Response to Reviewer Comments

Reviewer Comments

General Comment

Reviewer Comment: *I appreciate the authors' effort in editing the manuscript and answering my comments. I still had some issues with understanding the goals of this work. However, based on the original submission, the revised submission, and the authors' comments to my first review, I now suspect that some of it has to do with some persisting lack of clarity in communicating the authors' goals and findings — especially early in the manuscript — in a way that would – in my opinion - best serve their work. It is now my impression that the actual intents are: 1) to make the remote sensing community aware that their satellite-derived product and whichever validation dataset they use to assess the performance of these products, are inadequate, as long as they only categorize soils into two states – thawed and frozen – based on the* $0°C$ *temperature. Because — as previously known and again showed here — soils in cold regions are in fact in an intermediate (partially frozen) state for an extended period of time; 2) To show that the 'problematic' (i.e. requiring soil-specific calibration) conversion of permittivities into water content is, in fact, unnecessary if qualitative evaluation of a soil is sought in terms of frozen — partially frozen – unfrozen categories; 3) To show that quantitative evaluation of soil freezing state is possible in terms of freezing probability, based on the permittivity data. Additionally, the authors confirm that rather than air and soil temperatures, it is the soil type and vegetation/ecozone factors that are controlling the soil freezing probability. Assuming that my understanding as described above is now correct, I would have the following suggestions to the authors: Regarding point 1) above: As I've already commented earlier, the fact that soil doesn't turn into fully frozen at* $0°C$ *is a fact well known to (not only) cold regions scientists working in some capacity with soils. Even if common remote sensing practices consider ground below a temperature of* $0°C$ *as frozen, it is obvious, even just from physics and chemistry, that this cannot be the actual case. However, this may not be reflected in the current state-of-the-art of remote sensing products. This being the shortcoming is suggested in the references in the authors' response to my comments. If this is the case, I can see how the wide climatic and geographic spread of the presented in-situ dataset would provide possibly more compelling evidence for the remote sensing practitioners than a dry reference to soil/cold region science literature. But in the interest of clarity and readability, this should be explained more clearly, and earlier in the paper. The scope and applicability of the presented work only become clear in the Conclusions, which I think provide a much better summary of the relevance and application of the presented work than the Introduction currently does. I'd suggest to the authors to focus more attention in the Introduction (and the Abstract) on current practices and limitations in remote sensing (which this contribution claimed to be targeting) rather than re-stating known facts of soil science. The authors' own response to my first review could be used as a base for this: "Many remote-sensing freeze-thaw* *studies—from early work (Kim et al., 2011; Zhang and Armstrong, 2001) to recent applications (Taghipourjavi et al., 2024; Gao et al., 2020; Kou et al., 2017; Roy et al., 2020; Derksen et al., 2017)—rely on* $0°C$ *soil or air temperature thresholds for training and evaluation, without accounting for partially frozen states. Only recently have researchers begun integrating soil moisture, temperature through SFCCs into freeze-thaw model evaluation (Rautiainen et al., 2025)." I think that if the Introduction section was developed around this key message — and perhaps around additional clearly outlined points, such as the mine ones above (if my understanding is correct now) — then the purpose, scope and contributions of the paper would be clearer from the start. I apologize to the authors for referring to permafrost in my previous round of comments. I have now understood that my confusion as to which soil state they were analyzing has come from an interchangeable use of words like frozen ground, seasonally frozen ground, and permafrost. From the Line 23, the authors write: "Frozen ground is defined as a condition in which pore water (the water inside the soil) turns into ice (Williams and Smith, 1989). Recognized as a key climate change indicator by the Environmental Protection Agency (EPA), this phenomenon is widespread and affects most land areas above* $45°N$ *latitude (Zhang et al., 2003)" I find this confusing, because the reference Zhang et al, 2003, discusses "distribution of seasonally and perennially frozen ground" in the northern hemisphere. But seasonally frozen ground is not a "key climate change indicator by the EPA". According to Climate Change Indicators in the United States report (Fifth Edition, July 2024 EPA 430-R-24-00), it is permafrost i.e. the permanently, not seasonally, frozen ground, that EPA considers a climate change indicator (specifically, the temperature of permafrost). I am (in Europe) used to permafrost being defined as permanently frozen ground, or ground frozen for a minimum of 2 consecutive years, not just frozen ground which, by authors' definition and references also encompasses seasonally frozen ground, which is not permafrost, which is not a climate change indicator. I would suggest to the authors to edit and align definitions of key concepts they're using/referring to.*

