. Or perhaps I am misunderstanding what an active watershed is (I thought that it was one that contained water in the last iteration).

Thank you for this comment. An active depression is defined as the highest depression in the graph that contains water. This means that all depressions below an active depression also contain water, but are not considered active. Since precipitation is distributed over the entire planet, the watershed area of the active depressions is equal to the total planetary surface. However, you are correct that Equation 5 is only valid under the assumption of spatially uniform precipitation. We have corrected this equation by dividing the total evaporated volume by the total surface area of the planet in the revised manuscript.

Line 372: the unique steady-state reached for each GEL: This makes sense and I also think that it's simply an observation that when you use constant P-E rates over the whole planet, then water will distribute itself into the watersheds that have the largest contributing area. In other words, the multiple locations for setup of the starting water don't change the basic fact that, given enough time and, importantly, the globally constant P-E, depressions with the largest contributing area (and possibly smallest area-to-volume ratio) will pool all of the water. Given that DH provides both depression volumes and watershed volumes, I wonder whether you may have even been able to bypass the simulation entirely with some clever math (not sure if this would have been easier than just doing the simulation or not). (Note: I don't think you should remove the different starting distributions from the paper or anything, but possibly you could note that this result makes theoretical sense). What I find more interesting is how different the distributions are with different amounts of total water (fig. 5), indicating some interesting relationships between depression level (leaf or higher) and contributing area. This seems like a great opportunity for some kind of validation, since the water distributions are so distinct with different GELs.

Thank you for this interesting comment. We will further explore this idea.

That said, given the effort to conserve mass in the computation of P based on E, here is a place where I am really concerned about the choice to not calculate lake volumes directly - mass will not be conserved with the linear estimate currently done.

The precipitation rate depends on the evaporated volume, which is computed based on the lake area and the evaporation rate (E). The lake area is computed using the interpolation of pre-computed lake functions, which provides an estimate of the lake area based on the volume in the depression. This approach allows us to maintain mass conservation in our model, as the evaporated volume is based on the evaporation rate (E) and the estimated lake area (A_i^l) and then this evaporated volume is reinjected as precipitation (P) over the whole planet. The mass is checked at each time step to ensure the mass is conserved during the simulation.

Figures:

The figures are overall great, clear and informative. I just have a few comments to increase their clarity.

Thank you for the positive feedback. We have addressed the comments below in the revised version of the manuscript.

Figure 1: The dotted lines present on panel (b) are unclear. There are 4 dotted lines but only 3 numbers. I can't tell which line is supposed to correspond to which number; I know that the lowest line is the one that should not correspond to any number, but right now it looks as though it is intended to correspond to number 5.

We have clarified the dotted lines in the revised version of the figure. These lines represent the minimum and maximum elevation limits of the depressions. We have clarified this information in the figure caption in the revised manuscript.

