Response to Reviewer

Manuscript Title: Cold Climates, Complex Hydrology: Can A Land Surface Model Accurately Simulate Deep Percolation?

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Reviewer Comments and Author Responses

- **The reviewer's comments in orange**
  - (page # line #) refers to the page and the number where the annotated comment is placed.
- **The author's response in blue**
- **Excerpts from the manuscript in *green***

We would like to thank the reviewer for their time and constructive feedback. The comments are mentioned and addressed in the same order that appeared in the annotated PDF. When multiple comments point to the same issue, a response is provided under the first comment in this document.

Comment 1 (page 1 line 22)**:** Trenberth's seminal article makes no mention of groundwater or groundwater recharge. Please cite a more relevant article.
Response: We will replace Trenberth with the following references:
Green, Timothy R., et al. "Beneath the surface of global change: Impacts of climate change on groundwater." *Journal of Hydrology* 405.3-4 (2011): 532-560.
&
Taylor, Richard G., et al. "Ground water and climate change." *Nature climate change* 3.4 (2013): 322-329.

Comment 2 (page 2 line 32)**:** What is 'them' in this context?
Response: We will rewrite the sentence to remove the ambiguity. The revised sentence will be: "However, they substantially minimize errors compared to indirect methods and can even be used to calibrate those indirect approaches.".

Comment 3 (page 3 line 67)**:** According to Figure 2, L1 and L2 appear to have different heights. Is this correct and, if so, it deserves a brief mention? What other features distinguish the two lysimeters?
Response: The only difference between the L1 and L2 lysimeters is their heights. We will mention this in the revised version. The comparison between L1 and L2 was the subject of a study by our team (*Field-based assessment of the design of lysimeters for landfill final cover seepage control*; Kahale et al. 2022). It was shown that the L2 lysimeter collected almost as much percolation as the L1 (less than 5 % difference).

Comment 4 (page 4 line 74)**:** Please specify the soil water retention model used and cite the source. See later comments about some inconsistencies between various SWR models (and their parameters) that seems to have been carried over from previous published works.
Response: We will introduce the Clapp and Hornberger soil water retention model with proper citation.

Comment 5 (page 5 Table 1)**:** Again, I would suggest denoting the SWR model as a footnote.
Response: We will mention the Clapp and Hornberger soil water retention model.

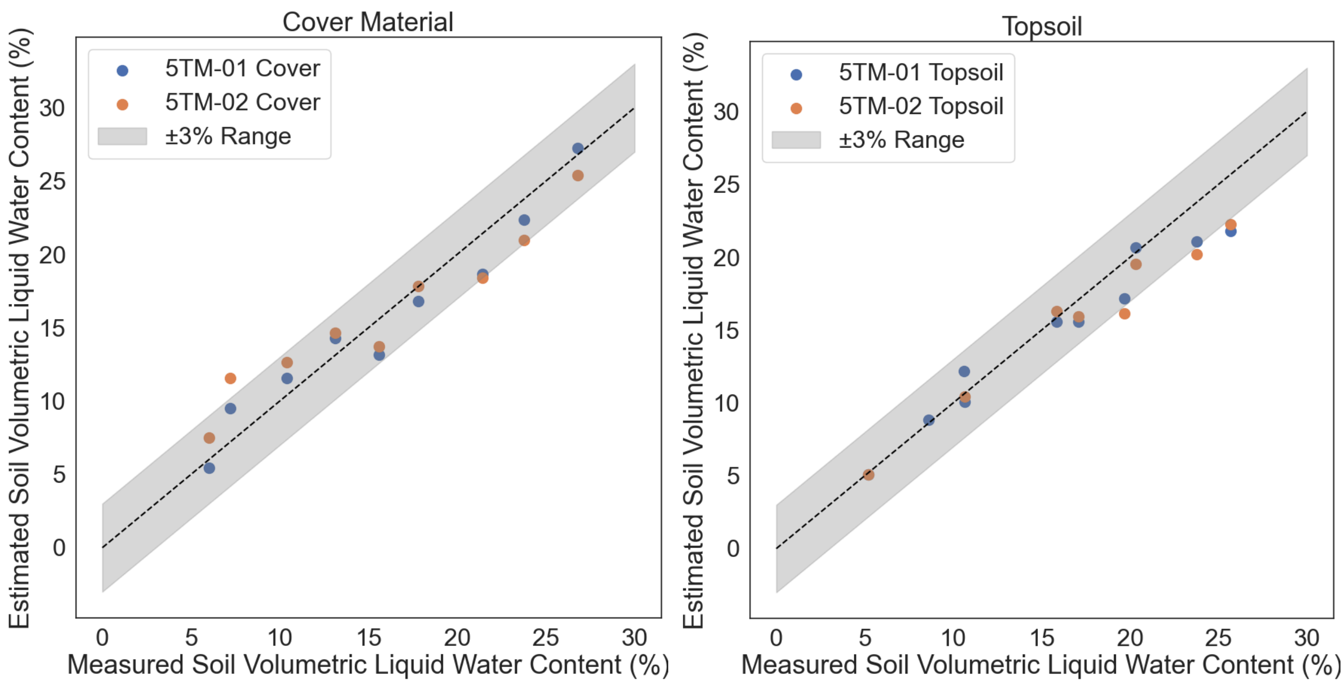
Comment 6 (page 6 line 103)**:** From what I can tell by looking at the literature cited here (I have not used SVS), SVS uses the Clapp and Hornberger (CH) model rather than the Brooks and Corey (BC) model. The two models have a similar foundation but there are key differences. CH uses a different definition for soil water saturation (BC defines effective saturation using a residual water content whereas CH does not). Also, the 'b coefficient' represents a pore size distribution index (BC's original terminology), but note that CH effectively defines b = 1/lambda, where lambda is BC original pore size distribution index.
Response: We acknowledge the difference between BC and CH models and we will introduce CH as the correct model for SVS in the revised version.

