Reviewer #1

We sincerely thank the reviewer for the careful evaluation of our manuscript and for the constructive comments. We have revised the manuscript accordingly and provide our point by point responses below.

Comment: I believe this study offers a novel perspective on how soil temperature and moisture control soil respiration. Generally, soil temperature is identified as the primary controller of the temporal variability of soil respiration (SR), subsequently modulated by water availability, which limits both autotrophic and heterotrophic respiration when water is scarce. This study demonstrates that, in these temperate forest ecosystems, the opposite view holds true: temperature appears to control the SR response to moisture. This control occurs above a threshold of 15-17°C. In other words, above a certain temperature, the variability in Rs is mostly explained by fluctuations in soil moisture, but with the modulation of Ts. Below this threshold, soil metabolic activity appears to decrease and lose all relationship with moisture, with temperature being the sole factor controlling the temporal variability of Rs.

Response: We thank the reviewer for this thoughtful summary of our study and for recognizing the novelty of our perspective on the interactive roles of soil temperature and soil water content in regulating soil respiration. We agree that the main finding of this study is that the contribution of SWC to Rs changes across temperature conditions. Specifically, the contribution of SWC becomes more evident above approximately 15°C, whereas under cooler conditions the relationship between SWC and Rs becomes weaker or less evident. In addition, a breakpoint in the Rs–Ts relationship was identified near 17°C. In the revised manuscript, we have further clarified this interpretation and its ecological context.

Comment: The result seems interesting to me, as I mentioned above, but some factors and limitations should be considered and discussed in the manuscript. First, the scope of this study needs to be better contextualized, since in this study system water availability is quite high throughout the year, and particularly during the warmer periods. In temperate systems outside the monsoon influence, this is not the case, and soil temperature and moisture tend to have a negative seasonal relationship. That is, in non-monsoon temperate systems, the relationship between soil moisture and Rs at high temperatures is very likely nonexistent because there is insufficient moisture for even minimal autotrophic or heterotrophic metabolism. In other words, in these systems it is humidity that limits the response to temperature, and not the other way around. This should be discussed and contextualized in the discussion.

Response: We appreciate the reviewer's valuable comment that the scope of our interpretation should be more clearly contextualized, and we agree with this point. In the revised Discussion, we now explicitly state that our findings are most applicable to monsoon influenced temperate forests, where SWC tends to remain relatively high during the warm season, and that this pattern may not be directly transferable to non-monsoon temperate systems, where SWC is more seasonally constrained. This revision has been added on Page 16, Lines 334–340.

4.5 Implications and considerations

> "In particular, in temperate deciduous forests, monsoon climates tend to maintain relatively high soil water availability during the warm season because precipitation is concentrated in this period, whereas non monsoon climates tend to experience intensified seasonal drying with increasing temperature (Chae, 2011; Prigoliti et al., 2023). In this context, our findings should be interpreted within the hydroclimatic setting of monsoon influenced temperate forests where soil water availability remains relatively high during the warm season, and the same pattern may not occur in non-monsoon temperate forests where water availability is seasonally limited."

Comment: Another aspect that I believe the authors do not sufficiently discuss in this study are the results obtained in Figure 3. Particularly interesting are the "jumps" in basal respiration rates between the different temperature ranges above 15°C, and the difference in the shape of the relationship between soil moisture and Rs between these ranges. On the one hand, what do these jumps respond to? They aren't controlled by humidity or temperature. They may respond to changes in the biomass of microorganisms and fruit roots in different phenotypic phases during warmer periods. Regarding the form of the relationship, I partially disagree with the interpretation of Fig 3. Made in lines 260. If oxygen limits, should limit Rs either similarly for each Ts ranges or should be higher at the highest temperatures, when O2 demand peaks. Here does not seem consistent with neither of those cases. Again, I think that the phenophase might have played a role here, because in these Ts ranges data from spring and fall are mixed up, and things are generally very different phenologically speaking between spring and fall. Maybe to try to separate also this phenophases with the temperature ranges will help explaining the differences in the slope of the relation between temperature ranges.