Response: We thank the reviewer for the clear synthesis of our work's objectives. Your summary captures our intent, and we agree that shifting the narrative focus from basic soil physics to the specific gaps in the Remote Sensing (RS) community significantly improves the impact and clarity of the manuscript. We have implemented the following revisions to address your suggestions:

- **Narrative Restructuring:** We have restructured the Abstract and Introduction to lead with the current limitations in RS freeze–thaw (FT) products. Rather than starting with soil science fundamentals, we now begin by discussing how state-of-the-art RS algorithms frequently rely on binary 0°C thresholds, neglecting the prolonged transitional states identified in this study.

- **Addressing the Gap in RS Evaluation Practices:** We added a new paragraph in the Introduction specifically detailing the existing gap regarding the integration of Soil Freezing Characteristic Curves (SFCCs) into RS evaluation practices.

- **Streamlining Soil Physics:** To improve readability, we shortened the sections focused purely on soil physics in the main body and moved the detailed theoretical derivations to the Supplementary Material.

- **Terminological Clarification:** We now ensure that throughout the manuscript we refer specifically to seasonally frozen ground (SFG), which is the focus of our dataset. We have strictly revised the text to use this terminology and have corrected the reference to the EPA climate indicators; the previously problematic lines have been removed to ensure accuracy.

We believe these changes sharpen the focus of the paper. These revisions are highlighted in the updated manuscript; we have omitted them here to keep this response letter concise.

Specific Comments

Comment 1

Reviewer Comment: *Line 46: "In this zone, the pore water content remains almost constant, regardless of temperature changes." I'd suggest an alternative formulation: "In this zone, the pore water content is independent of the soil temperature." If the authors' current formulations was correct, then they wouldn't have had all the issues with constructing the STCC where they argue that the variable water content (and water input) during thawing makes it problematic to reliably construct the STCC.*

Response: We agree with the reviewer's suggestion. The phrase has been updated to clarify that liquid water content is independent of soil temperature in this regime. This more accurate phrasing aligns with our later discussion regarding the challenges of constructing the STCC under variable moisture conditions. Please note that this section of the theoretical background has been moved to Section S1 of the Supplementary Material.

Revised Text (Sect. S1):

> "The first, termed the "unfrozen zone," occurs when the soil temperature is above the freezing point. In this regime, the liquid water content is independent of the soil temperature"

Comment 2

Reviewer Comment: *Line 63: as I understand the reference, the hysteresis discussed was observed in the active layer (seasonally frozen portion of the ground above permafrost), not in the permafrost portion of the ground.*

Response: We agree with the reviewer's correction. The reference to hysteresis in that study specifically concerns the seasonally frozen active layer above the permafrost, rather than the perennially frozen ground itself. We have amended the text to accurately specify the "active layer of permafrost-affected soils." This part has been moved to Section S1 of the Supplementary Material.

Revised Text (Sect. S1):

> "... reported approximately 10% higher unfrozen water content during freezing than during thawing at equivalent temperatures within the active layer of saturated, fine-grained permafrost-affected soils, reflecting latent heat effects..."

Comment 3 & 4

Reviewer Comment: *Fig 5: I am not super familiar with forest plots; however, the confidence intervals (plotted in the vertical direction) don't seem to have a corresponding scale on the y-axis. Should the confidence intervals be horizontal instead? Or should there be a secondary y-axis added ? Or are the confidence intervals only relative? Figure 6: The subfigures could benefit from a legend.*

Response: We thank the reviewer for these helpful suggestions regarding the clarity of our figures. We have implemented the following changes:

- **Figure 5 (Forest Plot):** We have revised the forest plot to ensure that the error bars (confidence intervals) are clearly mapped to the appropriate axis scale.
- **Figure 6:** We have added legends to all subfigures.

