Reviewer 2 – General Assessment

The authors present an excellent experimental study on the effects of macropores on the partitioning of rainfall between infiltration, runoff and drainage in frozen hillslopes. The hillslope-scale experiments are a significant advancement of past 1D soil column experiments, and allows investigation of more realistic multi-dimensional hillslope behavior. The results are not nearly as novel as the authors claim, almost all of the findings presented here have been shown in other studies (see below). However the results still provide valuable insight into the importance of antecedent soil moisture on preferential flow, as well as enhanced infiltration and bypass flow in frozen soils due to preferential flow, refreezing of infiltrated water in macropores and its effect on the temporal evolution of runoff generation in drainage in frozen soils.

A novel aspect is the direct observational evidence of freezing of water in macropores initiating from the pore-walls, corroborating the findings of Watanabe and Kugasaki (2017). We need more than one study to confirm this behavior, and this study does a great job of showing this in hillslopes, also corroborating the conceptual model put forward in Mohammed et al. (2018). The other major advancement in this study is providing a well-controlled dataset that will be extremely valuable for testing emerging dual-permeability models of water flow in frozen soils. I very much look forward to future modeling work with this dataset. This is a solid contribution and should be published. However, the authors need to walk back some of the strong language used in the manuscript, as this experimental work also suffers from many of the limitations of field studies, in that many of the preferential flow processes discussed are inferred rather than directly observed.

Preferential flow in frozen soil is still a pretty niche field compared to more mature fields in cold regions hydrology, but interest is growing due to increased consensus that it has significant control on water partitioning in frozen soils. There has been previous work that has shown many, if not all, of the findings in this work, yet the authors have not acknowledged many of these important studies. They appropriately mention the seminal experimental work of Watanabe and Kugisaki (2017) and Pittman et al. (2020), yet Holten et al. (2019) and Grant et al. (2019) have also shown many of these infiltration, preferential flow and refreezing dynamics. The authors need to acknowledge these works as their work builds upon these previous studies. Similarly, there have been pioneering field and modeling studies on hillslopes that have also shown almost all the findings of this work that the authors don't acknowledge either, including Mohammed at al. (2019), Rey et al. (2021), Mohammed et al. (2021b), Hyman-Rabeler and Loheide (2023), and Sanchez-Rodriguez et al. (2025). They refer to a review paper like Walvoord and Kurylyk (2016) that has almost nothing to do with the work presented here and yet don't mention Ireson et al. (2013). One might think that the authors have purposely left out references to these studies in this manuscript to seemingly enhance the perceived novelty of the work presented. The authors need to give credit where credit is due.

I believe with minor revisions, this will be an excellent contribution to the field of cold regions hydrology.

Response:

We thank Reviewer 2 for the positive and encouraging assessment of our work. We particularly appreciate the recognition of the slope-scale experimental design and the value of the dataset for testing dual-permeability and freeze-thaw modelling approaches.

We acknowledge the reviewer's concern regarding the strength of certain formulations and the framing of novelty. In the revised manuscript, we have moderated causal language, clarified the inferential nature of preferential flow interpretations, and strengthened the positioning of our study within the existing body of literature. We agree that some statements in the original version were formulated too strongly and that similar mechanisms have been observed and discussed in previous studies.

We also appreciate the reviewer's suggestions regarding additional literature. We recognise the importance of thoroughly situating our work within the established research landscape on preferential flow in frozen soils. In response, we have carefully incorporated the recommended references and expanded both the Introduction and Discussion to more explicitly acknowledge prior contributions. The inclusion of these studies has strengthened the contextualisation of our results and clarified how our work builds upon existing knowledge.

We emphasise that the novelty of our contribution does not lie in identifying preferential flow processes per se, which have indeed been documented previously, but in investigating these processes under controlled slope-scale conditions using a reproducible three-dimensional macropore network and dense spatial instrumentation. This intermediate hillslope-scale experimental configuration allows us to observe process interactions that bridge traditional one-dimensional column experiments and field-scale studies.

Minor Comments

RC2: L43: Should cite Larsbo et al. (2019) and Mohammed et al. (2021a) here, as these two were the first to include differences in freezing between macropores and the soil matrix in numerical models, using the conceptual model put forward in Mohammed et al. (2018).

AC: We have incorporated the suggested references into the revised manuscript and integrated them more explicitly into the Introduction to better position our study within the existing literature.

RC2: L52: This sentence should reference works like Mohammed et al. (2021b) and Sanchez-Rodriguez et al., (2025).

AC: We added the citations to this sentence.