Response: We sincerely thank the reviewer for this thoughtful and valuable comment regarding the interpretation of Figure 3. In the revised manuscript, we have further expanded the Discussion to clarify that the observed jumps in basal respiration around 15°C cannot be fully explained by Ts or SWC alone and may also reflect seasonal biological changes, including changes in fine root biomass and activity as well as enhanced microbial activity during warmer phenophases. In addition, we have refined our interpretation of the nonlinear response observed in some warm Ts bins. In the revised manuscript, we distinguish more clearly between the weakening of the Rs response at high SWC within warm Ts bins, which may reflect physical constraints under wet conditions such as limited oxygen diffusion, and the observed jumps in basal respiration around 15°C, which may also be associated with seasonal biological shifts. In this way, the revised text more clearly separates these two features in Figure 3: the nonlinear response of Rs to SWC within warm temperature ranges and the jumps in basal respiration across temperature ranges. These changes have been incorporated into the revised Discussion on Page 15, Lines 280–282, and in the surrounding discussion text where we clarified the distinction between the nonlinear Rs response to SWC within warm Ts bins and the apparent jumps in basal respiration across temperature ranges.

> “In addition, the shift in Rs response around 15°C cannot be fully explained by Ts or SWC alone and may also reflect seasonal biological changes, including increases in fine root biomass and activity and enhanced microbial activity during warmer phenophases (Schindlbacher et al., 2015; Heinzle et al., 2023).”

Comment: Finally, I understand that Figure 5 confirms what we see in Figures 3 and 4. What the authors mean is that below this threshold (which largely coincides with the 15°C shown in Figure 3), the variation in Rs is entirely explained by temperature, while above it, Rs is controlled by the interaction of Ts and SWC. However, the correlation coefficients and the slope that would confirm that Ts's control is more important below the threshold are not shown. It is also possible that below the threshold the roles of Ts and SWC have reversed: Ts is the primary control, modulated by SWC.

Response: We sincerely thank the reviewer for this careful and important comment regarding the interpretation of Figure 5. We recognize that the original wording could be read too strongly, particularly in suggesting that Rs variability below the threshold is explained entirely by temperature. In the revised manuscript, we therefore modified the text to clarify that our results support a change in the relative contribution of soil water content (SWC) across the breakpoint, rather than indicating that temperature alone explains the pattern below the threshold. More specifically, we revised the relevant text to emphasize that the relative contribution of SWC becomes more evident above the breakpoint, whereas below the breakpoint the contribution of SWC appears limited or less evident under the conditions of this study. Accordingly, we no longer describe the pattern below the breakpoint as entirely temperature controlled. This revision is consistent with the results presented in Figures 3–5, which together show that the contribution of SWC differs across temperature conditions and becomes more pronounced above the breakpoint. We appreciate the reviewer’s suggestion that, below the threshold, Ts may act as the primary control while SWC plays a secondary or modulating role. In response, we revised the manuscript to present a more cautious interpretation and to avoid stronger claims than those directly supported by our analyses. These revisions have been incorporated into the revised manuscript in Section 3.5, Section 4.3, and the Conclusions, where we clarified that the contribution of SWC becomes more evident above the breakpoint and remains limited or less evident under cooler conditions.

Comment: Title: looks like a riddle. Please rephrase

Response: We thank the reviewer for this helpful comment. We agree that the original title could be made clearer and more direct. In the revised manuscript, we have rephrased the title to better reflect the main finding and contextual scope of the study.

The revised title is:

"Temperature dependent changes in the contribution of soil water content to soil respiration in a monsoon influenced temperate deciduous forest"

Comment: References: Most references are from the last 5 years. Most important references in the study of the role of soil temperature and soil moisture in soil respiration are before 2020. Please make a better bibliographic search and cite other key papers

Response: Thank you for this helpful comment. We agree that earlier foundational studies are important for properly contextualizing the roles of soil temperature and soil water content in soil respiration. In the revised manuscript, we strengthened the background and Discussion by incorporating additional key references from the earlier literature, particularly in the Introduction and Discussion sections. Several foundational studies were already included in the original manuscript, and these have now been complemented by a broader set of classic references to improve the historical and conceptual context of the study.