Comment 7 (page 6 line 108)**:** Soil water potential or soil matric potential would be preferable.
Response: We will replace *suction* with *soil matric potential* throughout the text.

Comment 8 (page 7 line 131)**:** K_sat previously.
Response: We will ensure that the revised text is consistent regarding symbols and capitalizations.

Comment 9 (page 8 line 174)**:** Our group's experience indicates that manufacturer reported soil moisture sensor accuracies are highly optimistic, especially if no site-specific soil moisture calibration is undertaken. When using 'factory' equations that are not soil-specific, the 5TM used in this study (and other similar low cost sensors) often have much more significant absoluter errors, although they are generally reasonable for assessing temporal trends. There are numerous studies in the literature that have identified these issues, such as:
Mittelbach, H., Lehner, I., & Seneviratne, S. I. (2012). Comparison of four soil moisture sensor types under field conditions in Switzerland. Journal of Hydrology, 430, 39-49, https://doi.org/10.1016/j.jhydrol.2012.01.041

Response: We acknowledge the limitations of the probes used in our study. The comparison between measured and simulated time series in our study was focused on identifying general trends.
Additionally, our group's limited calibration exercise in the laboratory suggests that a ±3% accuracy range is reasonable for these probes and the calibration equation suggested by the manufacturer, as illustrated in this figure:



Scatter plot showing the relationship between measured and calibrated soil moisture sensor readings for both cover material and Topsoil. The shaded area represents the expected accuracy.

Comment 10 (page 9 line 212)**:** Why is there no data shown for the 2021-2022 winter period? Figure 3 shows snow data and SVS results for the period, so it seems curious that they are not included here (and in subsequent figures). Were they omitted due to the (relatively) poorer SVS model performance for snow depth in late winter/spring 2022, which would presumably then affect soil temp and moisture estimation? Or perhaps the soil moisture sensors were not collecting data in this period and I simply missed it?

Response: We were not collecting soil moisture/temperature data after October 2021 (the data loggers were used in another project). We will augment the figures to contain the simulated values for October 2021 to October 2022.

Comment 11 (page 10 line 218)**:** Based on the sensor used, this is presumably liquid (unfrozen) water content, so it should be specified for both observations and simulated results.

Response: We will make it clear in the revised text (including figures) that our manuscript only reports soil volumetric liquid water content measured values.

Comment 12 (page 15 line 267)**:** The literature cited here misses early fundamental work by Stahli on preferential flow in frozen soil. Stahli, M., Jannson, P.-E. and Lundin, L.-C. (1996), Preferential water flow in a frozen soil - a two-domain model approach. Hydrol. Process., 10: 1305-1316.
doi:10.1002/(SICI)1099-1085(199610)10:10<1305::AID-HYP462>3.0.CO;2-F
In addition, Mohammed et al. (2018) published a conceptual model of water flow and infiltration dynamics in frozen, macroporous soils that deserves mention here since it nicely highlights the complexities involved. Mohammed also

goes on in subsequent field and modeling studies to demonstrate the influence of macropores on snowmelt infiltration and deep percolation in frozen soils.