RC2: L67-68: Should also cite Mohammed et al. (2019) and Sanchez-Rodriguez et al., (2025) here as well.

AC: We agree and added the suggested literature to the sentence.

RC2: L100: Please report on the stability of the temperature range of the compressor used to main temperatures, i.e. show that the temperature control is actually stable during frozen conditions.

AC: We agree that the original wording may have implied that the climate chamber maintained a perfectly stable air temperature at the nominal set point, which is not the case. The cooling unit operates in periodic compressor cycles, resulting in controlled temperature oscillations around a target value. In the revised manuscript, we have clarified the description of the temperature regulation and adjusted the wording to avoid any misleading implication of constant air temperature. We now explicitly state that this is the possible operating range of the control unit. In addition, we have added a representative temperature record of the compressor cycling behaviour to the Appendix (E) for transparency. We thank Reviewer 2 for highlighting this point, which has helped us improve the clarity and accuracy of the methodological description.

*RC2: L102-104: What was the insulation material surrounding the soil box? The authors state that insulation "ensured that freezing initiates **exclusively** at the air-soil interface, akin to natural conditions". It is not sufficient to just state this, you have to show it. What you should have done is place temperature sensors at the edges of the box and compare the thermal profiles at the center and edges of the box. You should use the thermal profiles of the probes S1-S5 to show that ambient temperature interference was actually minimized, and lateral thermal gradients are at least an order of magnitude smaller than vertical gradients. Nagare et al. (2012) and Mohammed et al. (2014) showed that passive insulation like that used here will always fail at some point, so you need to show that the set-up actually mimicked top-down thaw, as the thawing period is more important that the freezing period for these experiments. While I don't necessarily think this is a major issue for this type of experiment, statements like this made with such certainty need to be backed up by data. Your figures 5-7 clearly show lateral thermal gradients with those curved lines showing frost depths, especially FN10.*

AC: We agree that a clearer quantification of lateral versus vertical temperature gradients strengthens the methodological transparency of the experimental setup. In the revised manuscript, we have moderated the original wording in the Methods section to avoid implying strictly one-dimensional freezing conditions. We now explicitly acknowledge that lateral thermal influences are present. In addition, we have included a ~~supplementary figure illustrating~~chapter in the appendices (F) showing the temporal evolution of ~~air temperature and~~ the relative magnitude of lateral and vertical temperature gradients ~~during the freezing phase~~.
We agree that lateral influences are detectable even during freezing, and we now state this explicitly in the manuscript. Future experimental configurations may include additional boundary-focused temperature measurements to further refine the assessment of lateral heat exchange.

RC2: L246-247: Again, here you mention The temperature and VWC distributions were found to be largely uniform across the soil body, with only minor spatial variations and weak boundary influences near the walls and bottom You're asking the reader to take your word for this. See my suggestion above about comparing lateral versus vertical thermal gradients. This could be in the appendices.
Figure 3: Somewhat related to the previous comments above. I understand why the authors have averaged the VWC and temperature probe readings, I suspect that there are plans in the pipeline to model this experiment with dual-permeability model similar to Khanahmadi et al. (2026) but modified for freeze-thaw. However, it would be nice to see the profiles from the individual probe profiles. This, again, could go into the appendices.

AC: We agree that the original wording was too qualitative and did not sufficiently quantify the reported spatial variability. In the revised manuscript, we have added explicit quantitative measures of lateral and vertical temperature ~~variability~~gradients in ~~Sect. 3.1~~the appendices (see ~~revised~~ also answer to L. ~~246–250). In particular, we now report the magnitude of lateral temperature differences at comparable depths~~102-104) and ~~compare them to vertical gradients~~ changed the original wording. We also added profiles from individual probes for two representative experiments in the appendices (G). We did not include all profiles, as including all plots in the manuscript would substantially increase its length. The experiments FN15 and FM15 were selected because they clearly illustrate the main experimental patterns discussed in the manuscript. These experiments were not chosen because they exhibit the strongest or most variable responses, but because they provide the most influential results.

~~In addition, we have included a supplementary figure in appendices illustrating the temporal evolution of chamber air temperature as well as the relative magnitude of lateral and vertical gradients during the freezing phase. We also added a plot with profiles from individual probe profiles within the appendices.~~

RC2: L263: You state that 'Concurrent VWC decreases in the upper 10-20 cm (> 0.5 %) indicate early redistribution of unfrozen water'… so where did this water go? All depths show either decreases or no change. Cryo-suction redistributes water to the freezing front, so was water redistributed to the shallow soil where no sensors were present? If you don't have measurements to confirm, another reason could be that there was vapor loss as well, so you can't say for sure that this loss was due to redistribution... especially in the coarse-grained soil used here.

