egusphere-2026-928 - A non-explicit representation of macropores in the SVS land surface model improves streamflow simulations under frozen soil conditions

Responses to the Anonymous Referee #1

First of all, we wish to thank Referee #1 for providing constructive and insightful comments and suggestions. In addressing these suggestions, along with those from the other two reviewers, we have significantly revised the manuscript. We believe these changes have greatly improved the clarity and focus of our work. In particular, we have reframed the narrative to center on the enhancement of frozen soil infiltration within an operational model, with corresponding revisions to the introduction and discussion. Moreover, we have strengthened the Results section by incorporating land cover- and soil texture-based analyses regarding the calibration and performance of the new frozen soil infiltration configuration. Note that "Macropore configuration", "Fr-MP" and "a_{MP}" are denoted "Enhance frozen soil infiltration configuration", "Fr-Inf" and "$a_{no\text{-}imp}$" in this document and in the proposed revised version of the manuscript. Our answers below are in blue, whereas excerpts from the manuscript are in *blue italics* with modifications in ***bold***. In our responses, when referring to other comments from any referee, we use the following terminology: RefX-[Maj or Min]Y where X and Y indicate the referee and comment (major or minor) ID.

Referee comments 1 – egusphere-2026-928

The manuscript addresses a significant and timely topic in cold-region hydrology. The methodology is sound, and the results provide a clear path toward improving operational streamflow forecasting. However, the manuscript requires a more critical discussion regarding the physical basis of the macropore parameterization and a deeper analysis of the spatial variability in model performance. Addressing the major comments regarding the physical justification and the sensitivity of the alpha_MP parameter will significantly enhance the impact and scientific rigor of the work. I look forward to seeing a revised version of this study.

Major Comments :

Ref1-Maj1. The authors propose a non-explicit, threshold-based representation of macropores. While this is computationally efficient for operational forecasting, the manuscript lacks a robust discussion on the physical limitations of this simplification. Specifically, how does this approach account for the spatial heterogeneity of macropore networks at the grid-cell scale? A more thorough comparison with existing dual-domain or dual-permeability models (e.g., those cited in the introduction) is needed to justify why this simplified approach is sufficient for large-scale hydrological modeling.

Concerns regarding the physical interpretation of the "Macropores configurations" (renamed "Enhanced frozen soil infiltration" in the revised manuscript) were raised by all three referees (see Ref2-Maj2, Ref2-Min4 and Ref3-Maj1). In response, we have thoroughly re-evaluated the physical basis of this configuration and expanded the associated discussion. Detailed modifications are provided in our specific responses to the comments cited above.

We further agree with the referee that a robust comparison with existing dual-domain approaches is necessary to justify our simplified approach for large-scale hydrology. Dual-domain configurations for frozen ground infiltration cover a wide range of complexity levels, from grid-cell permeability fraction (see Niu and Yang 2006) to models that explicitly simulate gravitational macropore flow or derive complex soil freezing curves (see Kurylyk and Watanabe 2013 and Mohammed et al., 2019). While highly accurate, integrating such complex dual-domain approaches into SVS would require a complete reformulation of soil water fluxes, which is beyond the scope of this study given the operational context of the SVS model. Consequently, during the review process, we have considered the simple dual-permeability configuration presented as 'Option SHP1' in Agnihotri et al (2023). This approach effectively accounts for permeable and impermeable grid-cell fractions using a freezing factor (F_{frz}), providing a balance between physical representativeness and computational efficiency.:

$$F_{frz} = \left(e^{-a\left(1-\frac{I}{W_{sat}}\right)} - e^{-a}\right)/(1 - e^{-a})$$

where I and W_{sat} are the ice fraction and the water content at saturation, respectively, and a is a scale-dependent parameter (a = 3 following Niu and Yang (2006)). More details on this configuration are provided in Agnihotri et al (2023). This approach has been implemented in SVS and we (re-)ran our GEM-Hydro setup in the Great-Lakes and Saint-Lawrence domain for the 2016-2021 evaluation period. Figure R1-1 shows a comparison of performance metrics for streamflow simulation between the AG23 configuration and the noFr, Fr and Fr-Inf configurations:

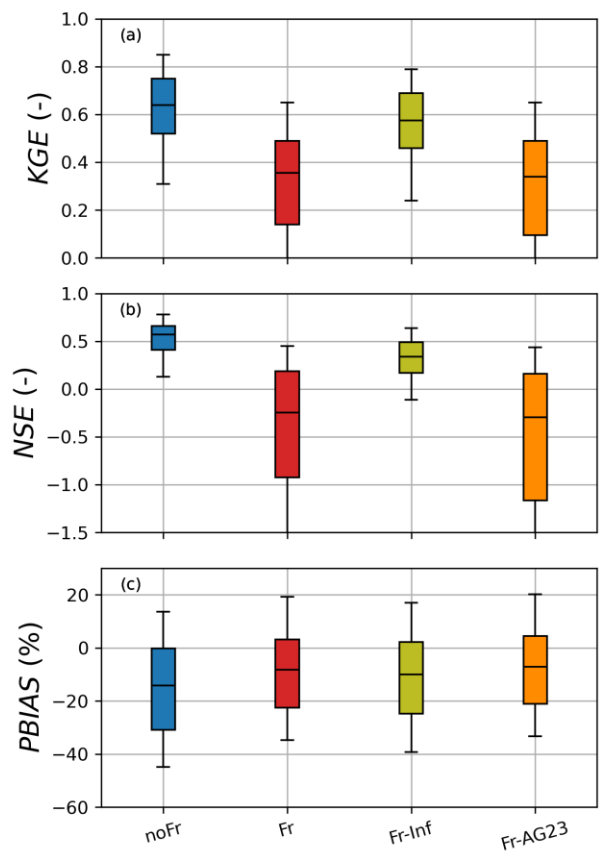

Figure R1-1: Boxplots of streamflow performances (KGE (a), NSE (b) and PBIAS (c)) for the three main configurations (noFr, Fr, Fr-Inf) and the AG23 configuration in the Great-Lakes and Saint-Lawrence domain for the 2016-2021 evaluation period. Details on boxplot representation are given in the legend of Fig. 4 of the manuscript. The target value for the KGE and NSE is 1.0 while the target value for the PBIAS is 0.0.

Our results indicate a clear degradation in performance when using the AG23 configuration compared to the Fr-Inf configuration, which can be attributed to the overly restrictive F_{frz} to infiltration used in AG23. Optimizing the performance of AG23 over the GLSL domain would require a site-specific calibration of the a parameter but it is highly uncertain that performance would reach that of Fr-Inf. Overall, this comparison suggests that the Fr-Inf configuration is better suited for improving frozen soil infiltration at the catchment scale despite not representing the sub-grid heterogeneity of soil water transport regime. We suggest the following modifications to the manuscript and the addition of Figure R1-1 to the Supplementary Materials:

ll. 631-635: "***A configuration of SVS using the dual-permeability approach of Agnihotri et al. (2023) was also evaluated. This simple configuration resulted in a marked degradation in model performance compared to the Fr-Inf configuration (Fig. S6). These results demonstrate that while Fr-Inf does not explicitly account for the spatial***

heterogeneity of subgrid macropore networks, it remains a suitable and effective approach for enhancing frozen ground infiltration and improving streamflow simulations at the catchment scale."

Ref1-Maj2. The calibration of alpha_MP is a central component of the study. However, the manuscript does not sufficiently address the potential for equifinality or the transferability of this parameter across different hydro-climatic regions. Are the optimal values of alpha_MP consistent across the diverse landscapes of the Great Lakes and Saint-Lawrence domain? A more detailed analysis of the spatial variability of the optimal alpha_MP and its relationship with soil/land-cover characteristics would significantly strengthen the paper.

This is a very good point. While Figure 4b in the original manuscript illustrates the spatial distribution of the optimal α_{no-imp} parameter, we agree that the relationship between land surface characteristics and α_{no-imp} deserved a deeper analysis.

In response to this comment and Ref2-Maj2, we extracted the dominant land surface types and average sand/clay fractions for the catchments associated with each hydrometric station. We then analyzed these characteristics in relation to the optimal α_{no-imp}, as shown in Figure R1-2:

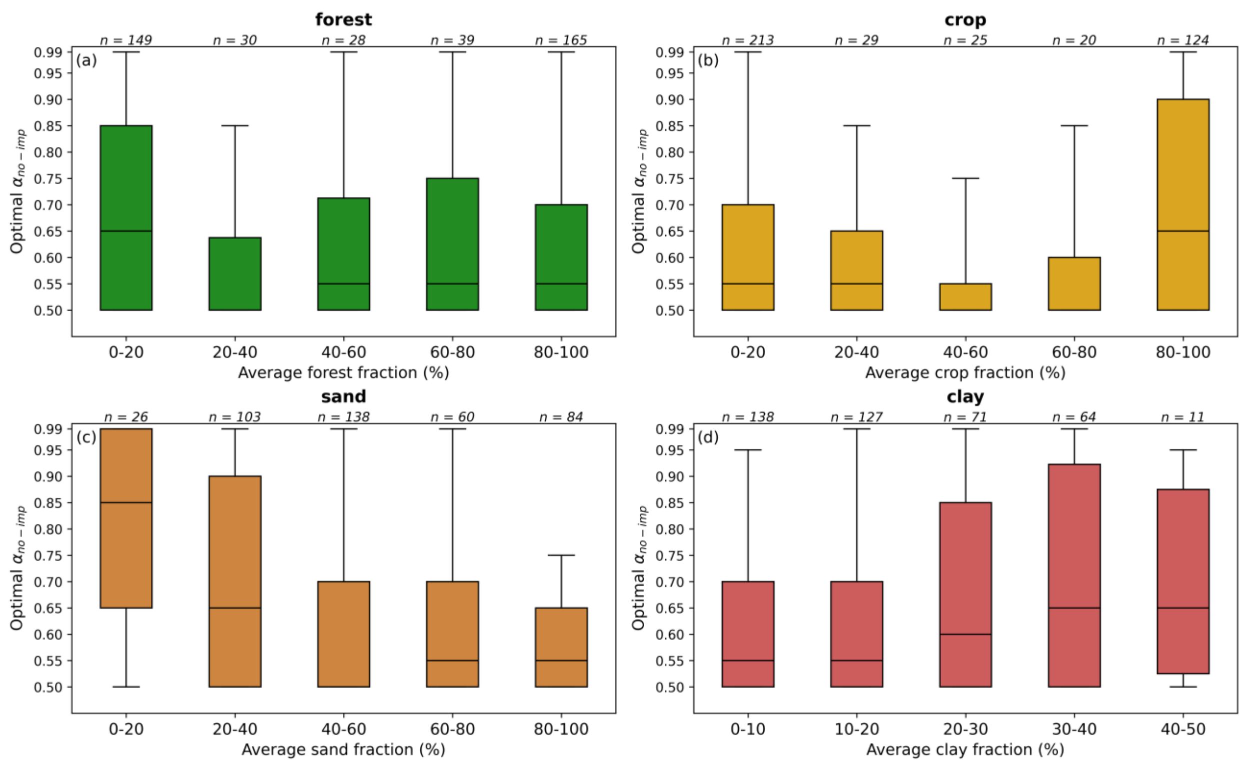

Figure R1-2: Boxplots of the optimal α_{no-imp} value in relation to the average forest (a), crop (b), sand (c) and clay (d) fractions for the enhanced frozen soil infiltration configuration (Fr-Inf). Simulations were performed using values of α_{no-imp} from 0.5 to 0.99 with increments of 0.05 on the Great-Lakes and Saint-Lawrence domain from 1 September 2016 to 31 August 2018. Details on boxplot representation are given in the legend of Fig. 4 of the main manuscript. The number of catchments for each surface cover fraction bin (n) is indicated on top of the boxes.

