REVIEW V2 SEO ET AL.,

I thank the authors for their careful and thorough responses to my first-round comments. The manuscript has improved substantially. The most critical concern from the first round, the coupling of the Rs–Ts breakpoint with a formal threshold in SWC sensitivity, has been addressed through revised text and a new supplementary figure (Fig. S2). The contextualisation of the monsoon setting has also been strengthened. Several points, however, remain insufficiently resolved and require further attention before the manuscript can be accepted.

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

Comment 1 — Breakpoint vs SWC threshold conflation

Status: Largely addressed, but residual ambiguity remains.

The authors have moderated their language throughout and added the clarification that the breakpoint is a reference temperature in the Rs–Ts response, not a formal SWC threshold. The new supplementary figure (Fig. S2) showing residuals of the Ts-only model coloured by SWC is a useful addition. However, in several places the revised text still implies a causal link between the breakpoint and the shift in SWC control that is not formally demonstrated. Specifically:

- 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 415–424): 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'.

These are minor but important wording adjustments for a scientifically rigorous manuscript.

Comment 2 — 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 phenologically-driven 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.

Comment 3 — SMC threshold values (10.8% and 13.1%)

Status: Resolved.

The authors clarified that these values represent the minimum of the fitted quadratic function. This is now reproducible. No further action needed.

Comment 4 — Daily averaging and Birch effect

Status: Adequately addressed.

The authors added the relevant clarification to the Methods and Results. The statement that 'Rs tended to increase on the day of rainfall and on the following day' is useful context. Accepted.

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

Comment 6 — Arrhenius curvature as alternative explanation for breakpoint

Status: Addressed.

The authors appropriately moderated the breakpoint interpretation and added Fig. S2. The acknowledgment that 'a change in slope may also reflect the curvature of the temperature response itself' is now present. Accepted.

New comment 1 — 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.

New comment 2 — 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.

New comment 3 — 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.

New comment 4 — 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.

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.

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.