Comment 5

Reviewer Comment: *Line 300-302: Our results were generally consistent with the physical factors affecting SFCC shape, including soil mineral composition, particle size, plasticity, initial water content, dry density, solute concentration, freezing rate, confining stress, and freeze-thaw history (references). – Were all these soil parameters known, to say that the results were consistent with the values in the references? In general, an overview of the vegetation cover and soil type at each site could be relevant for drawing conclusions, as this information should be available from the moisture probes installation.*

Response: We agree with the reviewer that the original list of factors was too broad and included parameters that were not directly measured across our sites. We have refined the manuscript to focus specifically on *soil texture* (sand, silt, and clay percentages) and *initial water content*, as these are the primary physical drivers supported by our data. The specific effects of both texture and initial water content on our constructed SFCCs are now discussed in detail. To provide further context, we have included the initial water content data for the freezing cycles analyzed in this study in Table S2 of the Supplementary Material. Additionally, we have included detailed summaries of the modeled soil physical properties at the network level in Table S1. Finally, we have explicitly separated the discussion regarding the **thermal timing effect**—where initial water content and surface insulation influence the onset of freezing—from the discussion of the **SFCC shape** (b parameter). This ensures a clearer distinction between thermal dynamics and the temperature-dependent phase-change characteristics.

Revised Text

> "Our results were generally consistent with the physical factors known to govern the SFCC, particularly soil texture and initial water content (Chai et al., 2018; Bi et al., 2023a). For instance, the KN network, which features the highest clay content ($\approx 27\%$), exhibited the most gradual freezing transition ($b \approx 0.92$), a result consistent with soil physics principles where small pores and clay surfaces create a broad distribution of matric suctions that allow liquid water to persist at temperatures well below 0°C (Tian et al., 2014; Zhang et al., 2019; Bi et al., 2023b). Conversely, the GR network, characterized by high initial water content (VWC ≈ 0.40) and sandy texture

(> 60% sand, < 8% clay), exhibited a much sharper transition ($b \approx 3.24$) (Tian et al., 2014; Zhang et al., 2019; Bi et al., 2023b). It is worth noting that while higher moisture content increases latent heat and delays the onset of freezing—a thermal timing effect clearly visible in our heatmaps where GR begins freezing a month later than the drier TV network—the SFCC b value itself is not a measure of thermal velocity; instead, it reflects the fact that once freezing begins in such coarse, wet soils, the water in large pores transitions to ice abruptly. Similarly, the BT network ($b \approx 1.53$) demonstrates that even in sandy soils ($> 80\%$ sand), extremely low initial water content ($< 8\%$) can lead to a more gradual transition as the remaining water is held as thin films under higher suction. Despite recognizing these controlling factors, fully disentangling their individual contributions within a natural, open-system environment is inherently challenging. Multiple drivers operate simultaneously and interact nonlinearly, making it difficult to isolate each parameter's effect."

Comment 6

Reviewer Comment: *Line 302-305: "Networks with wetter soils or limited insulating organic layers—such as BJ and LR FM and CP (wet), and GR, TV, (with thin organic layers)—exhibited higher b values (b > 3), indicating sharper freezing transitions driven by abundant capillary water and/or faster freezing rates... In contrast, finer textured or drier networks, such as KN and BT, showed smaller b values (b < 1.5), reflecting more gradual phase transitions". - I'd expect the wetter soils to freeze relatively slower, due to more water that has to undergo phase change. Such soil would also typically be more fine grained. Also "abundant capillary water" would in my opinion be typical of a finer soil, and causing slower, not faster, freezing rates. I understand if the findings don't align with all the previous references, but there seems to be contradictions in the conclusions drawn by the authors. These apparent contradictions lead me to question whether one can say, as in line 300-302, that the results are indeed consistent with references?*

Response: We appreciate this observation. We agree that the original wording conflated thermal dynamics (the time-dependent rate of freezing) with the SFCC transition slope (the temperature-dependent sensitivity of liquid water content). We have revised this section to clearly distinguish between these two physical phenomena. While wetter soils do freeze more slowly in terms of time due to the latent heat of fusion (a thermal timing effect), the b parameter describes the rate of phase change as a function of temperature. In coarse-grained, sandy, and wet soils (such as the GR network), the majority of the water is held in large pores as "bulk" or "free" water, which transitions to ice abruptly near 0°C, resulting in a high b value. Conversely, in sandy but dry soils like the BT network, the transition is more gradual because there is less free and capillary water available, leaving a higher proportion of bound water. In contrast, fine-textured soils (such as the KN network) hold water across a wide range of small pores under high matric suction, allowing liquid water to persist at much lower temperatures and resulting in a gradual transition (low b). To resolve the apparent contradictions, we have:

– Clarified that the b parameter reflects the distribution of pore sizes and matric suction rather than the temporal velocity of the freezing front.