The results indicate that the optimal α_{no-imp} value varies according to both vegetation and soil texture. While the influence of land cover (forest vs. crops) is less pronounced, a clear pattern emerges regarding soil texture: the optimal α_{no-imp} decreases when the average sand fraction increases at the expense of clay fraction. This explains why the Fr-Inf configuration (using the default $\alpha_{no-imp} = 0.55$) performs better in catchments with higher sand fractions (see revised Figure 7 in the response to Ref2-Maj2). We have updated the manuscript to include this analysis and have added the supporting figure to the Supplementary Materials (Fig. S1):

ll. 398-403: "***However, the optimal*** $\alpha_{no\text{-}imp}$ ***varies considerably with land surface characteristics (see Fig. S1) as shown by generally larger optimal*** $\alpha_{no\text{-}imp}$ ***value in catchments of low forest fraction (0–20%) and high crop fraction (80-100%). Nonetheless, the optimal*** $\alpha_{no\text{-}imp}$ ***parameter exhibits greater sensitivity to soil texture. In particular, catchments for which low*** $\alpha_{no\text{-}imp}$ ***performs better are also characterized by a higher sand fraction. In contrast****, the few areas where larger values of* $\alpha_{no\text{-}imp}$ *perform better* ***are characterized by a higher clay fraction.***"

ll. 469-470: "***This aligns with the use of a fixed*** $\alpha_{no\text{-}imp}$ ***of 0.55 in Fr-Inf, which performs optimally in catchments with higher sand and forest fractions (Fig. S1).***"

ll. 649-650: "***Better performance achieved in forested and sandy catchments is attributed to the calibration of the*** $\alpha_{no\text{-}imp}$ ***parameter to 0.55, which is more representative of these specific landscape characteristics.***"

Ref1-Maj3. The results indicate that the Fr-MP configuration performs differently in agricultural areas compared to forested catchments. The authors attribute this to the interaction between tile drainage and frozen soil. I suggest a more in-depth analysis of this interaction. Does the current model structure adequately decouple the effects of macropores from those of anthropogenic drainage systems? This is crucial for the model's reliability in human-altered landscapes.

The need for a deeper analysis of the relationship between land cover, soil texture, and model performance was also raised by Referee #2. We have performed this analysis and thoroughly revised Section 3.3, including an updated Figure 7. We invite the Referee to refer to our responses to Ref2-Maj2 and Ref2-Min18 for the full details of these changes.

Regarding the representation of tile drainage and ploughing, these are currently implemented as multiplicative factors applied to the unfrozen soil hydraulic conductivities ($K_{sat,v}$ and $K_{sat,h}$) of specific soil layers. For ploughing, a multiplicative factor (m_v = 10) is included in the calculation of the ice impedance effect ($K_{sat,v,fr}$) when resolving the Richards equations for vertical soil water fluxes. However, in the calculation of $K_{sat,h}$, which is derived from $K_{sat,v}$, the 'natural' value of $K_{sat,v}$ (unaffected by ploughing) is used to ensure that ploughing does not artificially amplify lateral flow in the top three soil layers.

Similarly, the effect of tile drainage, which occurs in the fifth soil layer, is applied to the $K_{sat,h}$ value adjusted for the presence of soil ice. When the enhanced infiltration condition is met in agricultural areas ($W > \alpha_{no\text{-}imp} W_{sat}$), $K_{sat,v}$ is restored to its unfrozen value and amplified by the ploughing multiplicative coefficients. To avoid any confusion, we suggest the following modifications to the manuscript:

ll. 404-407: "***Additionally,*** *these stations are all located at the outlet of highly agricultural catchments (see Fig. 2), where* ***the land surface is modified by agricultural tile drainage and often result in flashy runoff response. This behavior is more accurately captured by model configurations that are more restrictive to infiltration, even when accounting for amplified coefficients used to represent the effects of ploughing and tile drainage.***"

ll. 517-519: "***In this specific catchment, the multiplicative factors of*** m_h ***= 500 and*** m_v ***= 10 may be insufficient to adequately represent the effects of agricultural drainage, highlighting a limitation of this non-explicit approach (Gaborit et al., 2025).***"

Ref1-Maj4. While the authors evaluate the impact on TT and TD, the improvements are described as "slight" or "neutral." Given that the primary goal is to improve streamflow, the authors should clarify whether these meteorological improvements are statistically significant and if they have any meaningful impact on the broader numerical weather prediction (NWP) performance at ECCC.

Our objective in presenting these results was to demonstrate that the inclusion of soil freezing processes (with or without enhanced infiltration) does not degrade the ability of the model to predict near-surface temperature and humidity. As our primary goal was not to optimize these specific variables, we did not initially perform rigorous significance testing on the Fr-Inf configuration for these metrics.

However, to address this comment, we have provided a figure generated by our internal EMET evaluation tool (Fig. R1-3). The results indicate that improvements in near-surface air temperature (TT) bias primarily occur during nighttime hours (UTC-5). Furthermore, the 90% confidence intervals remain above zero, suggesting that these gains are statistically significant (Fig. R1-3b). This example illustrates the difference between the Fr-Inf and noFr configurations for air temperature bias over the period from March to July 2018. While these secondary improvements are encouraging, we have kept the focus of the manuscript on the hydrological impacts. Nonetheless, we propose the following addition to the manuscript:

ll. 571-572: "***In general, improvements are statistically significant during nighttime (not shown).***"

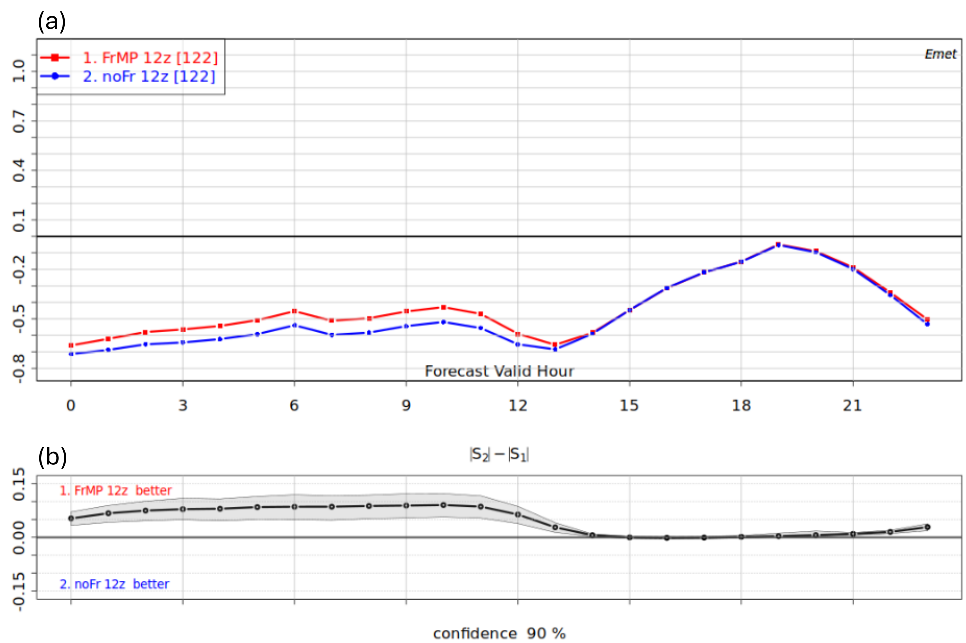

Figure R1-3: (a) Mean near-surface (2-m) temperature bias of the Fr-Inf (FrMP) and the noFr experiments for the period between 2018-03-01 and 2018-06-30. (b) Bias difference between both experiments with the gray shading showing the 90% confidence interval. The x-axis shows daily hours at UTC + 0.

Minor Comments :

Ref1-Min1. Page 2, Line 40: The term "discrepancy" is used well here; ensure this is consistently linked to the scale-dependency of the processes throughout the discussion.

In response to Ref3-Maj1, we have reframed the narrative of the study and thoroughly revised the Introduction. In the revised manuscript, the term "discrepancy" is used only once (in the Results section) to specifically describe model performance degradation.

Ref1-Min2. Page 7, Eq. 9: Please clarify if alpha_MP is a static parameter or if it could be dynamic based on seasonal vegetation growth.

In the model development presented here, $\alpha_{no\text{-}imp}$ is a static parameter. We have clarified this in the revised Section 2.1.3). It should be noted that while this parameter is currently fixed, the Fr-Inf configuration may be further refined in future iterations of the SVS model to include more dynamic representations of frozen soil infiltration (as discussed in our response to Ref2-Min18).

Ref1-Min3. Page 8, Figure 1: The conceptual diagram is excellent. Please ensure the legend clearly distinguishes between the "matrix flow" and "macropore flow" pathways in panel (c).

We thank the reviewer for this remark. In SVS, vertical soil water fluxes are consistently simulated as matrix flow. Under the “enhanced frozen soil infiltration” condition ($W > W_{no\text{-}imp}$), the parameterization effectively eliminates the ice-impedance effect on these fluxes. We have modified Figure 1c to more clearly illustrate this process and to ensure that the distinction between standard matrix flow and the enhanced infiltration configuration is clear.

Ref1-Min4. Page 10, Section 2.4.1: The exclusion criteria for stations (NSE < 0 or KGE < -0.41) are reasonable, but please provide a brief justification for why these specific thresholds were chosen over others.

We received a similar remark from Referee #2. The suggested correction is presented in our response to Ref2-Min5.

Ref1-Min5. Page 13, Line 355: The mention of the "perched aquifer" in Michigan is interesting. Could the authors provide a bit more detail on how this specific geological feature might be better represented in future iterations of the model?

There is no plan on the short-term to better represent complex aquifers in Watroute, which currently relies on a simple conceptual reservoir with a power law function to simulate baseflow (this is also the case for the Raven model). This simple representation is generally enough to represent the effect of superficial (free surface) aquifers on river flows, even with static parameters for the power law function for most basins across Canada.

To better represent complex aquifers, one could use a hydro-geological model instead of this simple conceptual reservoir. However, such a model is not trivial to setup and would add significant complexity to the current hydrological forecasting system, possibly increasing computation time and latency of the forecasts.

One relatively easy solution could be to try to determine climatological baseflows from applying a digital filter to the observed streamflow for regions where complex (confined) aquifers exist, and use the climatological baseflow instead of the baseflow computed from the simple conceptual reservoir. Indeed, even if the interaction between different aquifers of a region can be complex, the effect of confined aquifers on streamflow can sometimes be fairly simple to represent, since such confined aquifers can have a very slow response time to meteorology and their contribution to baseflow can remain quite constant through time. But this would still require quite some work, and the methodology is not fully clear either, especially with regard to the interpolation of such climatological baseflows or their extrapolation to ungauged areas.

Finally, in the real-time hydrological forecasting system used at Environment and Climate Change Canada named NSRPS (National and Surface River Prediction System), a baseflow assimilation procedure is currently in place, allowing to correct the state of the simple conceptual reservoir based on estimated baseflow derived from filtered observed flows. These corrections have a long inertia in terms of their effect on forecasts, that can generally last up to more than 10 days. Therefore, it is not even sure if putting more efforts into better representing baseflow in areas with complex aquifers would actually have a strong impact on the flow forecasts of such regions, compared to the current strategy involving the correction of the simple conceptual reservoir.

Ref1-Min6. Page 16, Line 411: The observation that only 4 stations show a Delta KGE < -0.5 is a strong point. Please emphasize this in the abstract as a measure of the model's robustness.