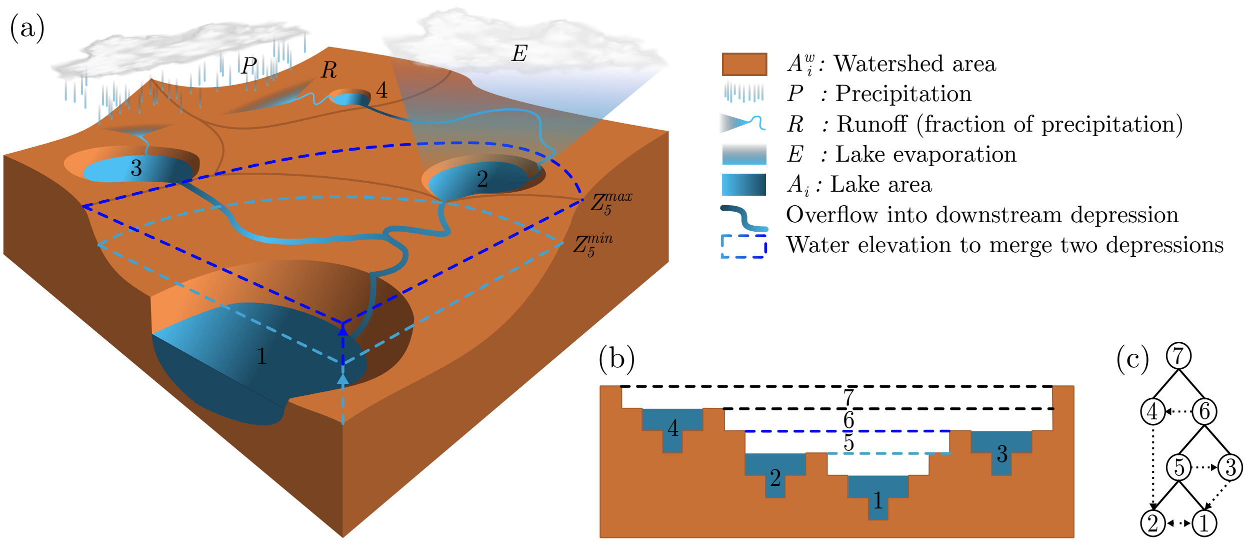

Figure 2. Representation of the water cycle on a conceptual topography with 4 leaf depressions. (a) 3D block represents the water flow from upstream to downstream. The links between the depressions, which represent streams, are symbolized by full blue curves. The sky blue dotted line represents the lake elevation where a depression No.1 can merge with a depression No.2 and create a new depression No.5. The dark blue dotted line represents the lake elevation where the depression No.5 merge with the depression No.3. The merge lines (blue and black dotted lines) are projected on the cross-section (b) of the 3D block diagram.*(b) The dotted lines represent the minimum and maximum elevation limits of the depressions.* (c) Binary tree of the depression hierarchy. Nodes No.1-4 are leaf depressions and nodes No.5-7 are meta-depressions. The black lines represent the merge into a new depression. The dotted arrows show the link with downstream depression. Adapted from Barnes et al. (2020).

Figure 2: Panels b, c, and d each contain a white star that I don't know the meaning of.

The white stars indicate the outlets of Nirgal Vallis, Jezero Crater, and Gale Crater. We have included this information in the figure caption in the revised manuscript.

Figure 3: I assume that these are watersheds relating to the lowest, leaf-level depressions? Please clarify.

Yes, these correspond to the watersheds associated with the lowest, leaf-level depressions. We have clarified this in the revised manuscript.

Figure 8 and related text: The text notes a lack of pooled water between -30 and 30 degrees longitude in the pre-TPW topography, and calls out its coarser resolution as the cause of this. However, the degraded MOLA topography still has water pooling in this region. This is not the only mismatch and generally, the full-resolution and degraded versions of MOLA match very closely (with one major exception noted in the text) while the pre-TPW topography looks significantly different. To me, this implies that spatial resolution is not the main causative factor, but rather that actual material differences in the topography are behind this. Comparing this figure with the distribution shown in Fig. 6, the main difference I can see visually is in the northern latitudes, where the MOLA topography has a vast ocean while the pre-TPW has virtually no water pooling. This also seems to be the cause of much of the difference in that -30 to 30 longitude zone. I can't see how such an expansive depression could be lost as a result of coarser topography – and indeed we know that the degraded MOLA still had water distributed in this region – so it would be great to see a discussion of that.

Thank you for this comment. We agree that the differences in water distribution between the pre-TPW topography and the MOLA topography are primarily due to the absence of the large northern ocean in the pre-TPW topography in this region. We have revised the text to clarify that the main cause of the differences in water distribution is the absence of the large northern ocean in the pre-TPW topography—rather than the spatial resolution.

Figure B2, B4, and B6: These figures refer to a second line of zoomed plots which does not exist.

We have removed the captions referring to a second line of zoomed plots in the revised manuscript.

