Author's response to comments from Anonymous Referee #2:

"Shifts in global atmospheric oxidant chemistry from land cover change"

by Ryan Vella et al.

We thank editor and referees for taking the time to review our manuscript and for the valuable feedback. Here, the comments from Anonymous Referee #2 (from June 06, 2025) are reproduced in black, while our comments are presented in blue.

From Anonymous Referee #2's response:

Summary

This study investigates how deforestation- and afforestation-induced changes in BVOC and NOx emissions influence OH reactivity, trace gas lifetimes, ozone production sensitivity, and radiative effects from ozone and methane changes. The authors employ a coupled climate-chemistry model (EMAC) with a dynamical vegetation model (LPJ-GUESS). Simulations were conducted for 2000-2011 under three scenarios: 1) a hypothetical no-deforestation case (PNV), 2) a present-day case with deforestation on both cropland and grazing land (DCGL), and 3) a case with deforestation exclusively on cropland, representing an extreme afforestation scenario with food production maintained (DCL). The study is well-designed, well-executed, and well-written in general. The methodology is solid with meticulous model setup and decision-making. The results exhibit a wealth of information, and the authors manage to present them clearly with in-depth and comprehensive discussions. The research question and findings are of novelty and great scientific implications. I recommend publication with only minor revisions needed.

We sincerely thank the reviewer for their positive and encouraging feedback. All suggested revisions have been carefully addressed in the revised manuscript.

Major Comments:

The afforestation part (section 3.2) is surprisingly short compared to the deforestation part (section 3.1). I assume the intent is to avoid repetition since these two effects share great similarities though opposite in sign; however, I'd recommend either moving this DCL scenario entirely to supporting info, or the authors should elaborate more on how much of the deforestation impacts are "reversible" and what impacts are "irreversible".

Indeed, the reforestation section was kept deliberately brief to avoid repetition. We have now expanded Section 3.2 to include a more detailed description of these changes. Additionally, a new paragraph was added to the Discussion highlighting the largely linear response of BVOC emissions to deforestation and afforestation, contrasted with the more complex and partially irreversible impacts on O_3.

There is an excess use of acronyms throughout the manuscript. I don't think it is worth introducing acronyms if they are only used once or twice, such as DGVM (line 63), PPFD (line 144), TOA (line 311), etc. Scenario naming is also a bit opaque, as DCL and DCGL are visually similar and not intuitively distinguishable. Renaming may improve clarity.

We fully agree regarding the redundant use of certain acronyms, and these have now been removed from the text. While we acknowledge that clearer acronyms could have been selected, we have chosen to retain the current ones for our scenarios to maintain consistency. Throughout the manuscript, we consistently remind the reader that DCGL−PNV corresponds to the deforestation scenario, and DCL−DCGL to the reforestation scenario. Moreover, these acronyms were also employed in Vella et al. (2025), and preserving them ensures alignment with that previous work.

Minor Comments:

Line 270: Previously you mentioned "changes in canopy densities changes dry deposition O3 fluxes", but this O3 deposition flux decrease (1.5%) seems proportional to overall O3 decrease (1.6%)? Did you mean that spatial redistribution, rather than total flux, is the key point?

Thank you for your observation. You are correct that the global total O_3 deposition flux changes appear relatively proportional to the overall O_3 concentration changes, with a decrease of 1.52% (DCGL–PNV) in deposition flux aligning closely with a 1.6% decrease in O_3 concentrations. However, our main point was not the total change in deposition burden, but rather the spatial redistribution of O_3 deposition driven by changes in vegetation distribution.

To clarify, we conducted an additional set of simulations with fixed LAI across scenarios for the dry deposition calculations. As shown in Fig. S1 below, the spatial variation in O_3 dry deposition flux is much weaker compared to the case including LAI changes (Fig. S10).

We found that the global annual O_3 deposition burden differs only marginally between the LAI-varying and LAI-fixed simulations. This suggests that LAI changes are not the primary driver of global O_3 deposition reduction, but they do influence *where* deposition occurs. In particular, spatial redistribution of deposition is more pronounced when LAI changes are included, especially in regions experiencing substantial shifts in vegetation.

We have clarified this point in the revised manuscript by emphasising that the spatial redistribution of O_3 dry deposition, rather than the total global burden, is the key outcome from perturbing vegetation cover.

