Reviewer #1

Overall recommendation: Minor to moderate revision required. The manuscript is close to acceptance but three issues require explicit resolution (detailed below).

Response: We sincerely thank the reviewer for the positive evaluation of our revised manuscript and for noting that the manuscript is close to acceptance. We have carefully addressed the three issues identified by the reviewer.

Comment — Breakpoint vs SWC threshold conflation

Abstract (lines 24–25): 'These results show a specific temperature range, suggesting possible difference in the relative importance of SMC on Rs variability across the threshold', the phrasing 'suggesting' is better than before, but 'across the threshold' still implies that the breakpoint and the SWC shift are the same feature. Consider rewording to 'near the temperature range where the contribution of SWC becomes more evident'.

Section 3.5 (lines 270–272): The sentence starting 'We then applied segmented regression [...] to examine whether a breakpoint occurred near the temperature range where this pattern emerged' is correctly framed. This phrasing should be applied consistently throughout the manuscript, including the abstract and conclusions.

Conclusions (lines 348–349): The phrase 'the relative contribution of SWC differed across this breakpoint' still reads as though the breakpoint is the driver of the SWC shift. Recommend: 'a breakpoint was identified near the temperature range where the relative contribution of SWC became more evident'.

Response: Thank you for pointing out this remaining ambiguity. We agree that the breakpoint in the Rs response to Ts should not be presented as a formal SWC threshold or as the driver of the shift in the contribution of SWC. To avoid this implication, we revised the relevant sentences in the Abstract, Section 3.5, and Conclusions to consistently distinguish the Rs–Ts breakpoint from the temperature range where the contribution of SWC becomes more evident.

"Therefore, projections of forest Rs may benefit from considering temperature dependent changes in the contribution of SWC, particularly near the temperature range where this contribution becomes more evident" [Lines 21–23].

"This temperature dependence suggests that the contribution of SWC to Rs variability becomes more evident under warm conditions, whereas its contribution is weaker or less consistent under cooler conditions" [Lines 278–279].

"In addition, a breakpoint in the Rs response to Ts was identified near the temperature range where the relative contribution of SWC became more evident, with estimates near 17°C in both years" [Lines 360–361].

These revisions clarify that the breakpoint and the increased contribution of SWC are treated as separate but related observations occurring within a similar temperature range, rather than as the same feature or as evidence of a causal threshold.

Comment — Autotrophic/heterotrophic partitioning and phenological ambiguity

Status: Addressed but acknowledgment remains superficial.

The authors now state that the breakpoint may reflect not only moisture-related constraints but also seasonal phenological changes in root activity. However, this notification appears late (Section 4.5) and only briefly in the conclusions. Given that the entire ecological interpretation of the breakpoint hinges on whether it is a moisture-driven or phenologicallydriven feature, this uncertainty deserves more prominent and earlier acknowledgment, ideally at the end of Section 4.4, where the breakpoint is interpreted mechanistically. The current placement in the 'Implications and considerations' section gives the impression that this is a secondary concern rather than a core limitation. Specific request: Add one explicit sentence at the end of Section 4.4 acknowledging that, without autotrophic/heterotrophic partitioning, the breakpoint cannot be attributed to moisture-driven microbial activity versus phenological transitions in root respiration.

Response: Thank you for this helpful suggestion. We agree that the uncertainty associated with autotrophic and heterotrophic contributions is a core limitation for interpreting the breakpoint and should be acknowledged earlier in the Discussion, where the breakpoint is interpreted mechanistically. Accordingly, we moved this point from Section 4.5 to the end of Section 4.4 and added an explicit sentence clarifying that the breakpoint cannot be attributed specifically to moisture related constraints on microbial activity versus phenological transitions in root respiration without component partitioning.

“However, because this study was conducted at a single site, the extent to which the identified breakpoint near 17°C can be generalized remains limited. In addition, because Rs was not partitioned into autotrophic and heterotrophic components, the breakpoint cannot be attributed specifically to moisture related constraints on microbial activity rather than to phenological transitions in root respiration” [Lines 326–329].

Together, these revisions acknowledge this uncertainty both within the mechanistic interpretation of the breakpoint and in the broader implications of the study.

Comment 5 — Temporal resolution and moisture signal attenuation

Status: Partially addressed. The revised Introduction now correctly states that annual aggregation can mask short-term SWC effects. However, the specific question I raised, what temporal resolution would be needed to fully resolve moisture–respiration dynamics near the identified threshold, is not addressed. This does not require a new analysis, but a brief sentence in Section 4.5 acknowledging the limitation of daily resolution for capturing threshold-proximate dynamics would be appropriate

Response: Thank you for this helpful suggestion. We agree that daily averaged data are useful for evaluating broader patterns across temperature conditions, but may not fully resolve short term moisture related Rs dynamics near the identified breakpoint. To acknowledge this limitation, we added a sentence in Section 4.5 clarifying that higher temporal resolution and event based approaches would be useful for resolving rapid Rs responses to changes in SWC within this temperature range.