Comment: Methodology: method section could be substantially improved. You learn at the end of the methods section that there were 5 automated chambers installed (what was the criteria where to install them, for instance?) but no information about where the sensor of SWC and Tz where installed with respect to the Rs measures. Were SWC and Ts measured near each automated chamber? Or there were only one measurement point? By the way, why the authors use the acronym SMC instead of the more commonly used in literature SWC (soil water content) to refer to soil moisture? It is just a matter of consistency and reproducibility of results.

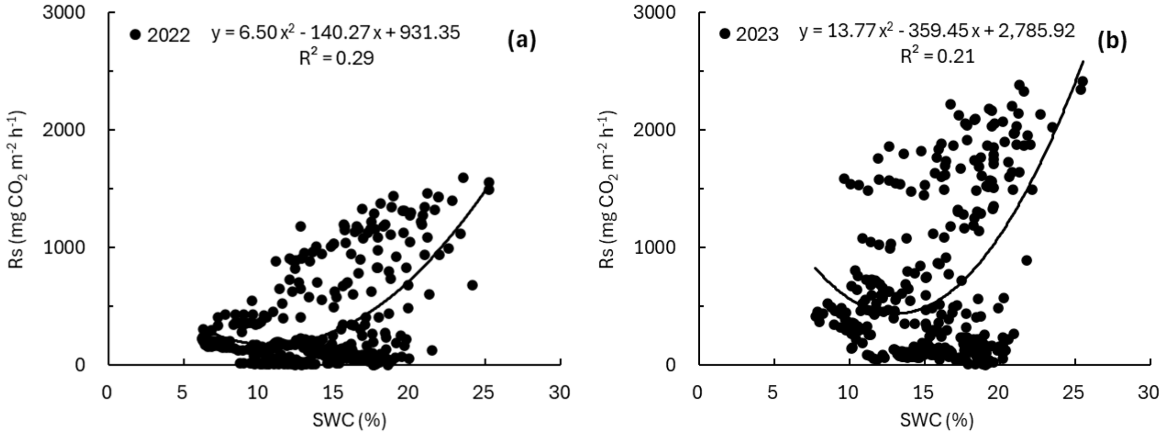
Response: Thank you for this helpful comment. In the revised manuscript, we substantially clarified the Methods section to improve transparency, consistency, and reproducibility. Specifically, we now state more clearly that soil respiration was measured using five automated chambers, and we added a more detailed description of the chamber installation criteria. The chambers were installed at least 1 m apart and were arranged to reflect the spatial heterogeneity of soil and surface environmental conditions within the study plot. To avoid spatial bias in chamber placement, the characteristics of each location were carefully assessed and the five chambers were arranged in a pentagonal configuration within the plot. Locations with substantial surface disturbance or conditions that could reduce measurement stability were excluded. In addition, the chambers were installed at least 50 cm away from tree stems to minimize the potential overestimation of root respiration associated with dense root distribution near the stems.

We also clarified the spatial relationship between the Rs measurements and the environmental sensors. In the revised Methods, we now explicitly state that soil temperature and soil water content were measured for each chamber, with the Ts and SWC sensors installed within 5 cm of each chamber so that they represented the same local environmental conditions. We further clarified that the Ts sensor was positioned adjacent to the SWC sensor while avoiding direct physical contact or interference that could affect SWC measurements. These revisions have been incorporated into the revised manuscript on Pages 5–6, Lines 110–138.

Regarding terminology, we agree that SWC is the more commonly used and reproducible term in the literature. To improve clarity, consistency, and accessibility for readers, we revised the terminology throughout the manuscript and now use SWC consistently.

Comment: Figure 2. Why different symbols if they are presented in two different panels?

Response: Thank you for this helpful suggestion. We agree that using different symbols was unnecessary because the two years were already separated into different panels. In the revised figure, we therefore unified the symbols across both panels to improve visual consistency and avoid redundancy.