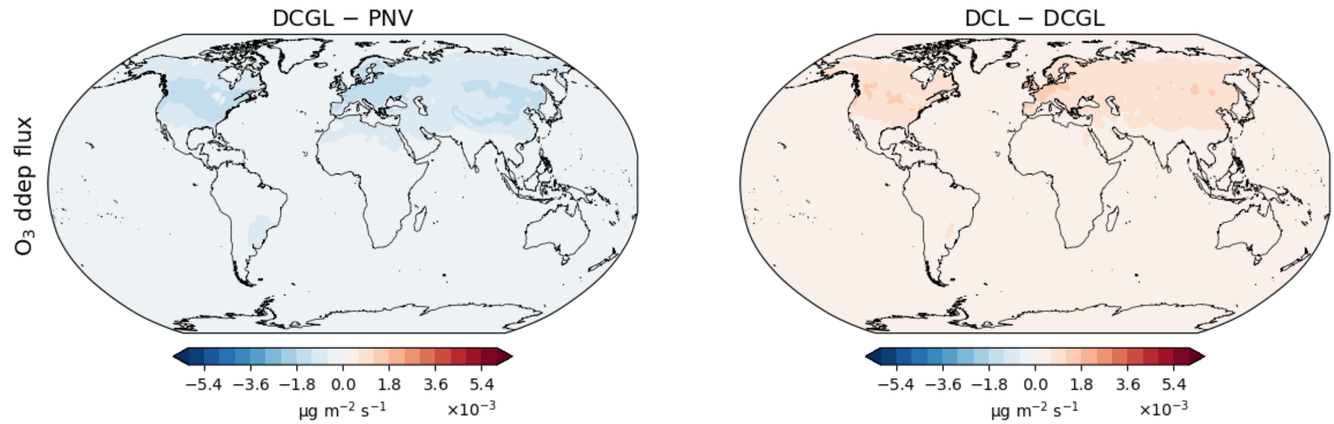

Figure S1. O_3 dry deposition flux changes with the same LAI for the ddep calculation.

Figure 7: I'd recommend adding "VOC-sensitive", "transitional", and "NOx-sensitive" in panel (a) color bar, and adding "<– more VOC-sensitive" and "more NOx-sensitive –>" in panel (b) color bar. It will help the audience to understand the figure even by it alone.

Fig. 7 and Fig.S7 now include labels for "NOx-sensitive", "transitional", and "VOC-sensitive" regimes.

Line 290: Please justify the choice of 0.7 and 0.9 as thresholds for ozone regime classification. Are these based on prior literature or model-specific sensitivity tests?

This method of using 0.7 and 0.9 as thresholds for determining the ozone formation regime based on the $\alpha(CH_3O_2)$ vs. NO curve is introduced here for the first time. It builds upon the approach used in Nussbaumer et al. (2024), where the gradient of the curve was used to assess sensitivity. The choice of 0.7 and 0.9 as thresholds is supported by a comparative analysis with the method from Nussbaumer et al. (2024), demonstrating consistency in identifying ozone formation regimes. Furthermore, sensitivity tests conducted with model data (Fig. 7a) confirm the robustness of this threshold-based approach within the context of our study. However, while the method proves reliable for our analysis, further investigation is needed to evaluate its general applicability. These thresholds should therefore not be considered universally standard, and caution is warranted when applying them in other contexts. This clarification is now included in the revised text.

Line 441: typo: "DCGl" to "DCGL"

Fixed, thank you.

References

Nussbaumer, C. M., Kohl, M., Pozzer, A., Tadic, I., Rohloff, R., Marno, D., Harder, H., Ziereis, H., Zahn, A., Obersteiner, F., Hofzumahaus, A., Fuchs, H., Künstler, C., Brune, W. H., Ryerson, T. B., Peischl, J., Thompson, C. R., Bourgeois, I., Lelieveld, J., and Fischer, H.: Ozone Formation Sensitivity to Precursors and Lightning in the Tropical Troposphere Based on Airborne Observations, Journal of Geophysical Research: Atmospheres, 129, e2024JD041 168, https://doi.org/10.1029/2024JD041168, 2024.

Vella, R., Forrest, M., Pozzer, A., Tsimpidi, A. P., Hickler, T., Lelieveld, J., and Tost, H.: Influence of Land Cover Change on Atmospheric Organic Gases, Aerosols, and Radiative Effects, Atmospheric Chemistry and Physics, 25, 243–262, https://doi.org/10.5194/acp-25-243-2025, 2025.