"However, daily resolution may not fully capture short term moisture related Rs dynamics occurring near the identified breakpoint, and future studies using higher temporal resolution and event based approaches could better resolve rapid Rs responses to changes in SWC within this temperature range" [Lines 341–344].

New comment — Title

The revised title 'Temperature dependent changes in the contribution of soil water content to soil respiration in a monsoon influenced temperate deciduous forest' is clear and accurate. However, 'temperature dependent changes' is slightly redundant, by definition, if SWC contribution changes, it changes with something. This is a minor suggestion.

Response: Thank you for this helpful suggestion. We agree that the phrase "temperature dependent changes" may be slightly redundant. We therefore revised the title to make it more concise while retaining the main focus of the study on the temperature dependence of the contribution of SWC to Rs.

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

New comment — Research questions reformulation (lines 93–103)

The four research questions have been reformulated in the revised manuscript. Questions 1 and 2 are now well-posed. Question 4, however, remains somewhat circular: it asks whether a breakpoint 'occurs near the temperature range where SWC becomes more evident', which is essentially the same as asking whether the two analyses agree. The logical structure would be stronger if framed as: 'We assess whether the temperature range of the breakpoint in the Rs–Ts relationship is consistent with the temperature range above which the contribution of SWC increases, and discuss what this convergence may suggest about the controls on Rs.' This is an editorial suggestion, not a requirement.

Response: We thank the reviewer for this helpful editorial suggestion. We agree that the fourth research question can be framed more clearly by focusing on the

consistency between the temperature range of the Rs–Ts breakpoint and the temperature range where the relative contribution of SWC becomes more evident. Accordingly, we revised Question 4 in the Introduction to strengthen the logical structure and to avoid a circular formulation.

"We assess whether the temperature range of the breakpoint in the Rs response to Ts is consistent with the temperature range where the relative contribution of SWC becomes more evident, and discuss what this convergence may suggest about possible differences in the relative importance of controlling factors governing Rs" [Lines 91–93].

New comment — Fig. S2 and residuals interpretation (lines 278–280)

The authors added Fig. S2 showing residuals of the Ts-only model coloured by SWC. The description in the text states that 'residuals appeared relatively more constrained at lower Ts, but became more widely dispersed in the warmer temperature range, where variation in SWC was also more evident.' This is good. However, the authors should clarify explicitly that the wider dispersion of residuals in the warm range does not, by itself, demonstrate that SWC explains this dispersion, it is consistent with that interpretation, but also consistent with other unmeasured covariates (phenology, root biomass) becoming more variable in summer. A one-sentence explanantion would suffice.

Response: We thank the reviewer for this important clarification. We agree that the wider residual dispersion in the warmer temperature range should not be interpreted as direct evidence that SWC alone explains the additional variation. Accordingly, we added a sentence in Section 3.5 to clarify that this pattern may also reflect unmeasured seasonal covariates, such as phenological changes in root activity or fine root biomass.

"However, this wider residual dispersion does not by itself demonstrate that SWC explains the additional variation, because it may also reflect unmeasured seasonal covariates such as phenological changes in root activity or fine root biomass." [Lines 251–253].

New comment — Foliage season analysis (response to Reviewer 1)

In the response to Reviewer 1's last comment, the authors conducted a foliage season (FS) versus non-foliage season (NFS) comparison that is mentioned in the response letter but does not appear in the revised manuscript. If this analysis was done, its results should either be incorporated as supplementary material or explicitly stated as 'not shown' with a brief description of the finding. Omitting it entirely while mentioning it in the response creates a discrepancy between what was done and what is reported.

Response: We thank the reviewer for pointing out this discrepancy. We agree that the foliage season and non foliage season comparison mentioned in the response to Reviewer 1 should also be clarified in the manuscript. Accordingly, we added a brief statement at the end of Section 4.4 to indicate that this comparison was conducted as an exploratory analysis. We also clarified that the results are not shown because root activity was not directly measured and Rs was not partitioned into autotrophic and heterotrophic components.

“An exploratory comparison between the foliage season and non foliage season showed that the contribution of SWC tended to be more evident during the foliage season and limited during the non foliage season. However, these results are not shown because root activity was not directly measured and Rs was not partitioned into autotrophic and heterotrophic components” [Lines 329–332].

Minor Points

Line 24 (Abstract): 'show a specific temperature range', grammatically incomplete.

Line 183 (Methods): The sentence about 5°C bins now states they were used only as a secondary step for interpretation, which is correct. However, the transition from the full-dataset model to the bin analysis is still abrupt. One linking sentence explaining why bins were chosen over a continuous sliding window approach would improve readability.

Figure 3 caption: 'In 2023, no days had Ts below 0°C, so the Ts bins below 0°C were excluded from the analysis', this is helpful. Consider also noting in the caption the approximate number of observations per bin, at least for the extreme bins, so readers can assess statistical reliability.

References: The addition of earlier foundational references is appreciated. Davidson et al. (1998, GCB) is now cited, which is appropriate. Lloyd and Taylor (1994) is present.