Comment: Discussion: throughout the discussion I see many paragraphs that looks more results than discussion (e.g. paragraphs 250, 275, 280…). The discussion should be used to discuss results rather than to reporte them again. Please try to improve this too.

Response: Thank you for this helpful comment. In response, we substantially revised the Discussion to ensure that it functions primarily as an interpretation of the results rather than a repetition of them. Repetitive descriptions of numerical values, statistical significance, and figure based patterns were reduced throughout the revised manuscript. We also rewrote the relevant paragraphs to highlight the ecological meaning of the observed patterns and to clarify the mechanisms underlying the temperature dependent contribution of soil moisture to soil respiration. In particular, the revised Discussion now places greater emphasis on why the contribution of SWC became more evident under warm conditions and how the breakpoint may be interpreted in relation to possible shifts in the relative importance of controls on soil respiration.

Comment: Figure 5. Which is the fit for each part of the threshold? It would be nice to see the slope and R^2 for the two different sections at both sides of the threshold. Does temperature fit better Rs at colder periods? Below the threshold there is also some variability around the model. Could this be done to fluctuations in SWC?

Response: Thank you for this important comment. In Figure 5, separate linear fits were applied to the two segments on either side of the breakpoint, and we agreed that it was necessary to present the slope and R^2 values for each segment more clearly. Accordingly, we added Table S2, which summarizes the Ts range, slope, R^2, *p*-value, and mean SWC for the segments below and above the breakpoint in both years.

However, the fact that the relationship between Ts and Rs was also evident below the breakpoint does not necessarily mean that temperature explains Rs better under colder conditions. Rather, our results suggest that the overall contribution of SWC below the breakpoint was limited or less evident, which may reflect possible differences in the regulatory mechanisms controlling soil respiration across the breakpoint. This does not mean that SWC had no effect at all, and although the relative importance of Ts may have increased under colder conditions, the present data do not allow us to determine the extent of this clearly.

In addition, some of the variability remaining around the fitted relationship below the breakpoint may have been associated with fluctuations in SWC. However, this variability may reflect not only short term changes in SWC following rainfall events, but also other environmental changes associated with rainfall. Therefore, additional analyses or experiments that separate the effects of temperature from those of various environmental drivers, including extreme dry and wet conditions, would be needed to more clearly distinguish the relative roles of temperature and SWC under colder conditions.

Table S2: Segment specific regression statistics for the relationship between soil temperature and soil respiration below and above the breakpoint in 2022 and 2023. Ts range, slope of the Ts–Rs fit, R^2, *p*-value, and mean SWC are shown for each segment.

Year	Section	**Ts range**	**Slope of Ts–Rs fit**	**R^2**	***p*-value**	**Mean SWC (%)**
2022	Below breakpoint	Ts < 16.94	10.88	0.65	< 0.0001	13.58
2023	Below breakpoint	Ts < 16.77	30.27	0.77	< 0.0001	15.27
2022	Above breakpoint	Ts ≥ 16.94	127.91	0.66	< 0.0001	15.22
2023	Above breakpoint	Ts ≥ 16.77	190.00	0.68	< 0.0001	16.18

Comment: There are still things not to clear to me. The authors show this equation: $Rs = a\exp(bTs)(cSMC2 + dSMC + e)$; which is actually not applied right? at least there is no statistics provided for the fit of this equation, which is pretty much a standard equation used to predict effects of Ts and SWC.

What authors use instead is an equation with this form: Rs = Trange * ($cSMC2 + dSMC + e$) (Trange is what you called Tbins, which you actually defined arbitrary...) . Am I right?

IS there differences in the fit of those two equations?

Response: Thank you for this important comment. We agree that this point was not sufficiently clear in the previous version of the manuscript. The equation $R_S = a\exp(bT_S)(cSMC^2 + dSMC + e)$ was indeed applied in our analysis and was fitted to the full daily dataset. The 5°C temperature binning was not used as an alternative model, but only as a secondary step to examine more clearly how the contribution of SWC changed across temperature conditions. We have revised the Methods section to clarify that Equations (2) and (3) were first fitted to the full daily dataset, whereas the temperature bin analysis was used only for interpretation. We have also added fit statistics for the full dataset models. For the full daily dataset, the Ts-only model yielded an R^2 of 0.782, an RMSE of 274.94, and an AIC of 183,047, whereas the Ts + SWC model yielded an R^2 of 0.846, an RMSE of 230.77, and an AIC of 177,346.