We suggest the following addition to the abstract:

ll.17-19: “***Strong degradations relative to the no freezing configuration (ΔKGE < –0.5) are observed only at 4 stations with Fr-Inf (< 1%) as opposed to 172 stations under the Fr configuration (33%), highlighting the robustness of the approach.***”

Ref1-Min7. Page 23, Line 495: The authors mention that wetlands and meanders are not fully represented in Watroute. It would be beneficial to include a brief sentence on how the transition to the "Raven" routing scheme might specifically address these limitations.

The current main limitation of Watroute with regard to meanderous rivers and relatively flat regions in general, is that the model assumes the same overbank slope (1%) everywhere. Therefore, it cannot represent what happens when a river floods and water spreads in large areas along the river. This tends to significantly decrease peak flow magnitude and lengthen the response time of the flood. Even if a spatially-varying overbank slope could be used based on DEMs, we still wouldn't fully capture the impact that wetlands can have during river floods, in such regions.

Raven includes more options to represent these processes, for example by defining some subbasins as "wetlands" that can capture part of a river flow through lateral transfer of water. See this citation of the Raven Manual 4.0 at page 95, paragraph 5: "larger lakes or wetlands systems could be more explicitly represented by abstracting laterally to WETLAND HRUs via, e.g., the :LateralFlush process, and there conditionally applying seepage, evaporation, and overflow. The benefit that Raven could have in such flat or wetland areas, compared to Watroute, has not been assessed yet, to the extent of our knowledge. This will be an interesting thing to assess when comparing both routing schemes in the context of open-loop runs.

We suggest the following revisions to the manuscript:

ll. 688-693: "***By design, Watroute cannot explicitly represent lateral spreading of water in flat areas adjacent to the river, as the model assumes a uniform overbank slope of 1%.*** *This likely contributes to the poor performance observed in the region bounded by the five Great Lakes (Fig. 7) and the exclusion of several stations for analysis in this region (Figs. 3 and 5). The envisioned transition in NSRPS from Watroute to the Raven routing scheme,* ***which includes an explicit representation of wetlands*** *(Craig et al., 2020), in the coming years may help alleviate these issues.*"

Ref1-Min8. Clarity of Figures: In Figure 4 and Figure 5, the color scales are effective, but please ensure that the symbols for different drainage areas are easily distinguishable in the printed version.

We appreciate the suggestion to use varying symbol sizes to represent catchment scale. We tested using different symbols and larger markers, but we found that these changes significantly reduced the readability and clarity of Figures 3 and 5, particularly in areas with a high density of stations. As for Figure 4b, the discrete color-scale makes it fairly easy to distinguish between stations of low and high optimal $\alpha_{no\text{-}imp}$ values.

Since the primary objective of these figures is to illustrate spatial patterns through the color-coded performance metrics, we have opted to retain the original symbol size to ensure the maps remain legible. Additionally, as Figure 7 has been resized in the revised manuscript (in response to Ref2-Maj2), the original symbol size was found to be the most effective for maintaining clarity across all map-based figures.

Ref1-Min9. References: Ensure that all cited preprints (e.g., Bauer et al., 2025) are updated to their final publication status if available by the time of revision.

The manuscript of Bauer et al. 2026 has just been published in its final form in *The Cryosphere* (TC) at the time of submitting this document (17 June 2026). We suggest citing the TC publication in the revised manuscript. Note that the manuscript of Khedhaouiria et al. (2026) is still only published as a preprint. When submitting the final version of our manuscript, we will cite the most up-to-date version of this article.

egusphere-2026-928 - A non-explicit representation of macropores in the SVS land surface model improves streamflow simulations under frozen soil conditions

Responses to the Anonymous Referee #2

We thank the referee for carefully reading the manuscript and for providing valuable feedback. We are pleased that the referee appreciates the relevance of our work in the context of operational hydrological forecasting. Based on comments from Referee #2 and those from two other anonymous reviewers, we propose significant modifications to the original manuscript, which we believe will improve the paper. Specifically, concerns about the narrative/physical interpretations (also explicitly raised by another referee) were addressed by extensively reformulating the narrative to avoid overstating the physical meaning of the structural changes brought to the model. While we respond to Referee #2 point-by-point here, an exhaustive breakdown of this restructuring can also be found in our response to Referee #3. Major changes also include the addition of two new analyses of model calibration and performance based on land surface characteristics. Note that "Macropore configuration", "Fr-MP" and "a_{MP}" are denoted "Enhance frozen soil infiltration configuration", "Fr-Inf" and "$a_{no\text{-}imp}$" in this document and in the proposed revised version of the manuscript. Our answers below are in blue, whereas excerpts from the manuscript are in *blue italics* with modifications in ***bold***. In our responses, when referring to other comments from any referee, we use the following terminology: RefX-[Maj or Min]Y where X and Y indicate the referee and comment (major or minor) ID.

Referee comments 2 – egusphere-2026-928

General comment:

This study presents a computationally efficient approach to improve infiltration under frozen soil conditions in a land surface model. The manuscript clearly demonstrates that the current soil freezing formulation leads to unrealistic runoff responses and that increasing infiltration can substantially improve streamflow simulations at large spatial scales. The topic is highly relevant, particularly in the context of operational hydrological forecasting, and the study provides a valuable and pragmatic contribution in this regard.

However, the proposed "macropore representation" is, in its current form, a conceptual parameterization that mimics the effect of preferential flow rather than explicitly representing macropore processes. The implementation effectively removes the ice-induced hydraulic impedance once a soil moisture threshold is exceeded and additionally modifies the runoff generation scheme. As such, the approach represents a combined structural modification of flow partitioning rather than a direct representation of macropore flow.

Ref2-Maj1. In its current form, the manuscript does not sufficiently disentangle these effects. A clearer separation of the impacts of the increased vertical hydraulic conductivity and the modified runoff generation would be necessary to properly attribute the improvements in model performance.

This point aligns with the concerns raised in Ref3-Maj1. To address this, we have conducted two additional SVS-Watroute coupled experiments over the GLSL domain between 2015-2021 to isolate the effects of the two structural modifications:

- Fr-SATSFC0: This experiment is identical to the default soil freezing configuration (Fr) but disables the subgrid-scale saturation mechanism for surface runoff generation.
- Fr-Inf-SATSFC1: This experiment utilizes the enhanced frozen soil infiltration configuration (Fr-Inf) but removes the modification to surface runoff generation.

Figure R2-1 presents the KGE, NSE, and PBIAS for the primary experiments (noFr, Fr, and Fr-Inf) alongside these two intermediate configurations. The results show that while the intermediate configurations offer marginal improvements in KGE and NSE compared to the baseline Fr configuration, they are significantly outperformed by the Fr-Inf configuration.

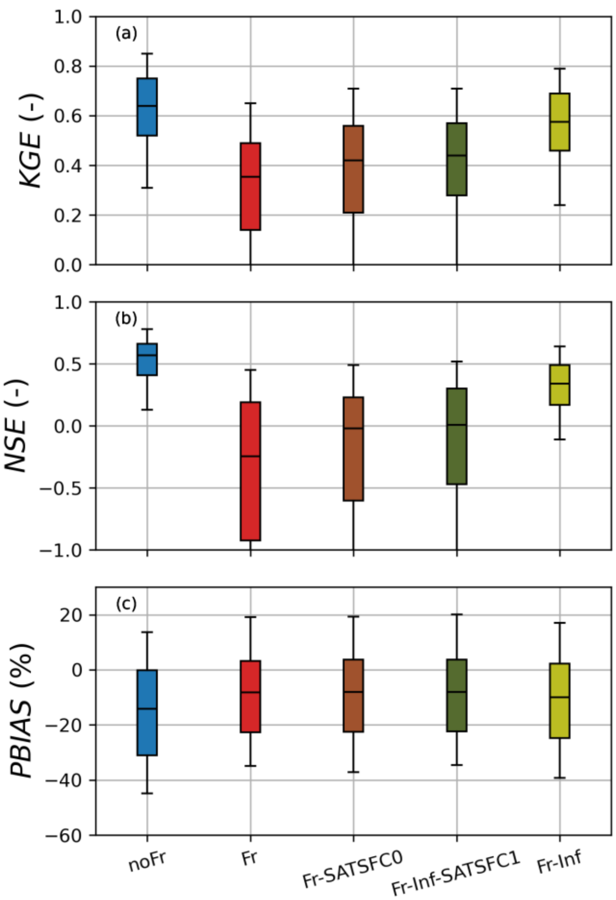

Figure R2-1: Boxplots of streamflow performances (KGE (a), NSE (b) and PBIAS (c)) for the three main configurations (noFr, Fr, Fr-Inf) and the two intermediate configurations (Fr-SATSFC0 and Fr-Inf-SATSFC1) in the Great-Lakes and Saint-Lawrence domain for the 2016-2021 evaluation period. Details on boxplot representation are given in the legend of Fig. 4 of the manuscript. The target value for the KGE and NSE is 1.0 while the target value for the PBIAS is 0.0.

This demonstrates that the combination of both structural modifications, namely the relaxation of ice impedance on vertical flow and the restriction of saturation excess for surface runoff generation are required to achieve a reasonable improvement in streamflow simulations when soil freezing is enabled. Based on this analysis, we suggest the following modification to the discussion of the manuscript and the addition of Figure 2-1 to the Supplementary Materials as Figure S7:

ll. 677-685: "***To isolate the effects of each modification, we conducted two intermediate frozen soil sensitivity experiments (Fig. S7). Disabling the subgrid-scale surface runoff generation as the sole modification resulted in only slight improvements over the default soil freezing configuration (Fr). Similarly, applying the Fr-Inf configuration while restoring the surface runoff generation mechanism yielded only minor performance gains compared to Fr. These tests demonstrate that both modifications are necessary to meaningfully improve model performance under frozen soil conditions and should therefore be implemented together in SVS. However, while these combined changes successfully reduce rapid-response fluxes, the Fr-Inf configuration still overestimates mid-winter runoff peaks (Figs. 8 and 9). This can be partly explained by*** *the near-surface anisotropy ratio ($K_{sat,h}/K_{sat,v}$)*, ***which*** *is of the order of 1000* ***and*** *strongly promotes lateral flow over infiltration.*"

Ref2-Maj2. Furthermore, the physical interpretation of the results could be strengthened. In particular, the manuscript would benefit from a more explicit discussion of:

- the conceptual nature and limitations of the parameterization,
- the implications of representing subgrid-scale preferential flow using a bulk threshold formulation,
- the spatial variability in model performance, especially in relation to land use, soil texture, and the exclusion of stations from the evaluation.

We agree that the physical interpretation of our conceptual approach to enhanced frozen soil infiltration required a more in-depth discussion. In the revised manuscript, we have reframed the narrative to clarify the structural changes made to the runoff/infiltration partitioning and to explicitly address the inherent limitations of this configuration (as also suggested in Ref3-Maj1).

To address concerns regarding the representation of subgrid preferential flow, we implemented a dual-domain approach based on Agnihotri et al. (2023). Our comparison (detailed in the response to Ref1-Maj1) indicates that this subgrid approach did not yield clear performance improvements. This suggests that a bulk threshold formulation remains a suitable and computationally efficient alternative for representing macro-scale frozen soil processes in large-scale hydrological simulations. Following Ref2-Min4, we have also added a more explicit discussion of these conceptual limitations to the manuscript.