– Explicitly separated the discussion of the "thermal timing effect" (the delayed onset of freezing due to latent heat and surface insulation) from the "phase transition characteristic" (the b value).

Comment 7

Reviewer Comment: *Line 320: The volume mismatch and/or the sensor geometry should only affect the measurements in a narrow range of temperatures close to the freezing point, and thus unlikely to explain the consistent hysteresis observed at lower temperatures, such as $-5°C$ in Overduin et al. (2006), and $-4°C$ in Tomaskovicova & Ingeman-Nielsen (2024). In any case though, it appears that it is mostly the thawing point that is affected, and the temperature at which the soil begins to turn into ice is more physically plausible in all of the Pardo-Lara, Overduin and Tomaskovicova… datasets. Since the authors of the present manuscript excluded the thawing curves from the analysis, shouldn't it help constrain the Tf estimation to physically more plausible (below $0°C$) values? And, in Line 321: I wonder if some of the positive Tf could be remedied by temperature sensor calibration? The temperature sensor accuracy of only $0.5°C$ in what corresponds to the transitional zone is relatively coarse (best practices for permafrost monitoring recommend $0.1°C$ or better; here I am citing permafrost practices, because it is in monitoring of frozen ground where accuracy of soil temperature measurement below $0°C$ is very important). Moreover, the temperature offset is non-linear, so the error can be different through a range of negative temperatures experienced at a site – even more so for what looks like a temperature sensor with relatively low accuracy.*

Response: We appreciate the reviewer's insights regarding sensor accuracy and the limitations of the T_f estimation. We agree that the hardware accuracy of the sensors—ranging from ± 0.3°C for the HydraProbe to ± 0.5°C for the TEROS12 and CS616—is the primary driver of the observed positive T_f values, rather than just volume mismatch or sensor geometry alone. We also agree that excluding the thawing curves helps in isolating the freezing transition. However, as noted in the revised text, the hardware's inherent accuracy and resolution remain the ultimate constraints on the precision of these estimates. To address the reviewer's concerns, we have revised the discussion to explicitly state that the positive T_f offsets fall within the expected accuracy bounds ($\pm 2\sigma$) of the sensor hardware. We now identify sensor accuracy as the primary cause of positive T_f values, acknowledging that while geometry and volume mismatch contribute, they are secondary to the hardware's specified limits. Collectively, these points confirm that positive T_f values are likely artifacts of sensor accuracy and resolution rather than a reflection of physically implausible soil freezing temperatures.

Revised Text

> "A major source of systematic error in permittivity-based sensors is the volume mismatch between the permittivity-sensing domain and the temperature-sensing thermistor (Pardo Lara et al., 2020, 2021). As shown by Pardo Lara et al. (2021), this mismatch can lead to apparent hysteresis and positive freezing-point depression artifacts, where permittivity sensors detect freezing before the thermistor. This occurs because permittivity sensors integrate over a larger, water-biased volume—one that shrinks in wetter soils—while thermistors measure temperature within a much smaller, localized, and water-independent zone (Hansson and Lundin, 2006; Logsdon, 2009; Pardo Lara et al., 2021). Sensor geometry also plays a role. The thermistor in the TEROS12 is embedded in the central needle,

minimizing spatial offset, while in the HydraProbe it is located in the base plate. The CS616 lacks a built-in thermistor, requiring external placement, which can exacerbate mismatch effects. However, the primary constraint in estimating T_f remains sensor accuracy. The stated accuracies of ± 0.3°C for the HydraProbe and ± 0.5°C for the TEROS12 are significant within the narrow freezing transitional zone. In our analysis, approximately 90% of T_f values from the HydraProbe and all values from the TEROS12 and CS616 fell within their respective sensor uncertainty ranges ($\pm 2\sigma$°C). This suggests that while volume mismatch may contribute to T_f variability, the observed positive offsets are primarily a function of sensor accuracy bounds rather than a significant systematic bias in the freezing process itself."

