Thanks to the authors for addressing all the reviewer comments. I have a few additional comments, which relate to new material which has been introduced at the suggestion of the reviewers. Subject to these new comments being addressed, I am happy for the manuscript to be accepted.

Figure 6:
I still think this figure is a bit confusing. The main text around line 353 says we have a new example where we harvest a broadleaf forest and plant an evergreen forest. This gives you the profile of RF in the solid blue line in fig 6. That is clear. However, the introduction of the red curves in the text is less clear. The dashed red is the sum of the RF over time after a 1kg pulse emissions of CO2. What exactly is the solid red line, i.e. what change in albedo is causing it? Can you explain how the two red curves relate to each other / why you are including them?

OK, we have provided additional explanation in Figure 6's caption about the two red curves plotted in panel A and why they are shown.

Section 6:
The equation for GWP* in Lee et al is the version from Cain et al (2019), but Lee et al say that (what they call) alpha is assumed to be zero for their case. Where you apply GWP* in fig 7, as you have a full time series, I think you can assume that alpha is not zero. You could then use the full equation which accounts for the average RF over the period Delta-t. As this accounts for the slower climate response to past changes to RF, perhaps GWP* will have better agreement to Delta T in fig 7b. The equation in Cain et al tried to improve on the Allen et al 2018 equation to have a better agreement with temperature, so it may do so in your example and I think it's worth testing. If that isn't possible, then I think you need to say that you haven't used the extra term in Cain et al (and why) and discuss whether you think it would improve the agreement with temperature (or not). You may also want to then amend your discussion around line 581 related to GWP*.

OK, we have invested notable effort here to demonstrating the faithfulness by which the GWP* approach reproduces the temperature response (revised Figure 7) for a range of time step sizes ("Delta-t") and "alpha" factors applied to the same widely divergent RF scenarios as used in the previous version of Figure 7 A. Although not easy, we believe we have been able to strike a good balance between adding new content which serves to further elevate the manuscript's scientific value while maintaining an orderly and logical flow. We feel that any additional elaboration on the GWP* measure at this point would begin to extend well beyond the current manuscript scoping.

Regarding the choice of time horizon in GWP* - the authors of GWP* use H=100 years and say that:

'In defining CO2-e and CO2-e* emissions, we use H=100 years following established practice. Results under GWP* are insensitive to this provided H is much greater than the lifetime of the SLCP because the absolute GWP of an SLCP becomes a constant at these timescales, while the AGWPH of the reference gas, CO2, increases linearly with H—see ref. 3 and Fig. 8.29 of ref. 14 Hence the H-dependence cancels out in the calculation of CO2-e* for both SCLP emissions and radiative forcing. In contrast, GWP-based CO2-e values for SLCPs scale approximately with 1/H, making the nominal relative importance of SLCPs and cumulative pollutants acutely sensitive to this choice of time-horizon.' (Allen et al 2018)

So I don't think that GWP* uses a subjective choice of time horizon like standard GWP100 does, as you have said around line 447, and suggest that you discuss what is in the paragraph I have quoted instead.

This is a fair comment and we have now revised the content as suggested and expanded Section 6 to provide a more up-to-date and balanced review of the state of the GWP* measure.