L265-266: Downward redistribution during freezing goes against our current understanding of the effects of freezing on water migration (i.e. cryo-suction). This does not make sense, especially at drier conditions in coarse grained soil as the authors have used here.

AC: We agree that classical cryo-suction mechanisms promote upward water migration toward the freezing front, particularly in fine-grained soils. The slight decreases in VWC observed in the upper 10-20 cm occur during the early cooling phase, prior to the onset of significant ice formation, and are therefore not interpreted as cryo-suction-driven redistribution. In the high initial water content experiments, small water losses into the drainage layer were detected before freezing commenced. These early changes are most plausibly explained by gravitational drainage during cooling and minor structural settling of the soil matrix. Given the coarse texture and low capillarity of the experimental soil, substantial cryo-suction effects are not expected. Temperature-dependent sensor effects associated with soil cooling cannot be entirely excluded (even though the applied three- and four-phase dielectric mixing approach explicitly incorporates temperature effects). However, the magnitude of the observed variations is small and consistently detected across experiments. Regarding the reviewer's question "where did the water go?", we note that during the early cooling phase, part of the observed VWC decrease in the upper layer corresponds to measurable drainage losses (in the high VWC experiments).

At later stages during freezing, the reduction in measured VWC primarily reflects phase change rather than mass redistribution. As described in the methods section, the sensors measure liquid volumetric water content. Consequently, a decrease in VWC during freezing represents conversion of liquid water to ice rather than physical water loss from the system.

To avoid confusion, we have revised Sect. 3.1 to clarify (i) the distinction between early gravitational redistribution prior to freezing and (ii) later reductions in liquid VWC due to phase change. We have also explicitly reiterated that the reported VWC values represent liquid water content. Furthermore, we have moderated the wording describing early VWC decreases in the upper layers during the initial cooling phase to reflect the associated uncertainty in their precise physical origin.

RC2: L279: Again, do you have data to support this statement?

AC: We revised the corresponding statement, as we agree that it was formulated too definitively.

RC2: L287-289: These cumulative plots are great, but it would be nice to see some temporal fluxes in addition to the cumulative plots.

AC: We agree that time-resolved flux representations provide valuable additional insight into the transient dynamics of the system. Especially at those experiments, where fluxes are changing dynamically. In the revised manuscript, we have therefore added a dedicated section in the appendices (H) presenting temporal inflow, drainage, and surface runoff rates of FM15 and FM16 in addition to the cumulative fluxes shown in the main text.

RC2: L317: 'as evidence of' should be replaced by 'suggests'. The similarity in infiltration behavior, and differences in drainage volumes help support this inference of bypass flow as well, since it suggests that more water is stored in the matrix in the FN12 versus FM13.

AC: We have revised the original sentence with more cautious wording ("is consistent with") to better reflect the inferential nature of the interpretation. We also clarified the argument by explicitly linking the earlier drainage response in FM13 to the combination of similar infiltration behaviour and differences in cumulative drainage volumes, thank you for this suggestion (see also response to Reviewer 1, Major Comment 1).

RC2: L320: Not sure your data shows any differences or 'acceleration' in infiltration rates… it does suggest bypass flow though.

AC: We agree that the term "acceleration" was imprecise and potentially misleading in this context. Our intention was to describe that the onset of drainage and the effective transmission of infiltrating water through the system occurred earlier and more rapidly in the macropore experiments compared to the non-macropore case. In the revised manuscript, we have avoided the term "acceleration" to avoid implying a quantified change in infiltration rate beyond what is supported by the data.

RC2: L335-336: 'which enabled water to bypass frozen regions and resulted in earlier and more pronounced drainage'…I'm wondering if you could also show the VWC profiles at the onset of drainage in FM15 and FM16. My reasoning is that if the probes show no changes prior to the onset of drainage, that will significantly strengthen your argument of bypass flow through macropores.

AC: We thank the reviewer for this helpful suggestion. In the revised manuscript, we have included the VWC profiles at the onset of drainage for FM15 and FM16 (see revised Fig. 8 and revised Results 3.3.) These profiles illustrate the spatial distribution of liquid water content immediately at the observed drainage response.

RC2: L337: Replace 'reflecting' with 'suggesting'. You don't have direct evidence of this.

AC: We agree that the term "reflecting" was too strong in this context. The wording has been revised ("consistent with") to better reflect the inferential nature of the interpretation (see also response to Reviewer 1, Major Comment 1).