Comment: I also propose two other equations:

Rs = Season*($cSMC2 + dSMC + e$)

Rs= Season* Trange*($cSMC2 + dSMC + e$)

and see which one gives the best fit

My feeling is that the different shapes of the relation between SWC and Rs will be related to season, which partially covaries with Ts, but not entirely. This may show that part of the variability in Fig 3 might also be related to phenology

Response: We sincerely thank the reviewer for this insightful suggestion. We agree that part of the variability shown in Figure 3 may also be related to phenology. To further examine this possibility, we conducted an additional comparison by separating the study period into foliage season (FS) and non-foliage season (NFS), which reflect differences in plant activity, while also considering temperature range. This comparison showed that the additional explanatory contribution of SWC was much more pronounced during the foliage season, whereas it was negligible during the non-foliage season. However, this comparison should be interpreted cautiously because the number of observations in the FS group below the estimated breakpoint was limited, which may reduce the stability of the fitted relationship in that subset. These results suggest that the apparent moisture control of soil respiration may shift, at least in part, in association with phenological changes in plant activity. However, a more rigorous assessment of phenological effects would require environmental overlap across phenological stages so that phenological influences can be distinguished more clearly from co-occurring temperature and moisture conditions. In field observations, such overlap is difficult to obtain because periods of high plant and microbial activity generally coincide with warmer conditions, whereas periods of low activity usually occur under cooler conditions. In addition, because this comparison was exploratory and did not directly distinguish autotrophic and heterotrophic components, we did not incorporate these grouped formulations into the main analysis. Instead, we clarified in the revised manuscript that part of the variability in Figure 3 may also reflect phenological influences, and we explicitly acknowledge this point as a limitation of the present study. A more direct evaluation of these phenological influences will require explicit assessment of root activity, together with separate quantification of autotrophic and heterotrophic respiration across phenological stages, in future work.

