Response to reviewer 1:

Thank you for taking the time to read our manuscript. Your comments have greatly improved the quality of the text. We have provided a point-by-point response (in blue) to your comments (in black):

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.

Thank you for your positive feedback, we appreciate it.

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?

This work contributes two major new points compared to the previous two papers at the Ljusdal fire sites reviewer 1 has referred to:

1. The introduction and analysis of soil fluxes at a new site (SLM) that allowed us, for the first time, to explicitly assess the impacts of salvage-logging after a low-severity fire compared to leaving the living trees standing on the soil carbon fluxes. This was not possible in Kelly et al. (2021), since we did not collect data from the SLM site in the first year after the fire. Although the SLM site was featured in the eddy covariance flux tower paper (Kelly et al., 2024), that analysis compared ecosystem flux data at two burnt stands. In the present study, we compare soil flux data at five burnt and unburnt stands with different post-forest management approaches. The inclusion of the SLM site is one of the most interesting and important aspects of the current manuscript. Low-severity fires are the most common type of fire in the Eurasian boreal forest and thus it is vital to understand how salvage-logging of live after these fires (a common post-fire management approach in Scandinavia) affects the ecosystem carbon balance.
2. The Kelly et al. (2021) paper only presented one year of soil flux data from our sites in the first year after the fire. It was therefore only a snapshot of how the sites were recovering after the fire. By presenting a time series of four years of data in the present manuscript, we could confirm our previous findings and highlight how long it is taking the sites to recover, since no major changes in the fluxes have occurred since the first year post-fire. The recovery time of a forest after natural and/or human-induced disturbance is a highly topical issue, and we see this study as an important contribution to the debate. We will emphasize this point in the revised manuscript by adding the following new section to the discussion:

“4.4 No recovery of R_{ff} four years after fire

By the fourth year after the fire, R_{ff} and total understory vegetation cover was still substantially lower at all the burnt sites compared to the unburnt site. These differences were largest after high-severity fire and/or salvage-logging. None of the site groups we tested showed positive trends in R_{ff} over time since the fire, indicating it may take many more years until R_{ff} recovers to pre-fire levels. Parro et al. (2019) found no significant difference in R_{ff} between 5 or 21 years after fire in Estonian Pinus sylvestris forests on sandy soils similar to our sites, and suggest that two decades may not provide sufficient time for R_{ff} to recover in such low fertility sites. Similarly, in their review of boreal forest R_{ff} fluxes after fire, Ribeiro-Kumara et al. (2020) found that it took between 10 to 30 years for R_{ff} to recover after fire. Since we have shown that tree respiration is such an important driver of R_{ff}, the recovery time of R_{ff} will likely be tightly coupled to the time it takes for trees to regrow or recover from fire-related injuries, which in turn is linked to how the sites were managed after the fire (salvage-logged versus unlogged, planted seedlings or seeds sown).”

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.

Good point, we will remove this section in the revised manuscript.

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.

Rogers et al. (2015) describes Scots pine as “resisting” fire because it is adapted to survive low-severity fires and prevent them from spreading into the tree canopy. Adaptations include thick bark and loss of lower branches. Trees that sprout and have serotine cones are called ‘fire embracers’ by Rogers et al. (2015) since they need fire to reproduce.

We will change the wording in the introduction to clarify this point as follows:

“In boreal Eurasia, forests include tree species such as larch and Scots pine that are adapted to survive low-severity fire and prevent it from spreading into the forest canopy (Rogers et al. 2015).”

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.

Thank you for your positive feedback. We agree that the difference in the post-fire management treatments between some of the sites is not ideal. However, as already explained in the methods, we do not compare the SHM and SLM sites because of these differences in their post-fire management. In the revised version of the manuscript we will add a 'Limitations' section to the discussion that clarifies this point:

"4.6 Limitations

Our study is based on an opportunistic design as we could not control nor influence the wildfire or the post-fire forest treatments. The wildfire burnt the study sites at different severities and the private owners of those sites independently decided which post-fire management approach to follow. Despite the inherent limitations of such a design, it did offer a unique opportunity as the UM, LM, HM, and SHM sites were all comparable and within less within less than 1000 m of each other. However, it was not possible to analyse the interaction between fire severity and salvage-logging because the SHM and SLM sites were treated differently after they were salvage-logged (i.e. pine seedlings planted at SHM versus soil scarification and spreading of pine seeds at SLM). We also did not have an unburnt clear-cut site that would have made a full factorial design and allowed us to separate the effects of the salvage-logging and the fire. Future work at other sites should investigate these effects, their interaction and should compare burnt sites with and without soil scarification to better distinguish between the effects of the fire and post-fire treatment of the soil on the forest recovery."

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.

We agree that the high R_{ff} in UM in 2022 is confusing. However, we have looked through our photos from the collars, the timing of the measurements, the raw data from the gas analyser and the soil moisture and soil temperature data and cannot find any errors and abnormalities in the measurements. We have therefore decided to keep these measurements in the analysis. Our decision is already explained in the results section 3.1.

"R_{ff} at UM was much higher in 2022 compared to previous years. The high R_{ff} values at UM in 2022 were driven by a few measurements of very high R_{ff} in August 2022. We could not find any fault with the measurements and therefore retained them in the analysis."

Reviewer 1 refers to "significant increases are seen [...] SLM from 2020 to 2021", which we believe refers to the decline in the CH_4 uptake at SLM (there is no change in SLM R_{ff} during that period). In the revised manuscript, we will add the following text to acknowledge this result in the discussion. Unfortunately, we have not been able to identify what is causing this decrease:

"Although CH_4 uptake decreased significantly between 2020 and 2021 at SLM, this change was not related to changes in soil moisture or temperature conditions because they were similar in both years. In addition, there were only weak correlations between CH_4 flux and all the soil chemistry variables. We have not been able to identify the cause of the changes in the CH_4 fluxes over time at the SLM site."

We have also added a new 'Limitations' section to the discussion which includes information on the high SWC at SLM compared to the other sites:

"The SLM site was located 3 km away from the other sites on loamy soil which increased the SWC at that site compared to all the others which were on sandy soils. In addition, furrows with exposed mineral soil at SLM retained more water and thus had higher SWC (data not shown) than all the other sites where the organic layer remained (which when burnt can become hydrophobic and retains less water; Certini, 2005). Although these conditions impaired the comparison between SLM and LM slightly, our analysis of the Rff data and vegetation cover still shows a significant impact of salvage-logging of living trees at SLM."

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.

Biogeosciences requires a conclusions section therefore we will keep this section.

We have also added a new section on the limitations of the study to the discussion as stated in response to a previous comment.