

I thank the authors for their careful consideration of my previous comments and detailed responses. Appendix A in particular is a careful, well-reasoned response to my initial major concern and a very nice addition to the manuscript. I commend the authors for these significant enhancements to their already valuable work. However, I still have two primary concerns along with several specific comments, noted below, before this work is ready for publication.

Primary concerns

1 Breakpoint detection Appendix A reveals substantially lower breakpoint detection rates than the main text suggests. I suggest to move key simulation percentages from Appendix A into sections 3.1 and 3.3, and revise your wording accordingly. Specifically, Appendix A reports:

- Pine (mineral soils): only 9/1000 simulations detected significant breakpoints
- Pine (organic soils): ~30% detection rate (vast majority returned no breakpoints)
- Norway spruce (organic soils): ~45% detection rate (majority found no break-points at all)

Consequently, current main text claims need recalibration. Specifically, section 3.3 states breakpoints were “detected for all examined variables” in spruce, and section 3.1 claims “post-change trends remained robust,” but these statements don’t reflect the low detection frequencies reported in Appendix A. The 2009 pine signal appears driven almost entirely by site-level BAI (n=6 plots?) with minimal regional GI support.

2 Uncertainty propagation into down stream analyses Uncertainty quantification stops at break-point detection. The subsequent segmented regression and dEMM analyses treat the detected break-point year as known (conditioning on the minority of simulations that found breakpoints), ignoring the substantial detection uncertainty documented in Appendix A. The existing Monte Carlo framework should propagate this uncertainty through the full pipeline rather than fixing the break-point year arbitrarily. This is especially important given my above comment.

Specific comments

The specific comments below include apparent contradictions between reported findings and presented statistics, missing or incorrect citations, and typographical or content errors in figures.

Line 248 BAI units and the stand-structure confound. Section 2.3.5 defines BAI as $\text{mm}^2 \text{tree}^{-1} \text{yr}^{-1}$, but every subsequent reference is in $\text{m}^2 \text{ha}^{-1} \text{yr}^{-1}$ which is a stand-level quantity. The conversion or scaling is never described, please clarify.

Line 498 “a statistically significant negative slope before 2016 that weakens thereafter.” Table 3 gives $\beta_{\text{pre}} = -0.035$ and $\beta_{\text{post}} - \beta_{\text{pre}} = -0.052$, i.e. $\beta_{\text{post}} = -0.087$ — the negative SPEI effect strengthens, this is also apparent in the bottom left panel of Figure 6. A strengthening negative SPEI slope in spruce apparently refutes the water-limitation interpretation presented and is never discussed.

Line 496-497 “the difference between periods is marginally significant” for spruce BAI * VPD. Table 3 gives $p = 0.310$. The marginal (“.”) flag is apparently on the SPEI interaction, not VPD. $p=0.310$ is well above your chosen significance threshold.

Line 495: “significant changes in intercepts indicate different baselines in BAI and d13C between periods.” For BAI, $\alpha_{\text{post}} - \alpha_{\text{pre}} = 0.033$, $p = 0.520$, only d13C is significant. This statement is a direct contribution to the statistical evidence provided in Table 4. Also, I wonder if you meant to reference Figure 6 here instead of Figure 4.

Figure 6 caption indicates these are “standardized effect sizes” directly contradicts section 2.4.3; I think they are only mean-centered? Importantly, SPEI is standardized by construction (1 unit change means 1SD), complicating direct visual and quantitative comparisons with VPD (1 unit change is 1 kPa and is roughly an order of magnitude larger than the interannual variability). Therefore comparing VPD and SPEI effect sizes is challenging. This figure’s caption also says blue/red points while the new version of the figure uses blue/orange instead. Figure 6 is also missing the A,B,C, D sub-panel labels referenced in the caption & main text.

Line 622: Norway spruce showed more stable d13C values over time than Scots pine, with the latter showing declining values.” Section 3.3 reports significant d13C breakpoints (shifting to negative trends) only in spruce, and none in pine. Please reconcile these two statements.

Line 137–139: I am unfamiliar with the CRU TS 4.08 data product, but a quick internet search tells me it is a monthly product, not a daily one (<https://www.nature.com/articles/s41597-020-0453-3>). This should be corrected in the text.

Figure 1 caption covers 1980–2021 while section 2.1.2 states the same data were obtained for 1990–2023. Please clarify / reconcile.

Isotope record start date Figure 6’s caption implies d13C begins in 1999, and Figs. 3C–D and A1 start at 1999–2000, but the Methods never state this (maybe I am missing this). Please state the isotope period explicitly in the text. Also, could you please clarify (at least in a response to this comment) why Fig. A1/A3 begin in 1999 while Fig. 2 GI begins in 1990.

Study period is variously defined as either being 1990–2022 (abstract, Figs. 2–3), 1990–2023 (Fig. 4 title, LMM post-period). Please correct throughout.

Missing references: Girardin et al. 2023 (cited line 39) and Zignol et al. 2025 (cited line 640) do not appear in the reference list.

A reference in the bibliography for Anderegg et al. (2018) is listed as “Science, In review”; it was published in Nature according to the linked DOI.

“A. Lehtonen, Eyvindson et al., 2023” and “Leppä, Hökkä, et al., 2020; Leppä, Korkiakoski, et al., 2020” (line 683) should probably follow the 2023b / 2020a,b convention used elsewhere; “H. M. Henttonen et al., 2024” and “K. T. Korhonen et al., 2024” carry unnecessary initials. Whatever convention is chosen it is helpful to remain consistent throughout the text.

I think the site-name spelling in Figure 5 “Vastämaki” may be misspelled (e.g. from the Methods there is “Vastamäki”).

Figure 3 y-axis gives iWUE as $\mu\text{mol mmol}^{-1}$; the text uses $\mu\text{mol mol}^{-1}$