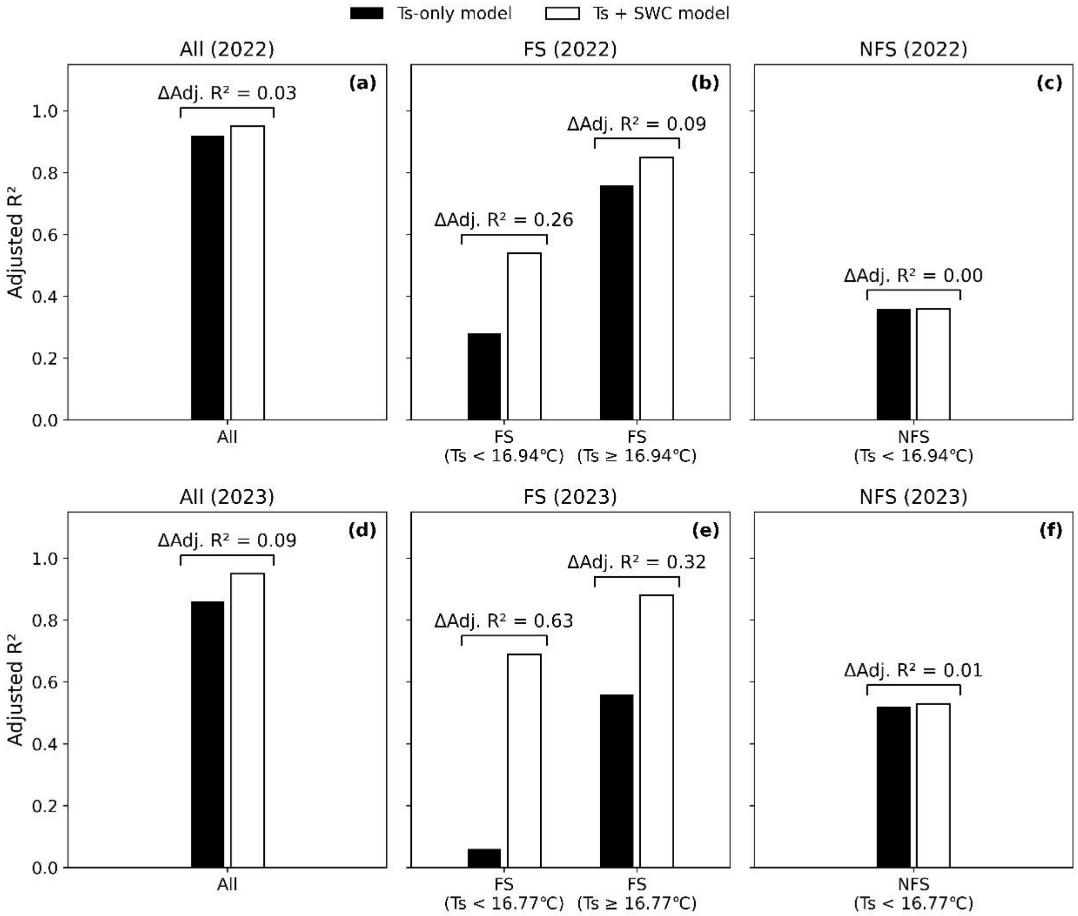


Figure R1. Exploratory comparison of adjusted R^2 values between the Ts only model and the Ts + SWC model for all observations, foliage season (FS), and non-foliage season (NFS) in 2022 and 2023. For FS and NFS, data were further separated according to the breakpoint-based temperature ranges identified from the Rs versus Ts relationship.

Reviewer #2

We thank the reviewer for the thoughtful and constructive comments, which helped us improve the clarity and interpretation of the manuscript. Our detailed responses are provided below.

Comment: The biological interpretation is largely consistent with established understanding. The invocation of substrate diffusion limitation under dry conditions, oxygen constraint under waterlogged conditions, and temperature-dependent stimulation of microbial and root activity is appropriate and well-referenced. Individual claims do not overreach the data.

Throughout the manuscript, the breakpoint in the Rs–Ts relationship is equated with a threshold in SMC sensitivity, and this equivalence is used to argue for a "reorganization of the dominant control structure governing Rs." This conclusion is repeated in the abstract, results, discussion and conclusions, but the logical bridge between the two analyses is never formally established. A change in slope in Rs~Ts does not imply a change in the variance of Rs explained by SMC, this would require additional assumptions that are neither stated nor tested. The bootstrap test confirms that the segmented model fits Rs~Ts better than an exponential; it says nothing about SMC sensitivity. More cautious phrasing throughout, acknowledging that the two lines of evidence converge suggestively without being formally linked, would substantially strengthen the manuscript.

A further interpretive gap concerns causal attribution. The strengthening of the SMC–Rs relationship above 15–17°C likely coincides with leaf-out and the onset of root activity, both of which covary with Ts and SMC in this monsoon system. Without autotrophic/heterotrophic partitioning, it is not possible to determine whether the breakpoint reflects a moisture threshold for microbial activity, a phenological transition in root respiration, or both. This ambiguity deserves more prominent acknowledgment than it currently receives.

Response: We thank the reviewer for this thoughtful and constructive comment. We agree that, in the original version, the breakpoint identified from the Rs–Ts relationship and the temperature dependent increase in the contribution of SWC to Rs variability were not sufficiently distinguished. As the reviewer correctly noted, the breakpoint near 17°C was estimated from segmented regression of the Rs–Ts relationship, whereas the temperature dependent contribution of SWC was evaluated separately from model comparisons across temperature bins using ΔAdj. R^2 and ΔAIC. We therefore agree that the breakpoint should not be interpreted as a formal threshold in SWC sensitivity.