2 RC2: "Comment on egusphere-2025-4992", Anonymous Referee, 04 Jan 2026

This manuscript presents the first global, kilometer-scale, quantitative assessment of surface water distribution on early Mars. This assessment was made using a newly developed hydrological equilibrium model, which was constrained by high-resolution DEM data. This approach is both original and technically impressive, as well as being scientifically important. Notably, the systematic exploration of the total water inventory and the subsequent shift from a nearly uniform distribution of water to a dominant northern ocean is a pivotal finding that will resonate widely within the communities of Mars climate, hydrology, and geomorphology.

The model framework is well designed, computationally efficient and clearly described. I particularly value the authors' intention to couple this hydrological model with a GCM in future work, as this makes the present study a valuable foundation for the next generation of coupled climate–hydrology simulations.

Overall, I consider this manuscript to be strong and publishable following revision. The comments below aim to clarify the physical interpretation of the results and increase the robustness and impact of the conclusions, rather than questioning the validity of the modelling framework itself.

Thank you for the positive feedback. We have addressed the comments below in the revised version of the manuscript.

Major comments

1. The assumption of spatially uniform precipitation and the interpretation of equilibrium states.

In the current version, it is assumed that precipitation and evaporation are spatially uniform, and the system is iterated until a steady state is reached. Under these conditions, the model robustly converges to a unique equilibrium water distribution for a given GEL, regardless of the initial conditions. This is an important and interesting result.

However, assuming homogeneous precipitation is a very strong symmetry constraint and likely plays a central role in the uniqueness and stability of the equilibrium solutions. Numerous GCM studies of early Mars have shown that rainfall (or snowfall) patterns depend strongly on obliquity, topography and atmospheric composition, and are generally far from uniform.

I therefore suggest clarifying the robustness of the conclusions with respect to this assumption and partially testing it if possible.

For example:

1. Could the authors include one or two idealized, non-uniform precipitation patterns (e.g. latitudinally varying precipitation or enhanced precipitation in mid-to-high latitudes) to test whether the qualitative, GEL-dependent transitions remain valid?

2. Alternatively, prescribing a fixed precipitation pattern derived from existing early Mars GCM studies (even in a simplified, time-averaged form) would be informative. Such sensitivity experiments would not require full GCM coupling, but would greatly strengthen the interpretation of the results by clarifying whether the derived equilibrium represent:

- purely topography-controlled end-member states, or
- dynamically robust equilibrium under more realistic climatic forcings.

Thank you for this comment. It is clear that the unique equilibrium states we obtained are a consequence of the homogeneous P–E assumption. Replacing the homogeneous P–E assumption with a non-uniform precipitation–evaporation pattern would *likely lead to multiple equilibrium states. The heterogeneity of the precipitation pattern would create dry regions, reduce the evaporated volume, and ultimately decrease the P/E ratio, since the precipitation rate depends on the evaporated volume. We agree that the conclusion that equilibrium states are unique for a given GEL is only valid under the assumption of the pattern of precipitation and evaporation.*

