We thank Jean-Batiste Filippi for his time and effort in providing this thorough review of our paper. Below in blue & italic, we elaborate on how we provide a detailed response to all feedback, including our approach to include the feedback to improve the paper.

This manuscript presents a Large-Eddy Simulation (LES) study of the 2021 Santa Coloma de Queralt (SCQ) fire using MicroHH, with the novel inclusion of in-plume radiosonde data for validation. The objective is to examine how pyro-convection modifies near-fire wind patterns and boundary-layer structure, but also the evaluation of Micro-HH and using radio soundings.

The topic is scientifically significant, addressing the mechanisms behind sustained nighttime burning and extreme fire behaviour. The work demonstrates good numerical design and physical interpretation; in particular, the use of radiosonde profiles to validate the plume structure is original and valuable for the field, asn well as comparing it to NWP.

Scientific relevance can therfore be high. The study advances quantitative understanding of fire-induced circulations and provides evidence that frontal inflow, rather than rear-inflow enhancement alone, may governs plume–atmosphere coupling. The use of an LES code not originally developed for wildfire problems shows capability and will interest both fire and boundary-layer communities.

Major remarks:

<u>Scope of "validation".</u> The paper repeatedly refers to validation, but it is not fully clear what is validated—MicroHH as a model, the fire setup, or the specific thermodynamic representation. Maybe just a "Comparison"or investigation. A clearer statement that this is a comparative test against a single radiosonde, not a formal model validation, would help.

The goal of comparing the simulation output against the radiosonde measurements was to focus on validating the thermodynamic structure inside the simulated pyro-convective plume. It was not intended to function as a full model validation of MicroHH.

To improve the clarity of the paper regarding this topic, we will change the terminology to evaluation/comparison, as these terms cover our goal with the radiosonde measurements: to compare how well MicroHH can replicate the radiosounding measurements.

Furthermore, with the following 3 statements in the introduction, methods and methodology, we expect that we have clarified the goal of the comparison within the paper:

1. **Introduction: (L74):** *the first objective of this study is to demonstrate the ability of LES to reproduce in-plume soundings of thermodynamic plume structures during extreme wildfire events.*
2. **Methods (L107-109):** *Consequently, we will use the radiosonde measurements to evaluate both the simulated pyro-convection and the ambient conditions in which the pyro-convection was simulated. For the evaluation, we will focus on comparing the observed and simulated temperature profiles. Furthermore, we will also compare the measured wind speed in the convective plume with the simulated wind speed.*
3. **Results (L212):** *To evaluate the simulated pyro-convection between 19 and 20 UTC, we compared the simulated plume shape and vertical profile of the virtual potential temperature (θ_v) and wind speed (U) with visual observations (Fig. 5) and the in-plume radiosonde (Fig. 6).*

Please note that we added a new evaluation metric: wind speed. An elaborate discussion on the choices in evaluation metrics is provided as a response to the third comment below.

<u>Fire representation.</u> The fire seems to be implemented as a dynamic heat-flux patch maybe with explicit combustion or spread, but it seems very vague or unclear in the current redaction, do you have any isochrones, fuel maps, orography, it appears not, as well as boundary condition, it is perfectly OK, but if it is a somehow idealized fire it should be clearly presented as such. Also this simplification should be justified earlier and clearly separated from coupled fire-atmosphere modelling claims. And if you have, it would be useful to provide basic the actual parameters of the assumed fuel type and flux intensity and to state whether topography was flat or taken from ERA5.

To improve the clarity on the fire representation in our study we will split section 2.2. into two subsections, 2.2.1 & 2.2.2. Section 2.2.1 specifically addresses the fire implementation in MicroHH, while section 2.2.2 serves as a description of the meteorological boundary conditions.

Regarding the fire implementation in section 2.2.1, to clarify the choices we made for our case study of the SCQ fire, we will explicitly state the following:

- We implemented the SCQ fire as a stationary, moon-shaped, constant heat flux patch. Hence, we do not simulate combustion or the spread of the fire.

- As you suggested, we will directly introduce the justification for the stationarity of the implemented fire, which is that: We assume that the movement of the fire is negligible from an atmospheric perspective as the fire rate of spread is significantly lower than the wind speed.
- The dimensions and the moon-shaped form of the heat flux patch are based on observations of the Catalan Fire and Rescue service, including the observed fire perimeters, which are shown in Figure 1a as hourly fire perimeters.
- The heat flux is based on the dominant fuel type (pinus halepensis), ROS, and fuel energy content based on Byram's equation for the FLI (Byram, 1959).
  - This includes a 50% correction for the radiative heat loss and a correction for the fine fuel moisture content (similar, albeit slightly different to Lareau & Clements, 2016)

Boundary conditions. ERA5 forcing and the periodic lateral boundaries may influence inversion height and plume recirculation. A short sensitivity test or discussion (possibly moved from the Appendix) should quantify the expected impact.

We will move figure A1 up to the methods, creating figure 4, which shows the profiles of the virtual potential temperature at all four lateral boundaries of the domain for both the *ref-run* and *fire-run*. Comparing the two simulations shows that there is no impact of the lateral recirculation on the boundary layer structure (including inversion height). Hence, we conclude that the chosen domain size is sufficiently large to prevent any unwanted recirculation of the fire-induced plume.

The analysis of the specific humidity stays in Appendix A (Fig A1). While it does not introduce new insights beyond Figure 4, it confirms the findings regarding the insignificant impact of lateral recirculation due to the periodic boundary conditions.