In response, we revised the manuscript throughout to clearly separate these two lines of evidence and to moderate our interpretation accordingly. In the revised Methods, we now clarify that the estimated breakpoint was interpreted as a reference temperature indicating a structural change in the Rs response to Ts, while temperature dependent changes in the relative contribution of SWC were evaluated separately. In the revised Discussion, we also explicitly state that the breakpoint in the Rs response to Ts does not directly represent a threshold in SWC sensitivity. We now emphasize that the two patterns occur over a similar temperature range and may together suggest a possible shift in the relative importance of controls on Rs, but that this inference is suggestive rather than formally demonstrated.

We also agree that the ecological interpretation of the breakpoint should be made cautiously. In the revised manuscript, we now state more explicitly that the single site design limits the extent to which the identified breakpoint near 17°C can be generalized. In addition, because Rs was not partitioned into autotrophic and heterotrophic components, mechanistic interpretation remains limited. We therefore explicitly acknowledge that the observed breakpoint may reflect not only moisture related constraints on microbial activity but also seasonal or phenological changes in root activity, or both. We believe that these revisions improve the consistency between our analytical framework and the scope of our interpretation.

Comment: 27–28 — The authors invoke increasing extreme hydrological events as a motivation for the study, yet daily averaging of SMC and Rs is likely to dampen the very transient responses they allude to — most notably the Birch effect. Could the authors comment on whether rewetting pulses occurred during the study period, and how their representation may have been affected by the temporal aggregation applied?

Response: We thank the reviewer for this helpful comment. We agree that daily averaging can dampen very short lived post rainfall responses, including Birch type pulses. In response, we clarified in the Methods that daily aggregation was used to reduce the influence of these transient responses and to better evaluate the broader effect of SWC on Rs across temperature conditions. We also added to the Results that short term increases in Rs were observed following rainfall related increases in SWC during the study period. These revisions have been incorporated into the revised manuscript on Page 5, Lines 111–113, Page 8, Lines 183–185, and Page 16, Lines 330–332.

"Because rainfall can induce very brief increases in soil respiration immediately after rewetting, including Birch type pulses (Xu et al., 2004), daily aggregation was used to reduce the influence of these transient responses and to better evaluate the broader effect of SWC on Rs across temperature conditions."

"Additionally, short term increases in Rs were observed following increases in SWC after rainfall during the study period. Specifically, Rs tended to increase on the day of rainfall and on the following day, and a similar increasing pattern was also observed at 0, 1, and 2 h after rainfall at the hourly scale"

"Although short term increases in Rs were observed following rainfall, daily averaged data were used to reduce the influence of these transient responses and to more clearly evaluate the broader effect of SWC on Rs across temperature conditions"

Comment: 64–65 — The authors acknowledge that SMC effects on Rs depend strongly on timescale, yet daily averaging may itself attenuate the moisture signal they seek to quantify. The manuscript implicitly positions daily resolution as an improvement over annual-scale studies, but does not discuss what temporal resolution would be needed to fully resolve moisture–respiration dynamics near the identified threshold. This point deserves explicit treatment.

Response: We thank the reviewer for this important comment. We agree that the influence of SMC on Rs depends strongly on temporal resolution. In the revised manuscript, we clarified in the Introduction that annual aggregation can mask short term and condition dependent effects of SMC by averaging across heterogeneous seasonal conditions. We also explained that daily resolution was used to evaluate the broader effect of SWC on Rs under comparable temperature conditions, while explicitly acknowledging that daily averaging can still attenuate very short lived post rainfall responses. These revisions have been incorporated into the revised manuscript on Page 3, Lines 64–66

> "It has been reported that the extent to which SWC explains variability in Rs depends strongly on time scale, phenological state, and temperature conditions (Kim et al., 2019; Podzikowski et al., 2025). Accordingly, analyses based on annual aggregation can mask short term and condition dependent effects of SWC by averaging across heterogeneous seasonal conditions"

Comment: 185 — The SMC thresholds reported (10.8% in 2022, 13.1% in 2023) lack a methodological basis in the text. Were these derived from the fitted quadratic fonction, from a formal changepoint procedure, or from graphical inspection? A clarification is needed for reproducibility.