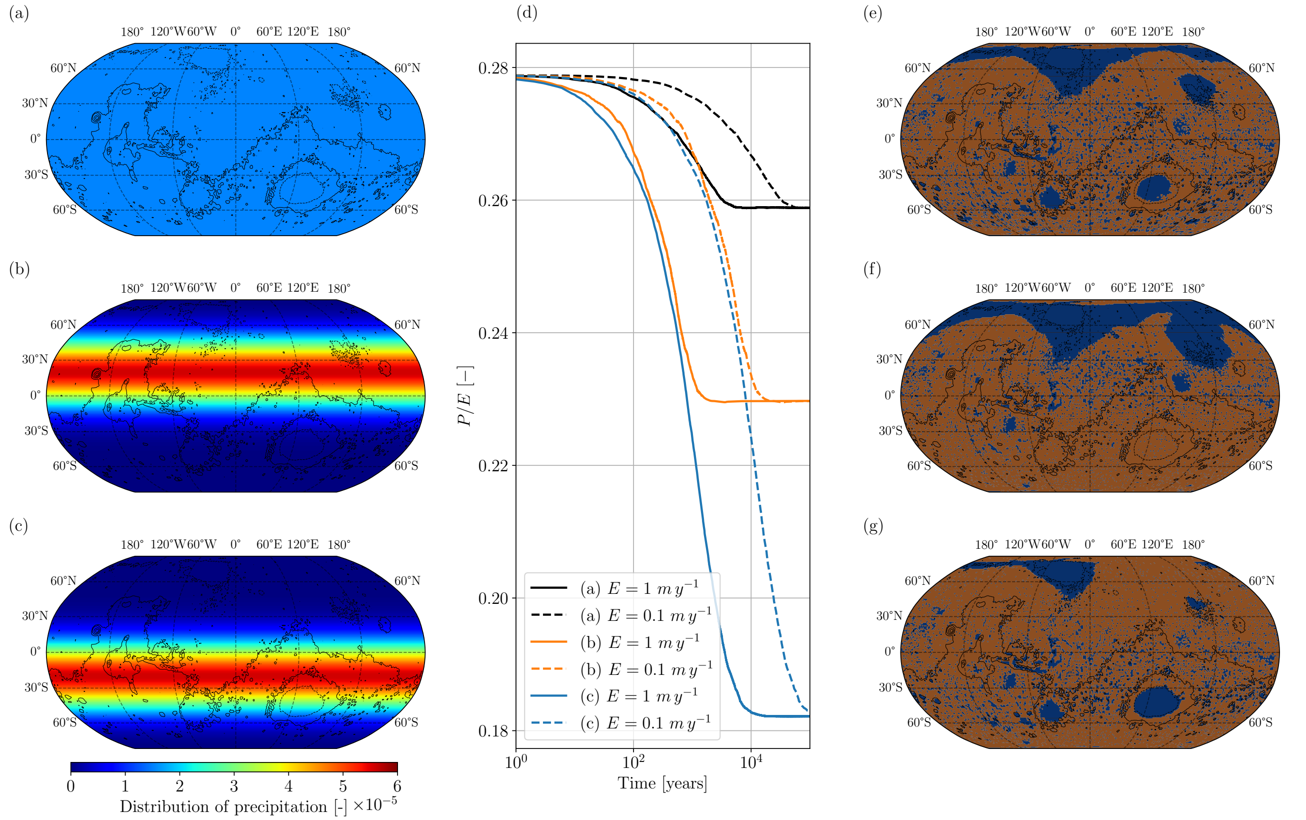

Figure 3. *Simulations at steady state for a GEL = 100m with a varying precipitation pattern: (a) homogeneous precipitation pattern, (b) precipitation is concentrated around 20°N and (c) precipitation concentrated around 20°S. Simulations were performed with a degraded MOLA topography at 1 pixel per degree resolution. (d) Evolution of the ratio P/E for the three precipitation rate patterns and two homogenous evaporation rate: 1 m/yr (solid curves) and 0.1 m/yr (dashed curves). Distribution of water reservoirs at steady state for the three precipitation patternes: (e) homogeneous precipitation pattern, (f) precipitation is concentrated around 20°N and (g) precipitation concentrated around 20°S.*

Sensitivity experiments with non-uniform precipitation patterns (Figure 3) were conducted to highlight the influence of the precipitation regime pattern. Three idealized precipitation patterns were tested: a homogeneous pattern (Figure 3a), a pattern concentrated around 20°N (Figure 3b), and a pattern concentrated around 20°S (Figure 3a). The results show that the equilibrium states are not unique for a given GEL when the precipitation pattern is non-uniform (Figure 3d). The final P/E ratio also varies significantly with the precipitation pattern. The water distribution at steady state is strongly influenced by the *precipitation pattern (Figure 3e-f-g), with water accumulating in regions of enhanced precipitation and drying out in regions of reduced precipitation.*

We have included these results in the revised manuscript to clarify the robustness of our conclusions with respect to the assumption of homogeneous P–E. We have added the following figure to the appendix of the revised manuscript to illustrate these results.

2. The climatic context of the inferred equilibrium.

I suggest explicitly stating in the manuscript that the present simulations should be interpreted as an idealized reference case, in which precipitation and evaporation are spatially uniform.