Response: We thank the reviewer for these helpful minor suggestions. We revised the grammatically incomplete sentence in the Abstract and added a linking sentence in the Methods to explain why fixed 5°C Ts bins were used instead of a continuous sliding window approach. We also considered adding the number of observations per Ts bin to the Figure 3 caption. However, because including all bin-specific sample sizes in the caption would reduce readability, we instead added this information in Section 3.3. Finally, we appreciate the reviewer’s positive assessment of the added foundational references, including Davidson et al. (1998) and Lloyd and Taylor (1994).

“Therefore, projections of forest Rs may benefit from considering temperature dependent changes in the contribution of SWC, particularly near the temperature range where this contribution becomes more evident” [Lines 21-23].

“We used fixed 5°C Ts bins rather than a continuous sliding window to provide discrete and interpretable temperature ranges while maintaining sufficient observations within each range for model comparison” [Lines 154-155].

“The numbers of daily observations in each Ts bin were as follows: in 2022, n = 20 for <0°C, n = 78 for 0–5°C, n = 39 for 5–10°C, n = 86 for 10–15°C, n = 56 for 15–20°C, n = 82 for 20–25°C, and n = 24 for 25–30°C; in 2023, n = 74 for 0–5°C, n = 46 for 5–10°C, n = 44 for 10–15°C, n = 67 for 15–20°C, n = 72 for 20–25°C, and n = 31 for 25–30°C” [Liness 213–215].

Final Recommendation

Recommendation: Accept after minor revision.

The remaining issues are primarily of wording precision (breakpoint vs SWC threshold language), structural placement of the phenological notification (Section 4.4), and a discrepancy regarding the foliage season analysis. None of these require new analyses. If the authors address these points carefully, the manuscript will be ready for acceptance.

Priority items for revision:

1. Harmonise language on breakpoint vs SWC threshold throughout (Abstract, Section 3.5, Conclusions).

2. Add explicit notification on autotrophic/heterotrophic partitioning at the end of Section 4.4.

3. Clarify the status of the foliage season/non-foliage season analysis (include as supplementary or state 'not shown').

4. Add one sentence qualifying the residuals interpretation in Section 3.5/Fig. S2. I look forward to reviewing the final revision.

Response: We sincerely thank the reviewer for the positive final recommendation and for indicating that the manuscript may be accepted after minor revision. We have carefully addressed all priority items listed by the reviewer. Specifically, we harmonized the wording related to the Rs–Ts breakpoint and SWC threshold throughout the Abstract, Section 3.5, and Conclusions; added an explicit statement on the limitation associated with the lack of autotrophic and heterotrophic respiration partitioning at the end of Section 4.4; clarified the status of the foliage season and non foliage season analysis; and added a sentence qualifying the interpretation of residual dispersion in Section 3.5. We believe that these revisions have improved the clarity and scientific rigor of the manuscript.

Reviewer #1

The authors have integrated well my comments and concerns, so I do not have more comments regarding the science. However, I encourage the authors to broaden their literature review to better reflect the longstanding understanding of the controls on soil respiration. There appears to be a growing tendency to overlook foundational literature, leading to repeated discussion of concepts that were thoroughly addressed in earlier generations of research. I believe there would be value in reengaging more closely with seminal works, as many of the issues currently discussed have already been examined in depth by previous generations of researchers. Some examples (but not only this):

Lines 58-59. Some of the conclusions presented are already well established in the literature and have been reported long before the recent studies cited. Many researchers, including F. J. Cook, James Irvine, María Almagro, and Ivan Janssens, among others, have previously identified

these processes.

Lines 69-72. Actually, there are studies on this too

Lines 80-85: But see Almagro et al. 2025

Response: We thank the reviewer for this constructive suggestion. We agree that the Introduction should better reflect the longstanding understanding of soil respiration controls. Accordingly, we revised the relevant text to acknowledge that moisture related constraints on soil respiration have long been recognized, added Cook and Orchard (2008), and clarified that previous studies have examined the combined effects of Ts and SWC on Rs. We also added Almagro et al. (2025) to the discussion of breakpoint based interpretation.

“Microbial activity is suppressed under dry conditions, whereas respiration can be limited by oxygen deficiency under wet conditions, a pattern that has long been recognized in studies of soil respiration responses to soil moisture (Cook and Orchard, 2008; Huang et al., 2023). Thus, Ts and SWC are key environmental factors controlling both root and microbial respiration” [Lines 49–52].

“Although previous studies have examined the combined effects of Ts and SWC on Rs, fewer field studies have explicitly quantified how the additional contribution of SWC changes across temperature ranges and whether this pattern is consistent with a breakpoint in the Rs response to Ts (Lai et al., 2012; Cui et al., 2020; Mao et al., 2024)” [Lines 56–59].

“Breakpoint based interpretation may therefore help identify the temperature range where such changes in response become more evident, although breakpoint estimates should be interpreted in relation to vegetation type, climate, and site specific environmental conditions (Carey et al., 2016; Almagro et al., 2025; Li et al., 2025a)” [Lines 75–78].