We also thank the Referee for the suggestion regarding spatial variability. We have performed a detailed analysis of model performance in relation to land cover (forest vs. crop fraction) and soil texture (sand vs. clay fraction). These findings have led to significant insights, and we have entirely revised the latter half of Section 3.3, including a modified Figure 7, to reflect these new results. We believe that this addition addresses several comments from the Referee and strengthens significantly the manuscript:

ll. 463-499: "***Figure 7 illustrates the spatial distribution of KGE, NSE, and PBIAS for the Fr-Inf experiment, alongside the corresponding forest-crop and sand-clay fractions for each catchment.*** *A* ***distinct*** *spatial pattern emerges* ***across*** *the GLSL domain* ***with the Fr-Inf configuration reaching its*** *highest* ***scores*** *for* ***all*** *three metrics in the Saint-Lawrence valley and north of Lake Superior and Lake Huron.* ***These regions are characterized by a predominance of evergreen and mixed forests (Fig. 2) and sandy soils. Stations yielding KGE scores above 0.8 (yellow dots on Fig. 7a-c) are consistently associated with higher forest and sand fractions. Conversely, performance declines to approximately 0.4 in the regions between Lake Huron, Lake Ontario, and East of Lake Erie, which are areas dominated by crops and higher clay content.*** *This aligns with the use of a fixed* $\alpha_{no\text{-}imp}$ *of 0.55 in Fr-Inf, which performs optimally in catchments with higher sand and forest fractions (Fig. S1). The lowest* ***KGE (< 0)*** *are obtained in the northern region between Lake Michigan and Lake Huron, and West of Lake Michigan,* ***regions with a high concentration of stations excluded from the experiment comparisons (red crosses on Figs. 3 and 5). One must note that the noFr and Fr experiments also exhibit better KGE in forested and sandy areas compared to agricultural and clayey regions (Figs. S2 and S3). This suggests that while activating soil freezing with enhanced infiltration modulates the KGE magnitude, it does not fundamentally drive the underlying spatial variability in model performance.***

The spatial pattern of the NSE closely follows that of the KGE, with the best results obtained in the northern and eastern parts of the GLSL domain (NSE > 0.5), and the worst scores obtained in the southern and western parts of the domain (NSE < 0.2). ***Accordingly, the NSE is better at stations associated with high forest and sand fractions.*** *The similarity between the KGE and the NSE implies that the KGE can be explained by its variability component at most stations****. Interestingly, the Fr experiment performs poorly (NSE < 0) across the majority of the domain, with the exception of the region bounding Lake Erie (Figs. S3). Specifically, the Fr configuration yields higher NSE values in catchments characterized by clay-rich soils, a trend that contrasts with the results of the noFr and Fr-Inf***

experiments (Figs. S2 and 7). This suggests that the NSE, which reflects the ability of the model to capture the timing and amplitude of discharge peaks, is more sensitive to soil texture characteristics than the KGE.

*In line with the KGE and NSE criteria, the absolute PBIAS is minimal (absolute values below 10%; pale blue or red on Fig. 7c) for many stations located in the Saint-Lawrence Valley and North of Lake Superior and Lake Huron****, where the soil tends to be sandy rather than clayey.*** *In these regions, the* ***Fr-Inf*** *experiment* ***slightly*** *overestimates streamflow (blue dots)* ***in general****, though it tends to* ***result in strong underestimations*** *elsewhere. The positive bias is attributed to the soil freezing configuration that remains too restrictive to infiltration despite* ***the increase of infiltration****. Consequently, most stations in the eastern part of the domain, where the bias is positive, are also stations where the* ***Fr-Inf*** *degrades the KGE compared to noFr (Fig. 5b). The overall negative bias in the rest of the domain can be explained by an underestimation of streamflow during periods of low-flow regime. As the available pore space for liquid water decreases with growing soil ice, lateral flow likely prevails over soil drainage. This reduces the liquid water that contributes to support base flow.* ***This behavior is consistent across all model configurations but is particularly pronounced in soils with higher clay fractions.***

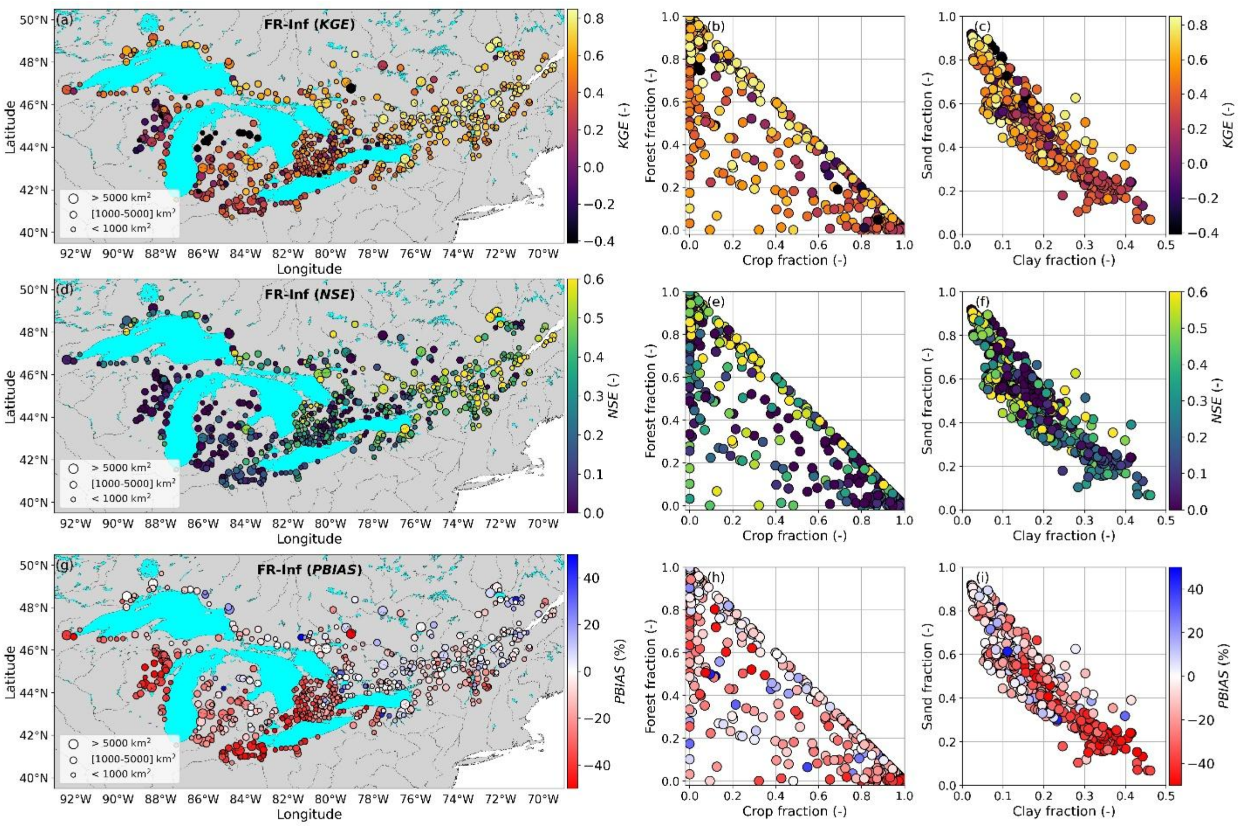

Figure 7: ***Spatial performance and catchment characteristics for the Fr-Inf experiment (2016–2021). Left panels (a, d, g) show the Great Lakes and St. Lawrence domain with KGE, NSE, and PBIAS at each station. Right panels (b, c, e, f, h, i) show corresponding forest-crop and sand-clay fractions categorized by performance scores.*** *In (a) to (f), a good performance of the model is represented by yellow shades. In (g) to (i), underestimation of streamflow is shown by red colors while an overestimation is shown by blue colors with pale red and blue dots show a low absolute bias.*"

Figure S1 will also be replaced by Figures S2 and S3 which are similar to Figure 7 but for the noFr and Fr configuration, respectively.

With respect to these changes, we also suggest the following addition to the methods:

ll. 309-311: "***For the upstream catchment related to each station, we extracted the corresponding land surface types and the depth-averaged soil texture (to 3 m) from the geophysical field dataset.***"

Ref2-Maj3. Finally, the transferability of the calibrated parameter α_MP within and beyond the study domain is not addressed and should be discussed.

In response to Ref1-Maj2, we conducted a detailed analysis of catchment characteristics (forest-crop and sand-clay fractions) relative to the optimal $\alpha_{no\text{-}imp}$ values identified in Section 3.2. This analysis reveals that a calibrated $\alpha_{no\text{-}imp}$ to 0.55 is particularly well-suited for catchments dominated by sand and forest, rather than those with high agricultural and clay content. This explains the superior model performance in these specific environments. Detailed revisions to the manuscript and Supplementary Materials are outlined in our responses to Ref1-Maj2.

Regarding the transferability of the calibrated $\alpha_{no\text{-}imp}$ beyond the GLSL domain, an essential next step is to evaluate the Fr-Inf configuration across other large-scale hydrological domains. Assessing the robustness of this approach and its calibration across diverse geographic regions is a prerequisite for its implementation into our operational forecasting systems.

Ref2-Maj4. Overall, I consider the manuscript suitable for publication after revision. The proposed approach is particularly interesting from an operational perspective, as it provides a simple and efficient way to improve infiltration under frozen conditions. I am especially interested to see how this concept will evolve in future work toward more physically based representations of preferential flow processes.

We thank the referee for showing interest in our work. We believe that the revised version manuscript, based on comments from all three referees, will strongly improve and tighten the manuscript.

Minor comments and technical notes:

Ref2-Min1. L. 44: The sentence is difficult to follow. Consider restructuring it for clarity (e.g. "This makes the representation … complex and challenging.").

This sentence will be removed from the revised version of the introduction (see Ref3-Maj1)

Ref2-Min2. L. 87, 204, 250, 273, Figure 1 caption: The manuscript frequently refers to a "macropore representation" or "macropore configuration". Given that the approach does not explicitly represent macropore flow but rather mimics its effect on infiltration, I suggest using a more precise terminology (e.g. "conceptual macropore parameterization" or "macropore flow correction") consistently throughout the manuscript.

References to "macropores" when describing the new configuration will be replaced by "enhanced frozen soil infiltration" in the revised version of the manuscript (see Ref3-Maj1).

Ref2-Min3. L. 150: The statement that a unique residual unfrozen water content is assigned independently of soil temperature is somewhat unclear, as Eq. (1) explicitly includes temperature. I may be misinterpreting the formulation. However, it would be helpful to clarify whether this temperature dependence is averaged out to obtain a constant value for each soil type.

Indeed, the residual unfrozen water content corresponds to the average of W_{res} estimated with temperature (T_i) values of 263.15 K to 271.15 K by increments of 2 K. We suggest the following modification to the manuscript:

ll. 176-179: "*It corresponds to the average of the values obtained from Eq. (3) of Niu and Yang (2006) for* ***soil temperatures (T_i) from 263.15 to 275.15 K by increments of 2 K*****:**

$$W_{res} = \frac{W_{sat}\sum_{i=1}^{n=5}\left[\frac{10^3 L_f\left(T_i - T_{ref}\right)}{g T_i \psi_{sat}}\right]^{-1/b}}{n} \quad (9)$$

where L_f and g are the latent heat of fusion (334 J kg^{-1}) and the gravitational constant (9.81 m s^{-2}), respectively."

Ref2-Min4. L. 214: The proposed parameterization represents a strong simplification of preferential flow processes. In particular it does not account for dynamic effects such as refreezing or the dependence of macropore flow on soil type and saturation. Some experimental studies even suggest that preferential flow can decrease at high saturation levels or vary significantly with soil structure. A short discussion (maybe later in the manuscript) of these limitations would strengthen the manuscript.

In the revised manuscript, we have reframed the proposed configuration as a structural modification designed to enhance frozen ground infiltration, rather than an explicit representation of macropore preferential flow. Section 2.1.3 (ll. 223–245) has been updated to reflect this change in terminology (see Ref3-Maj1).