In this framework, the derived steady states represent water distributions that are topographically reachable under globally distributed water input, rather than predictions for a specific early-Mars climate scenario.

It would be very helpful to briefly discuss, at a conceptual level, how the results might differ under other plausible climatic regimes for early Mars, for example:

1. high-obliquity climates with enhanced low to mid- to latitude precipitation or snowfall,
2. climates dominated by ice-edge melting or spatially localized water input, or
3. episodic or hemispherically asymmetric precipitation patterns suggested by previous GCM studies.

Such a clarification would help readers avoid over-interpreting the equilibrium distributions as direct reconstructions of early-Mars climate and would naturally motivate future coupling of this hydrological model with a GCM.

Thank you for this comment. We agree that it is important to clarify that the simulations represent an idealized reference case with spatially uniform precipitation and evaporation. However, as shown in the previous figure, water can be distributed very differently compared to the precipitation pattern. For example, in the case of a precipitation pattern concentrated around 20°S (Figure 3c), water pools in the northern hemisphere (Figure 3g). These results demonstrate that it is difficult to discuss or interpret, even conceptually, how simulations might differ under other plausible climatic regimes for early Mars without performing new simulations. This work focuses on the presentation and development of a global hydrological model rather than a detailed exploration of early Mars climate scenarios. We plan to investigate these aspects in future work. In the revised manuscript, we have clarified the climatic context of our simulations and explicitly state that they represent an idealized reference case with spatially uniform precipitation and evaporation.

We rename the section 3.2 : "Climate assumption and parameters exploration" and we added the following paragraph to the section 3.2 of the revised manuscript:

"The simulations presented here should be interpreted as an idealized reference case, in which precipitation and evaporation are prescribed as spatially uniform over the planetary surface. In this framework, the resulting steady states do not aim to reproduce a specific early-Mars climate scenario, but rather describe water distributions that are topographically reachable under globally distributed water input. The exploration of the parameter space allows to evaluate the sensitivity of the model to different climate conditions and initial states, and to identify the range of conditions that can lead to the formation of lakes and river networks. The homogeneous precipitation/evaporation pattern allows the identification, at the global scale, of water flow pathways and the locations of possible lakes."

Minor comments

1. Notation of depression IDs

Both italic and non-italic forms of the "ID" appear throughout the manuscript. Please clarify whether this distinction has a specific meaning or is a typographical inconsistency.

Thank you for pointing this out. This is a typographical inconsistency, and we have corrected it in the revised manuscript.

2. Line 218: expression for excess volume

The expression (Vexcess=Vexcess-Vavail) reflects a programming operation rather than a mathematical definition. For clarity, I suggest rephrasing this line in a mathematically explicit form.

Thank you for this comment. We are agreed that this equation reflects a programming operation. We rephrased this paragraph as follows in the revised manuscript:

"If the water volume in the considered depression V_i is less than its maximum capacity V_i^{max}, the remaining available volume $V_i^{avail} = V_i^{max} - V_i$ is used to store a part or the entire excess volume. In case where V^{excess} is smaller than V_i^{avail}, the reservoir stores all the excess water, and V^{excess} becomes equal to 0. Conversely, when V^{excess} is larger than V_i^{avail}, the outgoing water volumme of the depression is calculated as: $V_i^{out} = V^{excess} - V_i^{avail}$ and the depression fills up ($V_i^{avail} = 0$ and $V_i = V_i^{max}$). This outgoing water volume will be redirected to another storage location according to the steps (2) and (3)."

3. Line 228: order of water transfer in the hierarchy

The algorithm prioritizes transferring excess water to sibling or downstream depressions before considering higher-level (parent) depressions. Could the authors please explain why this order is preferred, either physically or algorithmically, rather than transferring excess volume first to deeper hierarchical levels?

Thank you for this comment. The order of water transfer in the hierarchy is designed to mimic the natural flow of water in a hydrological system. Water must first fill the nearest available depressions (siblings or downstream) before overflowing to higher-level depressions (parents). Transferring the excess volume directly to a parent depression without ensuring that all sibling and downstream depressions are filled could lead to unrealistic water distributions, with empty depressions located below the parent depression.