Response: We thank the reviewer for this comment. In the revised manuscript, we clarified that the reported SMC values were derived as the SWC values at which the quadratic function reached its minimum in each year. These revisions have been incorporated into the revised manuscript on Page 9, Lines 197–199

> "Based on the quadratic regression, the SWC value corresponding to the minimum of the quadratic function was estimated at approximately 10.8% in 2022 and 13.1% in 2023 (Fig. 2)"

Comment: Figure 5 — A change in slope in Rs~Ts may simply reflect the curvature of an Arrhenius-type temperature response rather than any shift in moisture sensitivity, raising the possibility that the identified breakpoint is an artefact of the functional form rather than an ecological threshold. A more direct test, for instance, examining at what temperature the residual variance of Rs (after Ts removal) begins to increase significantly, would more rigorously operationalize the authors' question. Adding SMC as a color overlay on the Rs–Ts scatterplot would also allow readers to visually assess moisture modulation across the temperature range.

Response: We thank the reviewer for this important comment. We agree that the breakpoint identified from the Rs–Ts relationship should not be interpreted directly as a threshold in moisture sensitivity, because a change in slope may also reflect the curvature of the temperature response itself. In the revised manuscript, we therefore moderated the interpretation throughout the text and clarified that the breakpoint was identified from the Rs response to Ts, whereas the temperature dependent contribution of SWC was evaluated separately using model comparisons across Ts bins. We also added a supplementary figure showing the residuals of the Ts only model plotted against Ts, with SWC indicated by color (Fig. S2). This figure provides a visual assessment of the variation not explained by Ts alone. Residuals were relatively more constrained at lower Ts, but tended to be more widely distributed in the warmer temperature range, including near and above the estimated breakpoint. However, this pattern is only suggestive and does not constitute a formal test of a threshold in SWC sensitivity. Accordingly, we revised the relevant text and now interpret the breakpoint more cautiously as a reference temperature in the Rs response to Ts, rather than as a direct ecological threshold in SWC sensitivity.

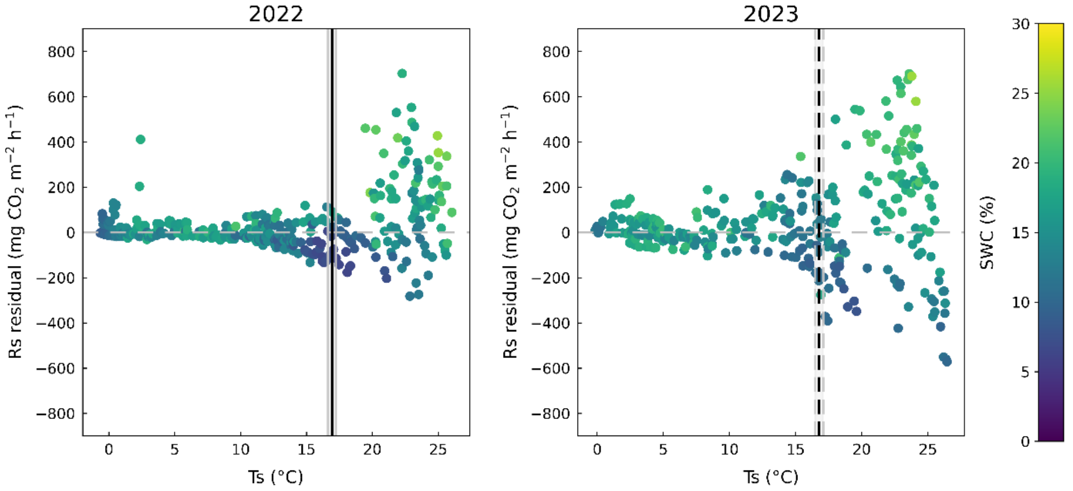


Figure S2: Relationship between Ts and residuals of Rs in 2022 and 2023. Residuals were calculated as observed Rs minus Rs predicted by the Ts only model. Points are colored by SWC. Vertical lines indicate the estimated breakpoint in the Rs response to Ts for each year, and the horizontal line indicates zero residual.