Comment 8

Reviewer Comment: *Line 362: "We did not perform site or sensor-specific calibrations for permittivity or temperature, but the sensors used—TEROS12, HydraProbe, and CS616—have been extensively validated and shown to perform reliably across diverse soil conditions". Temperature sensor accuracy is not directly linked to soil conditions, so I don't think that part of the argument adds credibility to the accuracy of the soil temperature measurements.*

Response: We agree with the reviewer's observation. Temperature sensor accuracy is a hardware-specific property and is not influenced by soil texture or mineralogy. Consequently, we have removed the lines that did not effectively support this point to ensure the technical accuracy of our discussion.

Comment 9

Reviewer Comment: *Line 328: the two sentences starting in this line are repeated twice, delete the repetition.*

Response: We apologize for the oversight. The repeated sentences have been removed to improve clarity.

Comment 10

Reviewer Comment: *Line 374: "This [underestimation of permittivity] is unlikely to affect our analysis, as saturated soils are rare during freezing periods—except at FM, where soil rarely freezes." Yet the E boreal forest was described as a "wet". Do we have estimates of the actual water contents?*

Response: We thank the reviewer for this comment. To provide better context, we have added the average initial soil water content for the freezing cycles included in our analysis to the Supplementary Material (Table S2). Within our dataset, the average initial volumetric water content (VWC) ranges from approximately $0.40\ \mathrm{m^3/m^3}$ in the wettest networks to $0.07\ \mathrm{m^3/m^3}$ in the driest. It is also worth noting that we did not include sites where the soil remained fully saturated throughout the season. In these cases, the soil often fails to freeze due to the high latent heat of the water volume, making it impossible to construct an SFCC. Consequently, our analysis focus is restricted to soils where a detectable phase transition occurs.

Comment 11

Reviewer Comment: *Line 343: "The thawing cycles, however, were not analyzed in this study because constructing the STCC from in situ measurements is not reliably feasible." – In my experience, before the temperature at the depth of the moisture sensor first time goes above the freezing point in the thawing season, the thawing curve should be possible to construct, because the liquid moisture content dependance primarily on temperature holds, assuming that a frozen ground doesn't accept much additional moisture from rainfall or rainfall or thawing. Perhaps hysteresis, or biased* T_f *during thawing, are more valid reasons as to why the STCC is unreliable, or why it is different from the SFCC.*

Response: We agree that while the temperature remains sub-zero, the physical relationship between temperature and liquid water content theoretically persists. However, the primary challenge in our dataset is that we cannot achieve a reliable model fit with acceptable uncertainty for the thawing cycle. This is because, in an "open system" like the active layer during spring, the total water content is rarely constant. Once thawing begins at the surface, the continuous downward influx of water—primarily from snowmelt—violates the fundamental assumption of a stable total moisture baseline required for curve fitting. Because the model cannot distinguish between temperature-driven phase change (ice-to-water) and the physical addition of external water, the parameter uncertainty becomes unacceptably high. We have revised the text to clarify that this external influx prevents the isolation of the thermal phase-change signal.

Revised Text:

> "Thawing cycles were excluded from this analysis because constructing a reliable STCC from in-situ measurements is not feasible. Once air temperatures rise above 0°C, the continuous influx of water—primarily from snowmelt—violates the assumption of constant total moisture content required for curve fitting. Because the model cannot identify a stable moisture baseline, the resulting uncertainty in the fitted parameters becomes unacceptably high. Ultimately, this external influx prevents the isolation of temperature-driven phase changes from changes in the total soil water volume."

Comment 12

Reviewer Comment: *Line 351-353: For this kind of application, could another solution be to monitor the soil moisture at a slightly larger depth, where only the sustained freezing/thawing cycles penetrate, i.e., at least below the depth of daily temperature fluctuations (20 cm?).*

Response: We thank the reviewer for this suggestion. While monitoring at greater depths (e.g., 20 cm) would likely provide more stable temperature and moisture signals, we focused our analysis on the top 5 cm to maintain consistency with our ultimate research objective. The next phase of our project involves training and evaluating a new passive microwave remote sensing algorithm. Since passive microwave sensors (such as L-band radiometers) primarily penetrate and "see" only the top 5 cm of the soil surface, focusing on measurements at this depth is critical to ensuring the representativeness of our model for remote sensing applications.

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