4. Relation between crater age and storage capacity (Line 380)

The discussion correctly notes that younger terrains tend to have fewer craters and thus lower storage capacity, whereas older terrains exhibit greater water retention. An additional latitude–longitude cross-section or map explicitly demonstrating this relationship (e.g. crater density versus storage capacity) would clarify and enhance the visual appeal of this argument.

Thank you for this suggestion. We propose to complement Figure 5 with a cumulative distribution of crater density (red curves on panels (a) and (b) in the figure below). This will help clarify the relationship between crater density and storage capacity. We have included this in the revised manuscript.

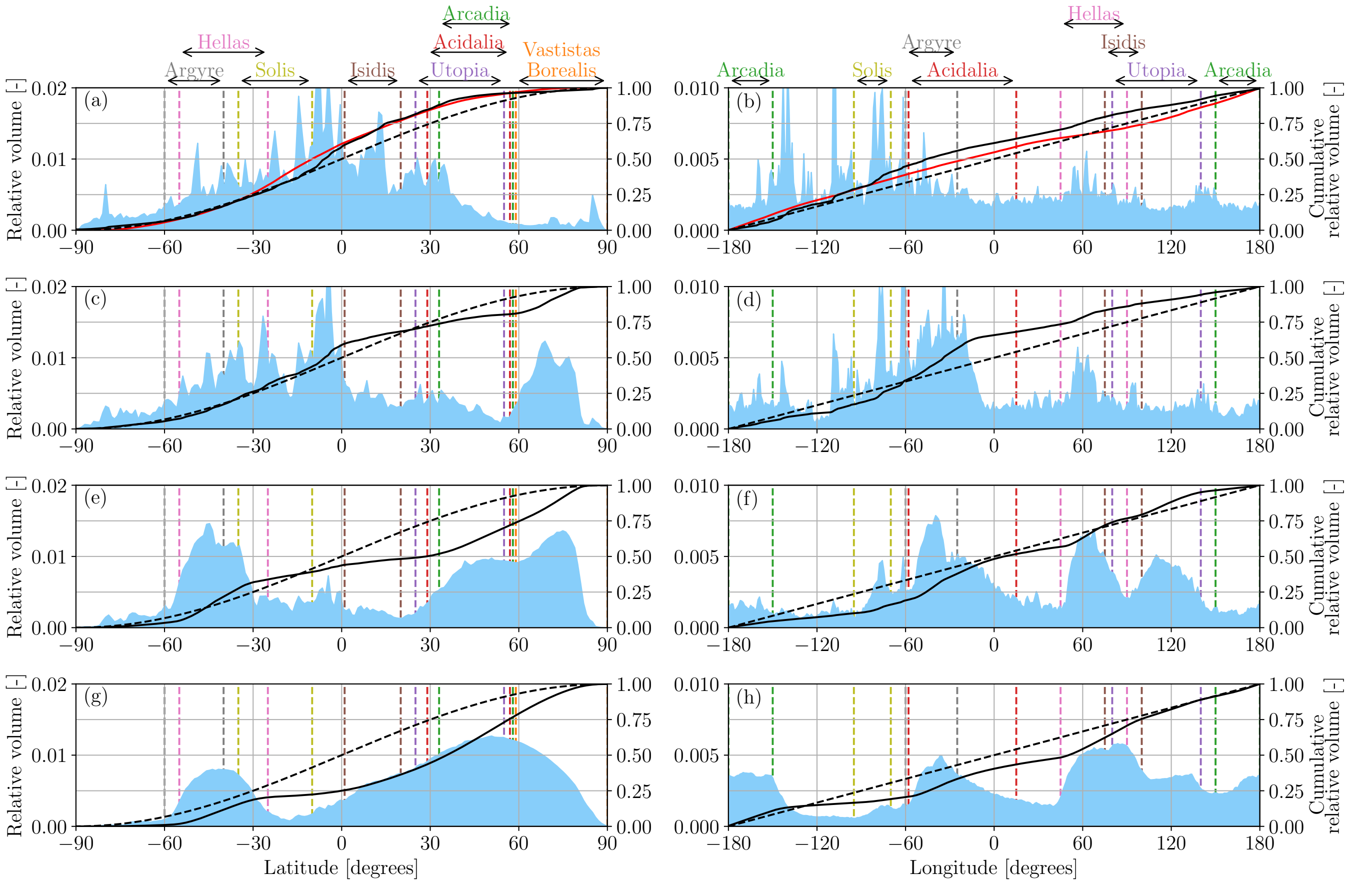

Figure 4. Distribution of water reservoirs at steady state for each GEL: 1 m (a, b), 10 m (c, d), 100 m (e, f), 1000 m (g, h). The blue areas represent the proportion of water relative to the total water volume (left y-axis) per degree of latitude (left column) and longitude (right column). The solid black lines show the cumulative relative water volume (right y-axis). The dashed black lines represent the cumulative relative water volume for a conceptual homogeneous distribution of water. *The red curves on panels (a) and (b) represent the cumulative distribution of crater density (right y-axis) from Robbins and Hynek (2012).*

Please note that the x-axis in panels (a), (c), (e), and (g) has been reversed compared to the first version of Figure 5.

5. Timescale to reach equilibrium

Although the paper focuses on steady states, briefly discussing the timescales required to reach equilibrium under typical evaporation rates would help to connect the results to geological constraints on lake lifetimes and valley network formation.

We have added a paragraph on the timescales required to reach equilibrium in Section 3.4 (Simulations and Validation) of the revised manuscript:

The timescale to reach equilibrium in our model depends primarily on the evaporation rate (E) and, to a lesser extent, on the Global Equivalent Layer (GEL) of water. The timescale varies linearly with the evaporation rate, as this parameter controls the rate of water transfer between depressions. Regarding the GEL, the timescale generally increases with GEL because a larger