We acknowledge that the literature presents complex findings on this topic. While some studies suggest that preferential flow is more prevalent in dry soil conditions (e.g., Merdun et al., 2008; Hardie et al., 2011), research specifically investigating frozen soils (e.g., Demand et al., 2019; Bauer et al., 2026) indicates that high antecedent moisture can actually facilitate rapid infiltration. This occurs because water in smaller pores may freeze first, leaving larger pores available for water transport. By reframing our approach as a structural enhancement to the infiltration partitioning, we maintain the physical reasoning backing the Fr-Inf configuration while acknowledging these complex subgrid processes.

We propose the following addition to discussion:

ll. 640-645: "***While some studies have shown that preferential flow is favored under low antecedent soil moisture (Hardie et al., 2011; Merdun et al., 2008), the approach proposed here relies on the exceedance of a threshold, derived from sensitivity analysis, to increase frozen soil infiltration. This is supported by empirical evidence suggesting that water may freeze preferentially in smaller pores, leaving larger pores available for rapid infiltration (Demand et al., 2019; Mohammed et al., 2019; Bauer et al., 2026). Nevertheless, this remains a structural modification to the SVS soil freezing scheme which does not explicitly account for the subgrid physical processes that drive frozen ground infiltration, such as macropore preferential flow.***"

ll. 659-660: "***Finally, given the reliance of the SVS vertical flux scheme on Darcian flow (Alavi et al., 2016), we tried to mimic preferential flow*** *to increase vertical hydraulic conductivity ($K_{sat,v}$)* ***but this*** *resulted in numerical instabilities.*"

Ref2-Min5. L. 311: The exclusion of stations based on NSE/KGE thresholds is understandable. However, it would be helpful to briefly explain the choice of the KGE threshold (-0.41) and its interpretation, as this value is not immediately intuitive for the readers.

We suggest the following modification to the manuscript:

ll. 330-332: "*When comparing two or more experiments for a given station, we decided to exclude the station from the analysis if the NSE or KGE of at least one experiment was lower than 0 and –0.41, respectively,* ***indicating that the mean of the observations is a better predictor of streamflow than the model (see Eq. 11 and 12 and*** *Knoben et al., 2019).*"

Ref2-Min6. L. 359: There appears to be a missing "Fr-" before the word "experiment".

Thanks for raising this up. This will be corrected.

Ref2-Min7. Figure3/Figure 5: The spatial distribution of excluded stations (red crosses) shows a pronounced clustering in specific regions rather than a random pattern. While the manuscript discusses potential causes for poor model performance in these areas, it remains unclear whether the station filtering systematically removes specific hydro-climatic or physiographic conditions from the evaluation. Please clarify how this affects the representativeness of the results.

In Figure 3, 5a and 5b, clusters of red crosses appear west of Lake Michigan and between Lakes Michigan and Huron, These markers indicate that none of the tested model configurations resulted in a NSE > 0 or a KGE > –0.41. This poor performance by all three configurations may be attributed to the fact that surface-ground water exchanges, flow regulations and wetland processes are not currently represented in SVS or Watroute.

Moreover, in Figure 5a, a third cluster of red crosses emerges northwest of Lake Ontario, indicating that only the noFr configuration meets the inclusion criteria in this area. This suggests that even with the enhanced frozen soil infiltration configuration, infiltration remains overly restricted in this specific region. We propose adding the following sentences to the manuscript:

ll. 378-379: "***This may also explain the emergence of excluded stations (red crosses) northwest of Lake Michigan and between Lakes Michigan and Huron.***"

ll. 435-437: "***The concentration of excluded stations northwest of Lake Ontario suggests that infiltration is still insufficiently captured in this area, even with the Fr-Inf configuration, indicating that the model remains too restrictive to frozen ground infiltration in certain localized conditions.***"

ll. 689-691: "*This likely contributes to the poor performance observed in the region bounded by the five Great Lakes (Fig. 7)* ***and the exclusion of several stations for analysis in this region (Figs. 3 and 5).***"

Ref2-Min8. L. 372: Please consider using more precise terminology such as "macropore parameterization" instead of implying an explicit representation.

As for Ref2-Min2, this will be revised in the revised manuscript.

Ref2-Min9. L. 406: Please clarify whether this statement applies to all stations or only the majority of them.

This applies to the majority of stations. It will be corrected

Ref2-Min10. L. 408: The underlying reasons for the observed behavior are not entirely clear. Additional explanation would improve the interpretation.

Additional explanation will be added (see Ref2-Min7)

Ref2-Min11. L. 414: Could the observed spatial variability in model performance be linked to differences in land use or soil properties? While some aspects are discussed, it might be helpful to further elaborate on the controlling factors (e.g. soil texture, land cover, or hydrological regime).

This point was addressed in response to Ref2-Maj2.

Ref2-Min12. L. 418: The results indicate substantial performance degradations associated with the soil freezing configuration in some regions. While general limitations of the freezing scheme are discussed later, it would be helpful to more explicitly link these limitations to the observed regional patterns in model performance.

In the revised section 3.3, we provide a deeper interpretation of the poor performance of the Fr experiment (especially with respect to the NSE). Please refer to Ref2-Maj2 for the proposed modification.

Ref2-Min13. L. 421: The influence of station exclusion on the interpretation of PBIAS should be clarified, especially since different configurations are evaluated on different subsets of stations. This may affect the comparability of the reported bias between experiments. In addition, the use of the term "demonstrates" appears too strong in this context. Given the differences in station subsets and the indirect nature of the comparison, "suggests" may be more appropriate.

In Figure 6, we compare the three experiments using a consistent subset of 521 stations where at least one configuration achieved an NSE > 0 or a KGE > –0.41. This subset includes several stations where the Fr experiment exhibits poor performance in terms of KGE and NSE, often due to the generation of unrealistic mid-winter and spring discharge peaks. These overestimations in peak flow effectively compensate for the model's general tendency to underestimate baseflow as a result of the lack of a water table representation. This phenomenon of compensating errors explains why the Fr experiment may appear to outperform the noFr configuration in terms of PBIAS, despite significant degradations in KGE and NSE scores. We suggest the following precision added to the manuscript:

ll. 450 to 455: "*The noFr and the Fr experiments result in the largest and the smallest negative biases, respectively (Fig 6c),* ***contrasting with the KGE and NSE****.* ***This may be caused by the multiple unrealistically high mid-winter and spring peaks of streamflow which tend to compensate for SVS general tendency to underestimate baseflow.*** *More interesting, however, is that the PBIAS of the Fr-MP experiment is nearly equal to that of the Fr experiment. It* ***suggests*** *that the large improvements in KGE and NSE resulting from the addition of* ***the enhanced frozen soil infiltration*** *configuration do not exacerbate the general underestimation of streamflow.*"

Ref2-Min14. L. 439: While some explanations are provided, the underlying drivers of the observed spatial patterns are not fully clear. Could these patterns be more explicitly linked to dominant hydrological processes (e.g. snowmelt dynamics, flow partitioning, or model structural limitations)?

The analysis of performance scores based on dominant land cover and soil texture provides a clearer understanding of the spatial variability in model performance. We have linked these results to the soil water balance of SVS under each model configuration. Full details of this analysis and the associated revisions are provided in our response to Ref2-Maj2.

We did not perform a specific analysis of the relationship between hydrological regimes and spatial performance patterns because several key hydrological processes (e.g., surface-groundwater interactions, wetland processes) are not yet represented in SVS or Watroute. Consequently, any conclusions drawn from such an analysis would remain largely speculative. We have chosen to focus our discussion on the land surface and soil texture characteristics, for which we have robust data and direct model sensitivities.

Ref2-Min15. Figure 8: Adding a finer temporal reference (e.g. months or seasons) to the x-axis would improve readability and help distinguish between winter, snowmelt, and summer events.

We will modify the x-axis of Figures 8, 9a and S4 to show a finer temporal resolution.

Ref2-Min16. Furthermore, differences between the model configurations appear to persist during summer periods, when soil freezing should not play a significant role. This may be related to the modified runoff generation scheme introduced together with the macropore parameterization. Please clarify whether such differences under unfrozen conditions are expected within the current model formulation.

Thank you for raising this up. Indeed, it is possible that differences arise in summer (i.e. under unfrozen conditions) due to modifications brought to surface runoff generation mechanism. However, since subgrid-scale surface layer interflow only happens when the surface layer is saturated, which seldom occurs when the surface layer is unfrozen, this situation is fairly rare. The following modifications will be added to the revised manuscript.

l. 240: "***This seldom occurs in the absence of ice in the topmost soil layer.***"

ll. 522-524: "***On both Figs. 8 and S4, differences between the assessed SVS configuration under unfrozen soil conditions arise due to the structural modification to surface runoff generation. These differences remain uncommon since it necessitates the saturation of the surface layer, which rarely occurs without modelled soil ice.***"

Ref2-Min17. Figure 9: Please clarify whether the improved agreement is related to specific freezing conditions during the selected period.

We selected the hydrographs for station 02HL001 during the 2018–2019 winter because they effectively illustrate the primary impacts of the Fr-Inf configuration. Specifically, Fr-Inf attenuates unrealistically high mid-winter peaks produced by the Fr configuration and provides a more accurate representation of both the timing and magnitude of the spring freshet. While specific meteorological and freezing conditions in any given year inevitably influence model performance, the 2018–2019 period is representative of the general behavior observed across the domain. In most cases, the Fr-Inf hydrograph sits between the noFr and Fr simulations regarding baseflow and rapid-response fluxes (surface runoff and lateral flow), providing a more physically consistent agreement with observations than the default Fr experiment.

We suggest the following addition to the manuscript:

ll. 558-559: "***One must also note that the impact of the Fr-Inf configuration on streamflow simulations varies among stations and years depending on the prevailing soil freezing conditions which differ spatially and inter-annually.***"

Ref2-Min18. L. 610: The influence of soil type on the presented results could be discussed in more detail, as it may significantly affect the applicability and transferability of the parameterization. In particular, experimental studies suggest that the role of preferential flow under frozen conditions can vary with soil properties and saturation state. A brief discussion of this variability would strengthen the manuscript.

Following the land cover- and soil texture-based performance and calibration analysis, as presented in responses to Ref2-Maj2 and Ref1-Maj2, we will provide additional interpretations in the discussion. We propose the following modifications to Section 4.2 (Limitations):

ll. 645-655: "***Moreover, the proposed parameterization*** *does not currently rely on land cover characteristics* ***neither on soil texture*** *to modulate infiltration,* ***despite well-documented effects on frozen soil hydraulic behavior*** *(Ala-Aho et al., 2021; Jarvis, 2007; Mohammed et al., 2019; Zhang et al., 2021).* ***Our results suggest that the observed spatial variability in model performance is influenced by land use and, to a larger extent, by soil texture (Fig. 7).*** *Better performance achieved in forested and sandy catchments is attributed to the calibration of the* $\alpha_{no\text{-}imp}$ *parameter to 0.55, which is more representative of these specific landscape characteristics.* ***However, to maintain model parsimony and avoid introducing additional free parameters, the current Fr-Inf configuration does not explicitly account for varying surface characteristics. Despite this simplification, the approach performs reasonably well across the Great Lakes and St. Lawrence domain. In the future, the calibration of*** $\alpha_{no\text{-}imp}$ ***is likely to be reviewed*** *when applying the model* ***across other large domains*** *in Canada and this approach may be revised* ***to include a root-depth or a soil-texture dependency.***"

Regarding the relationship between soil moisture and preferential flow dynamics, please refer to the response to Ref2-Min4 for the proposed modification to the manuscript.

