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.

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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

1. 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 km^2 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 ($W_{no\text{-}imp}$, unitless):*

$$W_{no-imp} = \alpha_{no-imp} W_{sat,fr} \tag{14}$$

ll.232-233: "*In the configuration proposed here, when W exceeds $W_{no\text{-}imp}$,* ***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 ($\alpha_{no\text{-}imp}$) indicating the fraction of soil moisture at saturation at which ice impedance is removed. We calibrated $\alpha_{no\text{-}imp}$ 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* ***$\alpha_{no\text{-}imp} = 0.99$ 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.***”

2. 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.

3. 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.

4. 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:*

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

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:***

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

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:

$$W_{sat} = -0.00126 X_{sand} + 0.489 \tag{3}$$

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

$$b = 0.137 X_{clay} + 3.501 \tag{5}$$

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:***

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

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:***

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

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:

1. 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)

2. 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)

3. 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

4. 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:

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