Physical metrics. Beyond potential temperature, additional diagnostics (e.g., CAPE, wind profile, potential temperature) could strengthen the interpretation of the radiosonde comparison and the discussion of plume dynamics.

We agree with your suggestion, those measurements could strengthen the interpretation of the radiosonde comparison. Below we elaborate per parameter why we will or will not include them in the

- **Cape:** is possible when we have a full environmental profile, but that one misses essential data on the near-surface properties of the atmosphere (Fig. 6a).

- **Wind speed profile:** We think this is a great suggestion, and we will add a comparison between the simulated in-plume wind speed and the observed in-plume wind speed. We do not compare the ambient wind speed profile because the measurements during the descent were taken at a negative vertical velocity of 8 to 9 m/s, rendering them unreliable for the comparison.
- **Specific humidity:** Although reliable, it is not of big interest to our case study. During our period of interest, 19 to 20 UTC, a dry convective plume was observed (i.e. no pyro-clouds), and MicroHH also produced a dry convective plume. Hence, the simulation is sufficiently dry. Subsequently, comparing the simulated moisture amount with the observations will not add value to our case study.

To clarify these decisions on what parameters we include, we will add the following statement to section 2.1 after introducing the radiosonde measurements and the extent of the comparison (see comment above): *For the evaluation, we will focus on comparing the observed and simulated temperature profiles. Furthermore, we will also compare the measured wind speed in the convective plume with the simulated wind speed. The measured wind speed during the descent of the radiosonde in the surroundings of the plume and the measured specific humidity are not used for the comparison. We consider the wind speed measurements during descent unreliable due to the high descent speed of the radiosonde (8 – 9 m s^{-1}). The specific humidity measurements, on the other hand, are reliable but redundant for evaluating our simulation, as both observations and the simulation indicate a dry convective plume (i.e., no pyro-clouds). Hence, the simulation was sufficiently dry.*

Figures. Some figures (e.g., Fig. 3–5) would benefit from clearer units and labels—particularly for "normalised flux", velocities (m s^{-1}), and altitude scales.
Thanks for pointing out the unclarity in the units. Below we discuss our improvements per figure(s):

- **Fig 3. (Fig 2 in the revised manuscript):** We recognise the mistake with the units; a normalised unit should not have a unit. However, considering also the feedback from Anonymous reviewer 1, We will replace the normalised heat flux with the actual heat flux in kW/m2 to match the units used throughout the text. Additionally, we will rescale the ratio of the axis to better represent the actual distance. (The x-axis represents a longer spatial distance in km than the y-axis, but currently is equally long as the y-axis in the figure, which hinders effective interpretation.)
- **Fig 2,4, and 5 (3-6 in the new manuscript):** To improve the clarity on the 'Altitude', we will replace the 'Altitude' label on the y-axis with 'Altitude AGL' to refer to the altitude above ground level (AGL). Additionally, we will add a clarification in the

methodology (section 2.2.1) about how elevation works in our simulation. We do not account for local topography, treating the terrain as a flat plain. However, we do consider the overall elevation of the region where the SCQ fire occurred using the ERA5 pressure field. Hence, the altitude in the simulation is equal to the altitude above ground level.

Minor issues

– Define clearly "frontal inflow" and "rear inflow" on first use.

We modified L45 and L48 to include an explicit definition of the rear and frontal inflow. Please note that we added Lareau & Clements (2017) as an additional source at the end of L48, per a suggestion from another reviewer.

Old:

L45 : Despite the variability, the LES studies agree with the theory proposed by Potter (2012b) that wildfires can accelerate the upwind inflow (Coen et al., 2013; Peace et al., 2016; Filippi et al., 2018).

L46-?...: In addition to the accelerated upwind inflow, the simulations by Coen et al. (2013) and Peace et al. (2016) show that wildfires can also modify the downwind airflow, creating frontal inflow, a feature of pyro-convection regularly used operationally to set backing fires.

New:

L46: Despite the variability, the LES studies (Coen et al., 2013; Peace et al., 2016; Filippi et al., 2018) agree with the theory proposed by Potter (2012b) that wildfires can accelerate the rear inflow, defined as the entrainment of air into the plume from behind the flaming zone.

L48-52?: In addition to the accelerated upwind inflow, the simulations by Coen et al. (2013) and Peace et al. (2016) show that wildfires can also modify the downwind airflow. This creates frontal inflow, where air is entrained into the fire from ahead of the flaming zone. The creation of frontal inflow by pyro-convection is regularly used operationally to set backing fires and matches with Doppler measurements of convective wildfire plumes, which reveal significant frontal inflow into the fire (Banta et al., 1992; Lareau and Clements, 2017; Roberts et al., 2024)

– Clarify whether "ERA5" or "ERA-5" is used consistently.

Thanks for noticing the inconsistency. We chose ERA5 and checked the consistency throughout the paper.

– Proofread for minor grammatical errors and duplicated references.

Overall the manuscript is scientifically sound and offers a significant contribution to the understanding of wildfire-atmosphere coupling. It would merit full review and likely publication after major revisions aimed at clarifying the methodological scope (validation vs. comparison), documenting the fire setup, and tightening figure presentation. Given these strengths and the importance of the dataset, I recommend accepting it for external review

References:

Lareau, N. P. and Clements, C. B.: Environmental Controls on Pyrocumulus and Pyrocumulonimbus Initiation and Development, Atmospheric Chemistry and Physics, 16, 4005–4022, https://doi.org/10.5194/ACP-16-4005-2016, 2016

Byram, G. M.: Combustion of Forest Fuels, in: Forest Fire: Control and Use, edited by K. P. Davis, pp. 61–89, McGraw-Hill, New York, 1959