egusphere-2026-928 - Enhancing runoff-infiltration partitioning in the SVS land surface model improves streamflow simulations under frozen soil conditions

Responses to the Anonymous Referee #3 – round 2

We wish to thank the referee for commenting on the revised version of the manuscript and for providing additional insightful suggestions regarding the framing of the study. Based on these suggestions, we propose reframing the narrative of the manuscript so mentions regarding macropore-based physical interpretation only appear in the Discussion. We hope that these changes, if accepted, will address the referee's concerns regarding the framing of the manuscript. Our responses below are in blue, while excerpts from the manuscript are in blue *italics* with modifications in ***bold***.

Report from Anonymous Referee #3 – egusphere-2026-928

I thank the authors for their detailed response and for the substantial revisions made to the manuscript. The revised version is much improved. In particular, I appreciate the revised title, the replacement of the former "macropore configuration" terminology by "enhanced frozen soil infiltration", the clearer description of the two structural modifications introduced in Fr-Inf, the additional sensitivity experiments separating these modifications, the new analyses based on land cover and soil texture, and the addition of the main governing equations for runoff and soil water flux partitioning. These changes address most of my previous major concerns.

I have one remaining comment concerning the overall framing of the physical interpretation. Although the manuscript has been substantially reframed, I think that the process-based justification related to preferential pathways, biopores, and macropore networks is still introduced too early and too strongly.

In the Abstract, lines ~8–12, the sentence stating that "the presence of large pores in structured soils maintains rapid water percolation even in frozen conditions" still gives a process-based justification before the proposed configuration has been introduced. I suggest either removing this sentence or reformulating it more generally, for example by stating that frozen-soil infiltration remains difficult to represent because it depends on subgrid soil structure and hydropedological heterogeneity.

We suggest replacing sentences at ll. 7-9 in abstract with the following:

ll. 7-8: "***In land surface models, frozen soil infiltration is difficult to represent because soil structure and hydropedological processes vary at scales finer than the model grid.***"

Similarly, in the Introduction, lines ~44–52, the paragraph on preferential pathways, biopores, and macropore networks still reads as if these processes provide the main a priori physical basis for the proposed parameterization. In my view, this material would be better placed in the Discussion, where it could be presented as one possible physical interpretation of why relaxing frozen-soil infiltration restrictions improves streamflow simulations. The Introduction should instead remain focused on the modelling problem: in SVS, the default soil-freezing configuration strongly restricts infiltration, this leads to unrealistic runoff responses, and the objective of the study is to test a pragmatic correction of runoff–infiltration partitioning in an operational model.

Thank you for this remark. We suggest replacing the third paragraph of the introduction with the following sentences at l. 42. Some parts of this paragraph will be moved to the Discussion (see response below).

ll. 42-46: "***Studies dedicated to understanding these processes suggested that small-scale heterogeneity in soil structure along with a high initial soil moisture promote rapid infiltration into frozen soil (Stähli et al., 1999; Demand et al., 2019; Bauer et al., 2026). However, most hydrological models neglect subgrid hydropedological processes and solely rely on a single-domain matrix flow regime to represent frozen ground infiltration, which often leads to inaccurate simulations (Mohammed et al., 2018, 2021).***"

The same applies, although more locally, to the Methods section. In lines ~223–235, the description of Fr-Inf is now much clearer, but the justification of the no-impedance threshold using non-equilibrium flow and preferential-flow studies could still be toned down. Since Fr-Inf remains a conceptual relaxation of ice impedance combined with a structural modification of surface runoff generation, I would suggest keeping the Methods focused on what is actually implemented and calibrated, and leaving the possible process interpretation to the Discussion.

We agree with the referee. This will be removed in the revised manuscript.

The Discussion, especially lines ~640–660, is in my opinion the right place to discuss the possible physical processes behind the model improvement. This discussion could explicitly mention that several mechanisms may facilitate infiltration into frozen soils, including preferential flow through biologically generated macropores such as earthworm burrows and root channels, but also pore-structure effects associated with soil organic matter, peat, wetlands, and broader hydropedological heterogeneity. This would be consistent with the authors' current discussion of soil organic matter and peat hydraulic properties, while avoiding the impression that macropores are the starting assumption of the study.

We thank the referee for this suggestion. Here is the proposed modification to the Discussion:

ll. 634-640: "***Empirical studies have suggested that water may freeze first in small pores, leaving connected, air-filled larger pores available for rapid infiltration (Demand et al., 2019; Mohammed et al., 2019; Bauer et al., 2026). This is favored by a large soil pore-size distribution at small scale driven by biological factors, such as earthworm burrows and root channels (Jarvis, 2007; Six et al., 2004), and by pore-structure effects associated with high textural heterogeneity (Beven and Germann, 2013)***. *Nevertheless,* ***the approach introduced here*** *remains a structural modification to the SVS soil freezing scheme which does not explicitly account for* ***these*** *subgrid physical processes that drive frozen ground infiltration.*"

Overall, I consider that the manuscript has been substantially improved and that the authors have addressed most of the major concerns raised during the first review round. I would support publication after this remaining framing issue is addressed and after minor language corrections.