Roelofs et al 2025 manuscript evaluation

1 – General comments

The manuscript presents a new method of evaluating LES simulations of wildfires by using sounding instruments (it is not explicitly said, but it seems to be a radiosonde carried by a weather balloon).

It is focused on studying the atmospheric circulations caused by pyro-convection and how these circulations impact the thermodynamic structure of the plume.

2 – Errors and typos

Line 71: it should be “(Ribau et al., 2025)” instead of “(Ribau et al.)”.

Line 92: it should be “a sounding was” instead of “a sounding as”.

Line 95: the descending profile is said to be “not shown” but Fig. 5a claims: “The vertical profiles, measured during the descent …”.

Line 112: ERA5 is retrieved at a sub-region not centrered on a single point (you actually show this in Fig A1). Provide the bounding box.

Line 136: should it be $kW/m^2/s$?

Line 169: in Line 95 it is said “between 0.2 and 1.4 km” while in L169 it’s 0.3 and 1.5 km.

Line 411: same issue with the reference of “(Ribau et al.)”.

Figure 3:

There seems to be an error in Figure 3 that states that the variable shown is the normalized heat flux while showing units of $K.m.s^{-1}$. Either show the actual heat flux (most likely in $kW.m^2.s^{-1}$) or show the normalized flux without units. Similarly, in the caption of Fig. 3, the heat flux is in $kW.m^{-2}$, I believe it should be in $kW.m^{-2}.s^{-1}$.

Also, provide the x and y axis of the zoomed-in box.

3 – Detailed comments

It is unclear what trying to be validated. The proposed approach to validate a LES simulation, where the fire is static, with a single sounding instrument seems ambitious. It would be better to talk about “evaluation” here.

The LES simulation is performed on an idealized set of conditions (no topography, no fuel, no microphysics schemes). This should be clearly stated. Furthermore, there is no coupling between the fire propagation (static heat flux) and the atmosphere.

The simulation parameters could be summarized into a single table.

A list of detailed comments can be found below:

Line 16: To be rephrased. A fireline intensity greater than 10 kW/m is just one of the indicators leading to EWE, other indicators include large values of RoS, Spotting, or plume dominated fire, see Table 2 from Tedim et al. 2018.

Line 84: provide the approximate ignition point if available (or add it to Fig 1).

Line 86: reference the work from Castellnou or Ribau when defining oPyroCu.

Line 89: reference the work supporting that expected behaviour.

Line 116: Is the sea breeze captured by the weather station? If so, provide the data and compare it to the ERA5 reanalysis.

Line 126: provide a reference supporting the claim "this is a common challenge…"

Line 133: Provide the stretch factor and the last cell height.

Line 136: Provide the equation for the moon shape. When is the fire started? How long is the heat flux provided?

Figure 3

- It would be clearer if the scale is respected (increasing the x-axis or decreasing the y-axis) to better represent the aspect ratio of the numerical domain.

Figure 4:

- it appears in Fig4d that the radiosonde is launched behind the fire injection area (at x=1.9km while the fire seems to start at 2.1km) while on Fig 1a it appears that the radiosonde is launched at a flank of the fire. What is the explanation?
- Please provide a scale for Figs 4 a,b,c.
- Please provide the Y-Z plan view of the plume and the lateral displacement of the radiosonde in that plane.
- Why is there no over shooting in your simulation?

Line 186: Where is the 30% coming from? What are you comparing?

Figure 5:

- Instead of "environment" use a different word like "ambient conditions" (used in L189 by the way), "undisturbed air", "background conditions" or other one.
- Include the measurement error (standard deviation or other meaningful metric) from the sounding as horizontal bars or shaded error band.

Line 221: There are also downdrafts in the west section, see Fig 6b.

Line 263: The maximum updraft of 32 m/s seems more realistic. Could you add a figure showing the distribution of updrafts during the 19-20 interval? You can also reference in the discussion some plume observations from Lareau et al, Clements et al and their values to compare with your simulation values.

Figure 7:

- How do you calculate the mass flows? Which density are you using?

Lines 243-252: The whole discussion on spot fires seems to be out of scope of this manuscript and the aims of the article.
The spread of spot fires and their coalescence likely depend on their distance to the main fire front:

- if a spot fire appears on the northern part of the front and outside the red square, it will likely spread in a north-east direction (considering the fire is static and there is no interaction between the main fire front and the spot fire).
- If a spot fire appears on the northern part of the front and inside the red square it will likely spread towards the main fire front (negative meridional wind velocity and almost null zonal wind velocity).

Further information is required on the reported spot fires during the SCQ event to make this paragraph useful.

Figure 9:

- The maximum average vertical wind velocity of 6 m/s seems very low considering the relatively high fluxes.

Line 300: Can you provide a reference for this method of calculating the ABL height?

Section 4.1: The discussion could be enhanced by including references to the numerical study from Cunningham (2005) ("Coherent vortical structures in numerical simulations of buoyant plumes from wildland fires"), the Lidar analysis from Lareau and Clements (2017) ("The mean and turbulent properties of a wildfire convective plume") where the inflow is clearly visible in their Fig. 5. The fire-induced circulation ahead of the fire front seems to have been observed experimentally. It seems incoherent to talk here about fire behaviour when the fire is modelled as a static heat flux.

Line 450: this claim seems to be in contradiction with the last paragraph of section 4.2 (lines 380-388). In particular, line 382: "Consequently, there has been insufficient time for the formation of a surface inversion in this case study".