In general, I liked the work. It is well written, it is clear, it is easy to follow and from the beginning, with Table 1 and Figure 1, the differences between the treatments are very clear, which is essential to be able to follow the results obtained later. Moreover, I agree with the authors that quantifying carbon fluxes during the initial years after fire is therefore crucial for estimating the net impact of wildfires on the carbon budget.

My main concern is that most of the conclusions of the paper were already given in a previous paper by the authors (Kelly et al. 2021) in the same sites but only one year after the fire, not four as in this case. Moreover, there is also another paper (Kelly et al. 2024) also carried out in the same fire in the same fire severities and forest management strategies 1–4 years post-fire, but in this case using eddy covariance flux towers. Therefore, my main question, given that the variations over the four years are generally not relevant to the conclusions obtained, is what does this work contribute compared to the other two mentioned above?

I also have some minor comments and doubts throughout the work:

- The recovery of the vegetation and the regeneration of the pine is added at the end without having anything to do directly with the title (which talks about soil carbon fluxes) or with the rest of the results. If it does not relate better (and not only indirectly in the end because vegetation breathes) it should not be maintained.
- In the introduction you say that the Scots pine is adapted to resist fire. I'm not sure how, it doesn't sprout, it doesn't have serotine cones, the crowns don't allow the fire to pass through without burning them. I would not make this statement.
- The design of the study is very good, with plots of different severity, and whether or not the trees are maintained. But then subsequent treatments can make the interpretation of the results difficult, both seedling planting and especially soil scarification in some cases. This issue should be discussed as a limitation of the study.
- In general, there is not a very clear temporal pattern through the four years of study, but in some cases significant increases are seen (such as that of Rff in UM in 2022 or that of SLM from 2020 to 2021) that are not fully explained and that confuse the results. The value of SWC for SLM compared to the other plots is also not evident.
- It is not clear to me that there is a need for a conclusions section like the one you currently have. Possibly some comments on the limitations found and the implications of these results on wildfire consequences on carbon fluxes would be more interesting.