

The manuscript *"Tales from the past: remapping dynamic tree- and forest lines in response to changing climate and current land use"* provides a valuable analysis of ecotone shifts. The authors effectively demonstrate an advancement of approximately 0.5 m/yr, linked to an extended growing season (specifically, mean temperatures exceeding 6.5 °C).

What I really enjoyed about the study is the thorough statistical analyses, that the authors perform to verify their claims, complemented by a transparent discussion section that meticulously addresses methodological rationale and study limitations.

When examining Figure 5, the central graph in the manuscript, it seems the coefficient of determination ( $R^2=0.20$ ) for the growing season length (mean temperature >6.5 degree C) is relatively low compared to what is typically expected in hydrological studies. However, when weighed against the statistical significance demonstrated in Table 2, these findings still provide a credible level of confidence in the underlying temperature-treeline relationship.

Furthermore, given the extreme scarcity of long-term in-situ historical records, creatively analyzing this data to yield insights into temperature-driven treeline shifts significantly adds to our understanding of ecotone shifts.

Only point for me requiring further clarification pertains to the confounding effect of observation duration and spatial positioning, given that the highest rates of change occur in the longest and northernmost record (JMN, 130 years) while shorter records (such as BAA, 50 years) are situated in the southeast.

This raises two questions for me:

1. To confirm that the higher rates of change in longer records are not an artifact of observation window length, could the authors perform additional analyses using a standardized record length (e.g., truncating all records to a uniform 50-year period, where possible)?
2. Currently, only aspect favorability carries spatial information; could the authors include latitude as a covariate in the model to ensure that macro-geographic gradients aren't confounding the local aspect signal?

These are just suggestions from my side, but please feel free to address these confounding issues using whichever approach the authors find best. Also, if running these additional analyses is not feasible, I suggest addressing these constraints explicitly as limitations in the text.

Moreover, I wholeheartedly recommend the editor to publish this manuscript in the journal provided adequate clarification of the issues raised.

Cheers,  
Prajwal