Response: We thank the reviewer for the suggestions. We will incorporate these references into our revised text.

Comment 13 (page 15 line 278**:** Mohammed (noted above) previously observed similar non-sequential wetting in the Canadian prairies, with deep percolation and groundwater recharge occurring through a significant soil frost zone. Mohammed, A. A., I. Pavlovskii, E. E. Cey, and M. Hayashi (2019), Effects of preferential flow on snowmelt partitioning and groundwater recharge in frozen soils, Hydrol. Earth Syst. Sci., 23(12), 5017-5031. doi:10.5194/hess-23-5017-2019

Response: We will include this reference in the revised text, which resonates with the observed behavior in our case study.

Comment 14 (page 15 line 292)**:** I'm not sure what the point is here. Deep soil moisture readings tend to be relatively stable (in this and other studies) since there is less influence of surface temperature and evapotranspiration, so the relatively small CRPS seems to largely be a reflection of that stability. I am not certain it is truly a reflection of 'better' model performance.

Response: We agree with this assessment and we will revise the text to reflect the low variability rather than a good model performance.

Comment 15 (page 15 line 296)**:** Readers can also see from Figure 10a that SVS is severely underestimating the dynamic soil moisture fluctuations at shallower depths, whereas it is largely overestimating the soil moisture fluctuations at greater depths. It highlights that SVS is struggling to capture soil moisture drainage and evapotranspiration in both shallow and deeper portions of the soil profile. Thus, it suggests to me that the capillary barrier effects are not the sole reason for the apparent discrepancies between model and observations. For example, the influence of preferential flow undoubtedly has an important influence on summer soil water flow and storage conditions as well, which is also likely contributing to the difficulty in SVS simulating field observations.

Response: We appreciate the comment and will include in the revised text (discussion) that the observed disagreement might also stem from inaccurate evapotranspiration simulation. This in turn could be due to vegetation parameter values and not only due to potential model structural limitations concerning surface energy balance simulation.

Comment 16 (page 18 line 327)**:** I would argue preferential flow is also likely contributing to the issues you see in summer soil moisture/flux estimates from SVS also. 1D single-continuum representations of soil water movement and storage rarely capture the complex realities that we observe in the vadose zone.

Response: We will revise the text to suggest the potential influence of preferential flow, as another explanation concerning the disagreement between the model and the observations.

Comment 17 (page 18 line 329)**:** G.Y. Niu has long advocated for improved inclusion of cold regions processes, specifically frozen soils, in land surface models and more recently has highlighted the need for inclusion of macropores/preferential flow in the vadose zone to properly capture the hydrology of cold regions. I think your study nicely emphasizes the need for this same inclusion as it relates to deeper percolation and groundwater recharge, since surface and subsurface systems are connected across scales.

Niu, G. Y., & Yang, Z. L. (2006). Effects of frozen soil on snowmelt runoff and soil water storage at a continental scale. Journal of Hydrometeorology, 7(5), 937-952.

Agnihotri, J., Behrangi, A., Tavakoly, A., Geheran, M., Farmani, M. A., & Niu, G. Y. (2023). Higher frozen soil permeability represented in a hydrological model improves spring streamflow prediction from river basin to continental scales. Water Resources Research, 59(4), e2022WR033075.

Response: We will include the suggested references, as they have a similar message to ours, in emphasizing the representation of preferential flow within our models.

Comment 18 (page 21 line 430)**:** What is wi in this case? Soil frozen water (ice) content? Earlier (Eq 2 and 3), theta was used to denote volumetric soil water content, so I would advise sticking with a consistent variable set and nomenclature (e.g., soil water content vs soil moisture).

Response: We will adopt the suggested consistency and clarity in the revised text.

Comment 19 (general)**:** ... the study doesn't conclusively answer the title's question …

Response: We will ensure that our conclusion explicitly addresses the question raised in the title. Can SVS Accurately Simulate Deep Percolation? The answer is no. This is mainly because SVS currently lacks representation of frozen soil infiltration and movement of water through preferential flow pathways. We demonstrated that these drawbacks can render a model unreliable in the simulation of cold region deep percolation.

Additional notes: Any other in-line comment concerning grammar, dictation, and references not mentioned in this document will be addressed during the revision.