Ref2-Min19. L. 615: The macropore configuration is implemented together with a modification of the runoff generation scheme by disabling the subgrid-scale interflow routine at the surface. This represents an additional structural change to the model. It would be helpful to more clearly separate and discuss the respective impacts of this modification and the macropore parameterization on the results.

The respective impact of disabling subgrid-scale saturation surface runoff generation and favoring vertical soil fluxes under frozen soil conditions was already briefly addressed in Section 3.4. The revised version of the manuscript will further include a clearer separation of both structural changes to the model (see Ref2-Maj1, Ref3-Maj1 and Ref3-Maj2).

egusphere-2026-928 - A non-explicit representation of macropores in the SVS land surface model improves streamflow simulations under frozen soil conditions

Responses to the Anonymous Referee #3

We thank the referee for providing constructive and highly-detailed feedback. Based on this feedback and comments from two other anonymous referees, we propose a thorough reformulation of some parts of the manuscript as well as new additions. We believe these changes successfully streamline the narrative and clarify the work's impact on operational land surface modeling in cold environments. More specifically, we address the main concern raised by the referee (also identified by the two other anonymous reviewers) about the overinterpretation of the results based on macropore-flow processes. In responses to referee's comments, we thoroughly revisited the manuscript as a whole to better frame the narrative around the structural modifications in the context of operational large-scale hydrological modeling. Other major changes include the addition of soil water balance equations in the methods and new analyses of model performance in regard land surface characteristics (land cover and soil texture). Note that "Macropore configuration", "Fr-MP" and "a_{MP}" are denoted "Enhance frozen soil infiltration configuration", "Fr-Inf" and "$a_{no\text{-}imp}$" in this document and in the proposed revised version of the manuscript. Our answers below are in blue, whereas excerpts from the manuscript are in *blue italics* with modifications in ***bold***. In our responses, when referring to other comments from any referee, we use the following terminology: RefX-[Maj or Min]Y where X and Y indicate the referee and comment (major or minor) ID.

Referee comments 3 – egusphere-2026-928

General assessment

This manuscript addresses an important problem for cold-region hydrology and operational forecasting, namely the strong sensitivity of simulated streamflow to the representation of soil freezing. The study clearly shows that activating the soil-freezing configuration in SVS (Fr) strongly degrades hydrological performance, and that the Fr-MP configuration recovers a substantial part of that degradation. In that sense, the work has clear applied and operational value.

However, the manuscript currently overstates the physical meaning of the proposed approach and frames it as a macropore-process study. In my view, the paper does not demonstrate a macropore effect on streamflow, and the catchment-scale references cited in the Introduction do not establish macropores as the explanation for the weak runoff sensitivity to soil frost.

The manuscript would be much more convincing if it abandoned that framing and instead presented the proposed approach as what it is better described as: a conceptual correction of frozen-soil infiltration/runoff partitioning in an operational model, combining a relaxation of infiltration restriction under frozen conditions with a structural adjustment of runoff generation. More generally, I do not think the manuscript should adopt a macropore-based justification in the **Introduction** or present the parameterization in the **Methods** as if it were a process-based macropore representation. If the improvement brought by Fr-MP is to be related to real hydrological processes, this should instead be examined later in the **Discussion**, on the basis of the results and in a much more cautious and multifactorial way, including not only preferential flow, but also soil organic matter, peat and wetland hydrophysical properties, and broader hydropedological heterogeneity across the domain. The **Title** and **Abstract** should also be revised accordingly, so that they reflect this conceptual and operational framing rather than implying a process-based macropore study.

I therefore recommend major revision.

Major comments

Ref3-Maj1. The manuscript is framed too strongly as a macropore-process study

My main concern is that the manuscript is framed too ambitiously as a study of macropore effects on streamflow under frozen-soil conditions, whereas the results mainly show that the Fr-MP configuration pragmatically corrects a large part of the degradation introduced by Fr, without outperforming the noFr reference overall. The authors themselves show that Fr-MP improves Fr at many stations, but that noFr remains the best overall configuration. The Supplementary Material points in the same direction, since the additional hydrographs do not show a general superiority of Fr-MP over noFr.

This framing issue already appears in the Introduction. The manuscript builds a narrative according to which the discrepancy between strong infiltration restriction observed at small scale and the weak runoff sensitivity observed at plot or catchment scale can be explained, at least partly, by soil macropores. This interpretation is plausible as a broad conceptual hypothesis, but it is not directly established by the catchment-scale studies cited at that point. In particular, the two references used there to support the weak runoff sensitivity at catchment scale, Lindström et al. (2002) and Stähli (2017), do not directly demonstrate that this weak hydrological response is explained by macropores. Lindström et al. mainly conclude that no clear effect of soil frost on the timing and magnitude of runoff could be identified in their forested basin, and they emphasize that spring floods usually occurred when the soil was already unfrozen, together with an inverse relationship between frost depth and snow amount. Stähli (2017) likewise concludes that soil frost had no significant effect on winter runoff in the investigated pre-alpine catchments, but explicitly states that the study does not reveal why that effect is weak. He discusses several possible explanations, including shallow frost and strong spatial heterogeneity allowing concentrated infiltration, but without directly demonstrating that macropores are the dominant explanatory mechanism.

At the same time, the broader literature does support the idea that preferential or dual-domain flow may occur in frozen or structured soils at small scale, but it does so in a much more explicit and process-oriented way than the present manuscript. For example, Stähli et al. (1996) introduced a two-domain model approach for preferential flow in frozen soil, and Stähli et al. (1999) further showed, based on lysimeter experiments in two sandy soils, that infiltration into frozen soil depends on the full winter evolution of coupled heat and water dynamics and that key empirical parameters are not constant across soils and seasons. Demand et al. (2019) reported that connected biopores can promote bypass infiltration through shallow frozen layers under specific conditions, while Mohammed et al. (2018) reviewed this broader literature and emphasized the need for more explicit preferential-flow representations in frozen soils, including interacting macropore and matrix domains. More recently, Bauer et al. (2025), currently under review, supported the possibility of rapid bypass infiltration under frozen conditions in controlled experiments with artificial macropore networks, although referee comments on that preprint have also raised substantial concerns regarding over-interpretation of the results and the inferential nature of the proposed mechanisms.

Beyond the frozen-soil literature itself, other studies reinforce the same point. Smettem and Ross (1992) explicitly described a matrix-macropore dichotomy, showing that hydraulic conductivity can change by about an order of magnitude between slight tension and saturation, while localized preferred wetting may still occur even when matrix-based hydraulic properties correctly predict the onset of ponding. Baird (1997) likewise provided field evidence that macropores can be important for water and solute movement in fen peat and concluded that, if macropore flow is common, models based solely on Richards-type matrix flow may be inadequate and a matrix/macropore partitioning approach may be required. At the LSM scale, Vereecken et al. (2019) emphasized that infiltration remains difficult to upscale rigorously, that soil structural effects are still mostly neglected in land surface models, and that representing structure-related effects generally requires much more explicit treatment of soil heterogeneity and preferential pathways.

Taken together, the cited catchment studies do not establish a macropore-based explanation for the weak runoff response, whereas the process-oriented studies that do support preferential flow do so at much smaller scales and with much more explicit representations of matrix-macropore separation, soil structure, or preferential pathways. This reinforces my broader impression that the manuscript over-interprets its physical basis and overstates its "macropore effect" framing.

More fundamentally, the proposed formulation does not align with the process-based approaches commonly used to represent preferential flow: it neither represents organic-soil controls on hydraulic properties explicitly nor uses a dual-domain framework such as dual-porosity or dual-permeability. Instead, it applies a threshold-based removal of ice impedance together with a modification of runoff generation. It may therefore be useful as an operational correction, but it should be framed accordingly rather than as a process-based macropore study.

I therefore recommend that the manuscript be reframed explicitly, starting from the Introduction, as a conceptual correction of frozen-soil flow partitioning in an operational model, rather than as a process study of macropores.

We thank the referee for providing insights regarding the literature in the introduction and for suggesting complementary references. We agree that the manuscript should be reframed away from a macropore-based study. Accordingly, we have revised the title and abstract as follows:

ll. 1-2: "***Enhancing runoff-infiltration partitioning*** *in the SVS land surface model improves streamflow simulations under frozen soil conditions*"

ll. 11-13: "*In this study, we propose* ***a new configuration*** *of the Soil, Vegetation, and Snow (SVS) model used within the operational prediction systems of Environment and Climate Change Canada (ECCC*) ***that enhances frozen soil infiltration by reducing both surface runoff and sub-surface lateral flow.***"

We suggest the following revisions to the introduction:

ll. 38-52: "*Based on multiple-year experiments in* ***a forested*** *catchment smaller than 20 km² located in northern Sweden, Lindström et al. (2002) found that soil freezing has no significant influence on runoff,* ***noting that snowmelt mostly occurred after the ground had already thawed. Similarly, Stähli (2017) found no significant relationship between soil freezing and winter runoff in small Swiss subalpine catchments (< 20 km^2). Although that study did not explicitly identify the underlying mechanisms, the author suggested that shallow frost depths and small-scale spatial variability in topography, vegetation, snow, and soil provide permeable areas where liquid water can infiltrate into frozen ground.***

The concept that frozen soil infiltration occurs through preferential pathways is well established. Based on experiments with two sandy soils during contrasting winters, Stähli et al. (1999) suggested that a preferential flow domain co-exists with matrix flow to promote infiltration when the soil is frozen. Demand et al. (2019) further reported that biopores, which generally originate from earthworms and plant roots (Jarvis, 2007; Six et al., 2004), allow frozen soil infiltration even under high initial water contents by providing connected, air-filled porosity. Similarly, recent experiments by Bauer et al. (2026) indicate that a macropore network within a structured soil matrix enables rapid infiltration under frozen soil conditions, also at high initial water contents. These findings highlight that the dichotomy between preferential and matrix flow, as described by Smettem and Ross (1992) for unfrozen soils, also applies to frozen conditions. Consequently, hydrological models that rely solely on a matrix flow regime may fail to accurately represent frozen ground infiltration (Mohammed et al., 2018, 2021)."

ll. 68-72: "***Several models have successfully accounted for frozen ground infiltration by simulating soil water transport through a dual approach of varying complexity: a high-flow regime (preferential flow) and a low-flow regime (matrix flow) (Agnihotri et al., 2023; Larsbo et al., 2005; Šimůnek et al., 2003; Stähli et al., 1996; Weigert and Schmidt, 2005). However, integrating such approaches into land surface schemes (LSS) is often impractical due to the inherent challenges of upscaling subgrid processes, like infiltration, to a large regional scale (Vereecken et al., 2019).***"

ll. 90-94: "*The main objective of this work is to improve streamflow prediction under frozen soil conditions by **adjusting the existing soil infiltration configuration of** the SVS LSS. The hydrological impact of this proposed configuration is assessed against **the default SVS infiltration configuration, with and without soil freezing enabled**, through a multi-year evaluation over a large domain that encompasses watersheds in the eastern US and Canada.*