GEL implies that more water must be redistributed before equilibrium is reached. However, a high GEL also corresponds to a larger cumulative lake area, which increases the evaporated volume and thereby accelerates the redistribution of water. This explains why GEL has a secondary effect on the timescale to reach equilibrium. In our simulations, the timescale ranges from approximately 200 years for a GEL of 1 m and an evaporation rate of 1 m/yr, to over 100,000 years for a GEL of 1000 m and an evaporation rate of 0.01 m/yr. These timescales are consistent with geological constraints on valley network formation on early Mars, which suggest that they could have persisted for 10^5 *to* 10^7 *years (Hoke et al., 2011).*

6. Role of subsurface flow

The absence of groundwater and infiltration is clearly acknowledged. It would be helpful to briefly discuss the expected direction of the bias introduced by this assumption, for example over- or underestimation of surface water retention in specific regions.

Thank you for this suggestion. We have included a discussion on the bias introduced by the absence of groundwater and infiltration in the revised manuscript. The following paragraph will be added to Section 4.2 (Groundwater Processes):

The process of infiltration and groundwater flow can significantly influence the distribution and retention of surface water on the planet. By adding these processes to our hydrological model, we would expect a redistribution of surface water driven by subsurface flow, with contrasting effects depending on topographic setting. In particular, surface water retention would likely be overestimated in high-elevation depressions, where infiltration would induce lake volume losses through recharge of the subsurface. Conversely, surface water retention would likely be underestimated in low-elevation depressions, which would act as groundwater convergence zones, receiving additional water through subsurface flow and thus sustaining larger or more persistent surface water bodies.

This study marks a significant advance in our ability to quantify potential surface water distributions on early Mars with high spatial resolution. The modelling framework is robust and original, and the findings are highly valuable.

I recommend revision prior to acceptance, mainly to:

- better contextualize the results climatically.
- assess (or more clearly define) the role of the homogeneous precipitation assumption.

Addressing these points will significantly increase the clarity, robustness and long-term impact of the manuscript.

Thank you for the positive feedback and the constructive comments. We have addressed the points raised in the revised manuscript based on our responses above.

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