RC2: Figures 5, 6, and 7 clearly show edge effects and that your set-up did not produce exclusively top-down thaw. That being said, I don't think this affects the interpretation of your results. I suggest the authors walk back their strong statements about the set-up's ability to reproduce one-dimensional vertical freeze-thaw. This is another reason where showing the vertical versus lateral thermal gradients would be helpful… although from these figures I doubt the vertical gradients are at least an order of magnitude greater than the lateral thermal gradients.

AC: The original wording may have overstated the degree to which the experimental setup reproduced strictly one-dimensional, top-down freeze-thaw conditions. In the revised manuscript, we have moderated this language to avoid implying exclusively vertical thermal propagation. In addition, we have quantified the relative magnitude of vertical and lateral temperature gradients during the freezing phase and included these metrics in the appendices as the reviewer suggested above. We therefore no longer describe the setup as producing purely one-dimensional freeze-thaw behaviour, but rather as predominantly top-down freezing under controlled boundary conditions.

RC2: L368-369: Soil shrinking or soil compaction/settling? I'm having a hard time seeing how the very coarse soils used in experiments would have that much shrinking? I would expect this to be more prevalent in fine grains soils.

AC: We agree that the original wording may have been misleading. Given the coarse grain size distribution and the absence of clay in our artificial soil mixture, classical shrinkage effects are unlikely. A more plausible explanation is mechanical settling or compaction of the soil matrix during wetting, particularly under low initial water content conditions. The effect appears to depend on initial water content, suggesting that increased pore-water lubrication may have facilitated structural rearrangement and densification of the granular matrix. This interpretation is consistent with the relatively high bulk density of the soil and its broad grain size distribution. However, we acknowledge that the precise mechanism cannot be resolved definitively within the present experimental framework. In the revised manuscript, we have therefore replaced the term “soil shrinking” with wetting-induced settlement.

RC2: L375: Are you sure this is due to advective heat transport? I don’t think your data allows you to make such a strong statement. See previous comments about edge effects. Soften your language please.

AC: We agree that the original wording was too strong and could be interpreted as implying a causal dominance of advective heat transport. In the revised manuscript, we have substantially revised the corresponding passages in both the Results and Discussion sections to clarify the underlying energy balance. The observed downward migration of the freezing front is now described as a coupled thermo-hydraulic response of the system, primarily controlled by initial thermal conditions and conductive heat transfer. While advective heat redistribution associated with infiltration may contribute locally in the macropore experiments, its relative importance cannot be quantified or isolated based on the available data. We have therefore removed language suggesting that advection was the primary driver and reformulated the interpretation accordingly (see revised L. 370, L. 395, L. 399, and L. 439 (see response to Reviewer 1, Major Comment 6).

RC2: L402: Your figures are units of degrees Celsius, yet you discuss in Kelvins. While I know the changes are equivalent, you should be consistent, I suggest using degrees Celsius.

AC: We agree and changed all units of temperature in degrees Celsius in the revised manuscript.

RC2: L427: Replace ‘demonstrate’ with ‘suggests’.

AC: The sentence has been revised accordingly to use more cautious wording (“suggesting”), in line with the broader revisions addressing inferential language throughout the manuscript (see also response to Reviewer 1, Major Comment 1).

RC2: L434-435: The main reason why these results may differ from Pittman et al. (2020) is that soils used in these experiments had significant amounts of smectite and thus swelled significantly at high saturation which likely sealed many of the macropores. Similar observations were seen at the field site where these cores were taken from in Mohammed et al. (2019), at the site named Triple G (figure 5) where recharge through was frozen ground was observed in in MW2, but at 80 cm the soil slowly became saturated while under the zero-degree curtain and no further infiltration and groundwater recharge was observed until the soil profile thawed.

AC: We thank the reviewer for this insightful comment and for highlighting the role of soil mineralogy, particularly smectite swelling, in influencing macropore functionality under frozen conditions. We agree that this mechanism provides an important context for interpreting differences between our results and those reported by Pittman et al. (2020). In the revised manuscript, we have incorporated this explanation into the Discussion (Sect. 4.2), explicitly noting that the clay-rich soils used by Pittman et al. likely experienced swelling and macropore sealing at high saturation, thereby limiting preferential flow. We also now refer to the field observations reported in Mohammed et al. (2019), where saturation under the zero-degree curtain inhibited further infiltration until thaw occurred.

RC2: L461-463: This is also a very novel, direct observation of pore-blockage due to sediment deposition and promoting freezing in macropores. Very nice!

AC: We appreciate the reviewer's positive assessment of this aspect of the study. The direct observation of pore blockage due to sediment deposition and associated freezing within macropores represents an important finding.