Throughout the manuscript, we propose replacing "macropore configuration" with "enhanced frozen soil infiltration configuration". Accordingly, we will rename the experiment "Fr-Inf" (formerly "Fr-MP") and update the index in all variable names and equations from "MP" to "no-imp". All text and figures will be updated to reflect this terminology. Finally, we propose the following revisions to the methods, discussion, and conclusion:

ll. 222-226: "2.1.3 ***Enhanced frozen soil infiltration***

Enhancement of infiltration under frozen soil conditions into SVS relies on a conceptual relaxation of ice impedance based on soil moisture and on a structural modification of surface runoff generation. First*, we implemented **a condition that** verifies if the liquid water content of each soil layer at any given timestep exceeds a no-impedance threshold (W_{no-imp}, unitless):*

$$\boldsymbol{W_{no-imp} = \alpha_{no-imp}} W_{sat,fr} \qquad (\textit{14})$$

ll.232-233: "*In the configuration proposed here, when W exceeds W_{no-imp}, **the vertical hydraulic conductivity of frozen soil ($K_{sat,v,fr}$) corresponds to that of an unfrozen soil ($K_{sat,v}$).***"

ll. 237-244: "***Second, the enhanced frozen soil infiltration configuration includes a modification to the surface runoff generation so it only occurs** when the vertical water input rate exceeds the infiltration rate. **Technically, it implies that satsfc in Eq. 1 is set to 0.** This new feature**, which is activated under frozen and unfrozen soil conditions**, influences runoff-infiltration partitioning when the surface soil layer approaches saturation. This seldom occurs in the absence of ice in the topmost soil layer. **We decided to apply this correction also for unfrozen soil since this approach is fundamentally designed to generate interflow rather than surface runoff. Overall, the enhanced frozen soil infiltration configuration limits the decreasing effect of growing ice on soil saturation and $K_{sat,v,fr}$ and corrects runoff generation formulation. This approach aims to foster infiltration over surface runoff and lateral flow under frozen soil conditions, as illustrated in Fig. 1c.***"

ll. 291-294: "*2.3* ***Calibration of the no-impedance threshold parameter***

***The enhanced frozen soil infiltration configuration comes** with a new parameter (**α_{no-imp}**) indicating the fraction of soil moisture at saturation at which ice impedance is removed. We calibrated **α_{no-imp}** by running open-loop simulations with GEM-Hydro and **using no-impedance thresholds** starting from 0.5 to 0.99 by increments of 0.05 over the GLSL domain."*

ll. 297-298: *"We set the upper boundary for the sensitivity analysis to **α_{no-imp} = 0.99 to replicate restrictive conditions to infiltration similar to that of the default soil freezing configuration (Fr).**"*

ll. 605-607: "*In the current study, we present an updated soil freezing scheme for the SVS land surface model that includes **two major structural changes: the removal of the ice-impedance effect on the vertical soil water fluxes above a calibrated soil moisture threshold and the use of infiltration capacity as the only driver for surface runoff generation.***"

ll. 660-662: "***Overall**, a comprehensive reformulation of the soil water transfer module is necessary **to include preferential flow in SVS and represent more precisely downward gravitational flow that occurs in macropores under frozen soil conditions (Mohammed et al., 2019).***"

ll. 695-696: "*In this work, we proposed **a new configuration of soil freezing** within SVS, the land surface component of the GEM-Hydro hydrometeorological modelling platform**, to increase infiltration.***"

ll. 699-701: "*The improved configuration utilizes unimpeded hydraulic conductivity when liquid water content surpasses a specific threshold relative to available pore space* ***and prevent surface runoff generated from subgrid-scale saturation interflow.***"

Ref3-Maj2. Fr-MP combines multiple structural changes, so the gains cannot be attributed to "macropores"

Fr-MP does not merely alter a "macropore representation." It combines at least two major structural changes: when W > W_MP, the ice-impedance effect is removed so that K_sat,v,fr = K_sat,v, and the surface-runoff rule is also changed so that runoff occurs only when the incoming vertical flux exceeds the local infiltration capacity, effectively suppressing the subgrid saturation-driven runoff mechanism at the surface. The discussion further states that the subgrid-scale interflow routine was disabled at the surface in Fr-MP.

As a result, the improvements cannot be attributed specifically to "macropores." The paper shows that a combined modification of frozen-soil flow partitioning improves hydrographs relative to Fr. That is not the same as demonstrating a macropore effect. This point is made even stronger by the fact that infiltration itself is never shown explicitly as a diagnostic variable. The paper mostly shows changes in surface runoff, lateral flow, and soil drainage.

This concern also extends to the interpretation of the calibrated parameter α_MP. The calibration is performed over 1 September 2015 to 31 August 2018, with the first complete year used as spinup, whereas the main streamflow evaluation covers 2016–2021. Calibration and evaluation therefore overlap, which weakens claims about robustness and transferability of α_MP. In addition, α_MP is selected from a tested range extending from 0.5 to 0.99, and the optimal value retained by the authors (α_MP = 0.55) lies close to the most permissive end of that range. This suggests that the model mainly benefits from a strong relaxation of frozen-soil infiltration restriction, rather than from the identification of a physically robust macropore-activation threshold.

Finally, the statistical comparisons are not always based on identical station subsets. The three-experiment boxplot comparison is reported for 521 stations, whereas the manuscript also states that only 79% of stations are included in the Fr versus Fr-MP comparison. This does not invalidate the reported gains, but it does make the aggregate comparisons harder to interpret and weakens any strong claim that the calibrated α_MP has been shown to be spatially robust across the full domain.

We agree that the original manuscript did not clearly separate the two modifications we made to the model: (1) the removal of subgrid-scale saturation for runoff generation, and (2) the relaxation of the ice-impedance effect on soil water fluxes. We propose to clarify this distinction in the revised Methods and Discussion sections (see Ref3-Maj1). Furthermore, as noted in our response to Referee #2, we conducted a new stepwise analysis applying each modification individually. The results show that each change slightly improves model's performance compared to the default Fr configuration but that only the combination of both changes considerably improves the streamflow simulations under frozen soil conditions. Please refer to our response to Ref2-Maj1 for full details on this analysis and the suggested modifications to the manuscript.

Regarding the calibration of α_{MP} (renamed $\alpha_{no\text{-}imp}$ in the revised manuscript), we acknowledge that using a sub-period of the 2016-2021 evaluation period may limit the transferability of the 0.55 value. However, this was necessitated by computational constraints. A single experiment over the GLSL domain takes roughly 5 days to compute. Because we chose to run 11 calibration experiments, using the first three years (including a spin-up year) was the most practical compromise between robustness and efficiency. Additionally, as the reviewer noted, Section 3.2 demonstrates better performance with configurations that are less restrictive to infiltration. We also conducted a detailed analysis on the calibration of $\alpha_{no\text{-}imp}$ in relation to land use and soil texture (see Ref1-Maj2). We believe that this analysis with the corresponding changes and our revised framing, which focuses on structural model changes rather than physically based "macropore" flow, strengthens the manuscript.

Finally, while the size of the station subsets varies across Figures 3 to 6, the comparative statistics remain valid because all experiments within a given comparison are evaluated against the exact same subset of stations. The same concern was raised by Referee #2. Please refer to Ref2-Min13 for a more detailed response and suggested modifications to the manuscript.

Ref3-Maj3. The physical interpretation is limited by the soil formulation and the spatial heterogeneity of the domain

The physical interpretation of the results remains limited because the soil formulation used here is still essentially mineral-texture based, with hydraulic and freezing properties derived from variables such as X_sand, X_clay, W_sat, ψ_sat, and b. There is no explicit representation of organic soil horizons or organic-soil hydraulic properties, even though the study domain includes many wetlands and other cold-region environments where soil organic matter can strongly affect pore structure, storage, conductivity, and thermal behavior. Recent land-surface modeling work has shown that soil organic matter cannot be treated as a minor correction to mineral soil properties, and that physically consistent representation of organic matter is required to capture its effects on hydraulic and thermal processes. Decharme (2025) makes this point explicitly and shows that conventional LSM parameterizations based on mineral-soil assumptions or simplified SOC-based corrections can be physically inconsistent.

This limitation is particularly important in peat-rich or wetland environments. Liu and Lennartz (2019) show that hydraulic properties of peat soils depend strongly on pore structure, peat decomposition, and botanical composition, and that macroporosity and K_s follow different relationships with bulk density above and below a threshold near 0.2 g cm^{-3}. Lennartz and Liu (2019) further emphasize that pristine peat is dominated by very high porosity and abundant macropores, whereas degradation reduces porosity and conductivity and may even turn highly degraded peat into a hydraulic barrier. Liu et al. (2020) then show that drainage progressively shifts peat pore structure over decadal to centennial timescales, reducing macroporosity, increasing small-pore volume, and decreasing K_s by roughly two orders of magnitude, with different trajectories under forest and agricultural land use. They also note that the different BD–K_s relationships cannot be explained by macroporosity alone, because pore connectivity and pore geometry also matter.

More broadly, recent work also shows that the spatial variability of K_s cannot be interpreted satisfactorily from soil texture and bulk density alone. Gupta et al. (2021) explicitly argue that texture-based pedotransfer approaches are limited because they neglect soil structure and pedogenic information, especially in vegetated and structured soils. Their results show that terrain, climate, and vegetation covariates affect spatial patterns of K_s, and that maps based on these environmental covariates better capture soil-formation-related spatial structure than maps based only on basic soil properties. At the same time, their spatial cross-validation performance remains modest, which underlines how difficult it is to interpret or predict K_s patterns robustly at large spatial scales.

In this context, I find the process-level interpretation in terms of "macropore effects" too broad. The model performs better in forested and more natural environments, and worse in several agricultural, urban, or hydrologically complex areas, but the current discussion does not convincingly disentangle what is due to frozen-soil physics, what is due to the mineral-soil formulation itself, and what may instead reflect wetland extent, peat degradation, drainage history, routing limitations, or other forms of hydropedological heterogeneity. This is especially important because the manuscript already acknowledges missing wetland representation and also includes non-explicit representations of tile drainage and ploughing, which further complicates any strict process interpretation in terms of macropores alone.

I therefore think the manuscript should be much more cautious in its spatial interpretation of the results. At present, the domain heterogeneity is too strong, and the soil formulation too simplified, to support a broad physical interpretation of the observed regional patterns in terms of macropores or preferential flow.

We thank the referee for the detailed explanation of how organic matter and peat influence macropore prevalence and soil hydraulic behavior, along with supporting literature. We acknowledge that organic soils play a major role in surface hydraulic properties. Unfortunately, the current version of SVS does not explicitly represent organic matter, though efforts to integrate soil organic layers into version 2.0 (SVS2; Vionnet et al. 2025) are currently ongoing. We suggest adding the following revisions to the discussion:

ll. 656-659: "***Additionally, SVS does not include a representation of soil organic matter despite known relationships between peat composition and degradation, pore structure and hydraulic properties of the vadose zone (Decharme, 2025; Liu and Lennartz, 2019). Indeed, governing pedotransfer equations in SVS neglects effects of soil structure, land cover and climate, among other factors.***"

ll. 663: "[…], ***and more broadly to improve the robustness and transferability of the model.***"

ll. 670-671: "***Note that efforts are also ongoing to include organic soil into SVS2.***"

Furthermore, because Reviewer #2 raised related questions regarding the physical interpretation of the results, we performed a more thorough analysis of the model performance in relation to the forest and crop fractions, and sand and clay fractions, across the domain. Consequently, we have revisited Section 3.3 and bonified Figure 7. Accordingly, we further expanded the first paragraph of Section 4.2 about limitations. Please refer to our response to Ref2-Maj2 for full details on this new analysis and the corresponding changes made to the manuscript.

Ref3-Maj4. The failure of Fr is not diagnosed clearly enough

A central problem is that the paper still does not make it possible to diagnose clearly why the Fr configuration fails so strongly. To be fair, the manuscript does provide the main elements of the soil-freezing formulation itself, including the residual unfrozen water parameterization, the reduction of available pore space by ice, the impedance factor applied to hydraulic conductivity, and the threshold-based rule used in Fr-MP to suppress that impedance when W > W_MP . It also states that, in Fr-MP, surface runoff is only generated when the incoming vertical water flux exceeds the infiltration rate, thereby suppressing the subgrid saturation-excess runoff mechanism at the surface.

