

The authors present a substantially improved manuscript. They have mostly addressed my concerns about over-interpretation of non-statistically significant results and reworded accordingly. They have reframed the plant inputs mechanism as a hypothesis, acknowledged the contradiction with Ofiti et al.'s root biomass data, addressed it with the Riley et al. (2025) modeling, and added discussion of soil moisture. However, two main issues remain:

- 1) The authors now included SI table S14 to add statistical support for the PCA-based claims of changes in bulk soil composition, but the table shows that all AUC bond type for bulk soil are $p > 0.1$ (with the exception of one $p=0.071$) for both Treatment and Treatment $\times$ Depth. Thus, there is no statistical support for the Section 3.3 (L315-316) claim that "subsoils below 40 cm tended to shift... towards C-H aromatic (range 1) and lignin-like residues" under warming. The Discussion section 4.1.2 then builds interpretation on top of that unsupported trend.
- 2) The MAOM composition section (4.2.3) still makes interpretive claims — "largely unchanged chemical composition" and "on average increased DSI" — where the stated stats are non-significant ($p > 0.1$). The issue I originally flagged (describing directionality from non-significant results as if it supports a conclusion) remains in this section.

Thanks for the constructive comment. We have rephrase the title of this section and be more conservative in interpreting the trend.

A technical note that in-text reference errors need to be fixed. L229. I believe the acronym DSI should be introduced here, since it is used in the following section without being defined.

Thanks for the really helpful comment. We have added the acronym behind DRIFTS stability index.