However, this remains insufficient for a clear mechanistic diagnosis of the Fr behavior, because the manuscript does not provide the full governing equations for the hydrological flux partitioning on which the interpretation actually relies. Surface runoff, lateral flow/interflow, and soil drainage are discussed extensively in the results and discussion, but their full formulations are not given explicitly in the manuscript itself. Instead, the reader is left with a conceptual diagram and a verbal description, while important structural details are only mentioned qualitatively, such as the disabling of the subgrid-scale interflow routine at the surface in Fr-MP and the strong near-surface anisotropy that promotes lateral flow.

As a result, it remains difficult to determine which component is primarily responsible for the severe degradation in Fr: the ice-impedance term itself, the runoff-generation scheme near the surface, the lateral-flow formulation, the anisotropy assumptions, or the interaction among these components. This matters because the paper interprets the improvements in terms of changes in surface runoff, lateral flow, and soil drainage, yet the reader cannot fully reconstruct how these fluxes are partitioned in each configuration. The fact that Fr-MP improves Fr while still generally underperforming noFr at the domain scale further reinforces the need for a more transparent diagnosis.

To make this diagnosis possible, I strongly recommend that the authors provide the missing governing equations, at least in the Supplementary Material. In particular, the manuscript should include the explicit formulations used for surface runoff generation, lateral runoff/interflow, and gravitational drainage, together with a compact summary of how these formulations are altered by Fr and by Fr-MP. That would make it much easier to understand which structural component of the frozen-soil configuration is actually responsible for the strong degradation and which component is effectively corrected in Fr-MP.

We thank the reviewer for raising this up. We agree that providing these equations in the manuscript would help the reader to interpret more easily our results. We suggest adding in section 2.1.1 the governing equations for surface runoff, lateral flow from underlying-layer saturation and for the horizontal hydraulic conductivity. Base flow, as for lateral flow in part, is computed using the soil interflow formulation from Soulis et al (2011), which relies on the calculation of bulk field capacities and on 1D Richards Equation. This approach is used as an alternative to look-up table or pedotransfer functions. In this approach, interflow occurs when soil moisture is between field capacity and saturation. Presenting explicitly this formulation in the manuscript would make it uselessly bulky. Instead, we suggest presenting the equations for the field capacity of each soil layer with additional in-text details. We believe that this makes the manuscript clearer and the results of each configuration easier to diagnose. We suggest the following modification to Section 2.1.1:

l. 97: "*2.1.1* ***Soil water balance in SVS***"

ll. 110-140: "*Precipitation and snowmelt that reach the ground surface can exit the grid cell as surface runoff or infiltrate into the upper soil layer. Surface runoff* ***(ROF; in m s^{-1})*** *occurs when the* ***vertical influx*** *rate* ***(VI; in m s^{-1})*** *exceeds the vertical hydraulic conductivity of the upper soil layer* ***($K_{sat,v,1}$; in m s^{-1})****, or when a portion of this layer becomes saturated:*

$$ROF = \left(1 - sat_{sfc}\right) \max\left(VI - K_{sat,v,1}, 0.0\right) + sat_{sfc} VI \quad (1)$$

where sat_{sfc} is the saturated fraction of the surface which exceeds 0 only when the soil moisture (W; unitless) of the surface layer exceeds saturation. This subgrid-scale variable *is dependent on the pore size distribution and bulk saturation of the first layer (Alavi et al., 2016; Soulis et al., 2011). Water that infiltrates into the soil column can be conveyed to the layers below using a finite difference solution of the* ***one-dimensional*** *Richards equation, or it can exit the column as lateral flow* ***based on the interflow calculation described in Soulis et al. (2011). This approach relies on the exceedance of the field capacity (W_{fc}; unitless) of any i soil layer:***

$$W_{fc,i} = \frac{W_{sat,i}}{b-1}\left(-\frac{\psi_{sat,i}\, b}{2D_d\, s}\right)^{1/b}\left((3b+2)^{(b-1)/b} - (2b+2)^{(b-1)/b}\right) \quad (2)$$

where the saturated water content, W_{sat} (unitless), the saturated soil matric potential, ψ_{sat} (m), and the slope of the water retention curve from Clapp and Hornberger (1978), b (unitless), are all soil texture-dependent parameters:

$$W_{sat} = -0.00126 X_{sand} + 0.489 \quad (3)$$

$$\psi_{sat} = \frac{10^{(-0.0131 X_{sand} + 1.88)}}{100} \quad (4)$$

$$b = 0.137 X_{clay} + 3.501 \quad (5)$$

with Xsand and Xclay being the sand and clay percentages (between 0 and 100) of any soil layer, respectively. ***In Eq. 2, $2D_d s$ corresponds to the tile slope-to-length ratio with D_d and s being the drainage density (in m m^{-2}) and the slope (in m m^{-1}) of the tile, respectively. The slope-to-length ratio is then applied to the horizontal hydraulic conductivity ($K_{sat,h}$; in m s^{-1}) to modulate the interflow. Additionally, $K_{sat,h}$ depends on $K_{sat,v}$ and decays with depth, favoring lateral flow for layers near the surface:***

$$K_{sat,h} = aK_{sat,v}\, exp\left[c\frac{D-d}{D}\right] \quad (6)$$

where D (m) is the depth of the whole soil column while d (m) is the depth of the soil layer, and a and c are anisotropic constants (a = 10 and c = 5)*. Lateral flow also occurs when there is insufficient space in the underlying layer to receive* ***the excess water of a given i soil layer:***

$$LAT = \max\left((W_i - W_{sat,i})\Delta d_i - (W_{i+1} - W_{sat,i+1})\Delta d_{i+1}, 0.0\right) \quad (7)$$

where LAT is the lateral flow (m) and Δd (m) corresponds the thickness of each layer. LAT is cumulated over each calculation timestep and converted in m s^{-1}. By configuration, this mechanism does not generate lateral flow in the last soil layer. Instead, *water that reaches the* ***deepest*** *soil layer* ***(denoted N)*** *leaves the soil column as drainage when the field capacity of this layer is exceeded****. The field capacity of the deepest soil layer is computed using Eq. 2 adjusted to account for total soil depth (Soulis et al., 2011):***

$$W_{fc,N} = \frac{W_{sat,N}}{b-1}\left(\frac{\psi_{sat,N}\, b}{D}\right)^{1/b}\left((3b+2)^{(b-1)/b} - (2b+2)^{(b-1)/b}\right). \qquad (8)$$”

As suggested by the referee, we also suggest including a brief summary of how the Fr and Fr-Inf configurations influence the formulation of soil water fluxes:

ll. 218-221: “*Overall,* ***growing soil ice has to major impact on the soil water balance. First, it reduces the saturated water content which makes the saturation of the soil layers more likely (Eq. 7) and the exceedance of field capacity easier (Eq. 2) for lateral flow generation. Second, it reduces the vertical hydraulic conductivity, favoring even more soil saturation and surface runoff (Eq. 1). These effects are shown*** *in conceptual Fig. 1b.*”

ll. 240-244: “***We decided to apply this correction also for unfrozen soil since this approach is fundamentally designed to generate interflow rather than surface runoff. Overall, the enhanced frozen soil infiltration configuration limits the decreasing effect of growing ice on soil saturation and $K_{sat,v,fr}$ and corrects runoff generation formulation. This approach aims to foster infiltration under frozen soil conditions, as illustrated in Fig. 1c.***”

Minor comments

I do not have essential additional minor comments beyond those already raised by the other reviewers. However, I strongly suggest:

Ref3-Min1. not referring to this parameterization as a “macropore” representation, as it is more accurately described as a conceptual correction of frozen-soil infiltration/runoff partitioning in an operational model;

This will be done (see Ref3-Maj1)

Ref3-Min2. adding the key equations of the model, especially those governing soil freezing, runoff generation, lateral flow, and drainage;

This will be done (see Ref3-Maj4)

Ref3-Min3. clarifying whether the modified runoff-generation rule affects the model under unfrozen conditions;

This concern was also raised by Referee #2. This case seldom occurs but indeed, the modified formulation for runoff generation may impact runoff/infiltration partitioning under unfrozen conditions if the surface layer becomes saturated. Please, refer to the response to Ref2-Min16 for the suggested changes to the manuscript

Ref3-Min4. revising the abstract and conclusion so they more clearly reflect that Fr-MP mainly improves Fr, without outperforming noFr overall.

We suggest the following modifications to the abstract and to the conclusion:

ll. 15-17: “***Fr-Inf significantly*** *improves the Kling-Gupta Efficiency (KGE) compared to* ***the default soil freezing*** *configuration* ***(ΔKGE = 0.28) but slightly underperforms the configuration of SVS without frozen soil (ΔKGE = – 0.07).***”

l. 699: “*[…]*, ***without outperforming a configuration without soil freezing overall.”***

Cited References:

Agnihotri, J., Behrangi, A., Tavakoly, A., Geheran, M., Farmani, M. A., and Niu, G.: Higher Frozen Soil Permeability Represented in a Hydrological Model Improves Spring Streamflow Prediction From River Basin to Continental Scales, Water Resour. Res., 59, e2022WR033075, https://doi.org/10.1029/2022WR033075, 2023.

Bauer, J., Müller, S., Heinze, T., Khanahmadi Bafghi, H., and Baselt, I.: Thermohydraulic experiments on water infiltration into frozen slopes: the role of macropores and initial water content, The Cryosphere, 20, 3483–3509, https://doi.org/10.5194/tc-20-3483-2026, 2026.

Demand, D., Selker, J. S., and Weiler, M.: Influences of Macropores on Infiltration into Seasonally Frozen Soil, Vadose Zone J., 18, 1–14, https://doi.org/10.2136/vzj2018.08.0147, 2019.

Hardie, M. A., Cotching, W. E., Doyle, R. B., Holz, G., Lisson, S., and Mattern, K.: Effect of antecedent soil moisture on preferential flow in a texture-contrast soil, Journal of Hydrology, 398, 191–201, https://doi.org/10.1016/j.jhydrol.2010.12.008, 2011.

Kurylyk, B. L. and Watanabe, K.: The mathematical representation of freezing and thawing processes in variably-saturated, non-deformable soils, Adv. Water Resour., 60, 160–177, https://doi.org/10.1016/j.advwatres.2013.07.016, 2013.

Merdun, H., Meral, R., and Riza Demirkiran, A.: Effect of the initial soil moisture content on the spatial distribution of the water retention, Eurasian Soil Sc., 41, 1098–1106, https://doi.org/10.1134/S1064229308100128, 2008.

Mohammed, A. A., Pavlovskii, I., Cey, E. E., and Hayashi, M.: Effects of preferential flow on snowmelt partitioning and groundwater recharge in frozen soils, Hydrol. Earth Syst. Sci., 23, 5017–5031, https://doi.org/10.5194/hess-23-5017-2019, 2019.

Niu, G.-Y. and Yang, Z.-L.: Effects of Frozen Soil on Snowmelt Runoff and Soil Water Storage at a Continental Scale, J. Hydrometeorol., 7, 937–952, https://doi.org/10.1175/JHM538.1, 2006.

Vionnet, V., Leroux, N. R., Fortin, V., Abrahamowicz, M., Woolley, G., Mazzotti, G., Gaillard, M., Lafaysse, M., Royer, A., Domine, F., Gauthier, N., Rutter, N., Derksen, C., and Bélair, S.: Enhancing simulations of snowpack properties in land surface models with the Soil, Vegetation and Snow scheme v2.0 (SVS2), Geosci. Model Dev., 18, 9119–9147, https://doi.org/10.5194/gmd-18-9119-2025, 2025.