We would like to thank the reviewer for their very helpful comments and critiques that allowed us to improve the manuscript. Below we supplied our responses to the specific comments.

Response to Reviewer#2

For sake of clarity, I think that the comparison would be easier to appreciate if organised by point location (e.g. plume behaviour at the main tower and the short tower) rather that by measurement (e.g. temperature, horizontal velocity, vertical velocity).

Thank you for this comment. Actually, one of the previous versions of the manuscript was organized as suggested, but after looking at the whole paper we decided to reorganize it. There were three *main* reasons for writing the paper as in the current version. First, we wanted to focus separately on the analysis of the thermal and dynamic properties, and the fire progression, since the model treats them in very different ways, and they required different datasets. The flow is resolved at the large eddy scale, the heat transfer is a combination of the resolved advection and very coarsely parameterized radiation, and the rate of spread is fully parameterized, so we wanted therefore to present these components separately. Second, we wanted to present information from both towers in the same section, in order to give some sense of the fire evolution by presenting the state of the fire and the atmosphere at two consecutive times and locations.

Specific comment

123-l12: "A major difficulty in developing realistic wildfire behavior prediction ..." this sentence could be the start of the following paragraph rather than the conclusion of the one it is ending.

Thank you, we agree with this comment. This sentence has been moved as suggested to the beginning of section 2.

125-l14'- "making it", I might be wrong I was expecting "making them "

The sentence has been corrected.

125-l19: "fire front propagation" instead of "fire-atmosphere interaction". The fire-atmosphere interaction is one component of the fire front propagation.

We rephrased the sentence as suggested.

125-l25: "provide details of the 3D plume structure" rather that "recorded the state of the atmosphere "

We rephrased the sentence to emphasize that the dataset provides information about the plume and atmospheric structure.

126-l5: "evaluation" rather than "validation"

The sentence has been corrected.

section2: is that possible to give more information of the location of the sonic anemometer.

The locations of the sonic anemometers have been added to Table 1.

section3: I had difficulty to understand how the heat is release in the WRF atmospheric cell. Is this depending of the values of the local ROS, or is it a constant?

Thank you for pointing that out. We modified the sentence (127-l20) to clarify the way how the heat is computed.

127-l19: "the fuel is assumed to decay exponentially with time". Here, do we need to understand fuel consumption?

Yes. We modified this phrase, which now says that "[..] the amount of fuel remaining is assumed to decrease exponentially with time [...]"

128-l27: The author mentions "no-Fire" runs in the discussion section. Do we know if subgrid turbulent scheme of WRF-LES is validated for a resolution of 10m.

The 10m horizontal resolution used in this study is indeed higher than the 20-100m typically used in atmospheric-type numerical experiments performed for WRF-LES testing/validation (Moeng et al. 2007, Wang et al. 2009, Solomon et al. 2011, Yamaguchi and Feingold 2012). However, as noted by Mirocha et al. (2010) increasing resolution reduces errors associated with SGS paramterizations as the model resolves wider range of smaller scales. Therefore, we believe that even though the WRF SGS scheme probably wasn't explicitly tested at 10m resolution, it should perform better than at coarser resolutions at which WRF-LES was tested.

130:l113-14: Here it is just a comment. The no wind ROS or flank fire is mainly driven by radiative transfer which are not resolved in the SFIRE model. As discussed in the final section of the paper the authors are considering implementing other fire propagation model. It could be interesting to consider a fire model which can transport ROS and Fire Intensity (see Tymstra et al 2010 for a description of the Prometheus fire model of the Canadian forest service). Such quantity can be related to fire radiation and may be used to derive local value of "no-wind ROS".

Thank you for this comment. We think that is a great idea, and we put it into the manuscript (145-l28)

131-l10 and around: the description of the available thermocouple is not clear. May be a list of the available temperature data would be clearer.

To make the ta

131-last sentence: This sentence would fit better in the discussion section.

We removed the sentence since this point is already discussed in later sections.

Section 5-1: Is this really ROS that is described in this section? ROS characterizes the fire front propagation. Here the author shows time series of local temperature around 5m agl. It looks more like residence time and/or arrival time, though it is not temperature at ground level.

Unfortunately, WRF-SFIRE does not resolve the fire affected surface temperature, so we couldn't use it for one-to one comparison between the simulated and observed rate of spread. Therefore we decided to use the 5m temperature as a temperature giving the model a chance to resolve the plume. However, the rate of spread as computed from 2m temperatures was practically identical. See also our response to the comment 134-l1-5 below. In order to make this section more organized we also moved the last paragraph to the next section.

133-l25: suggest revising, "coarse" instead of "course"

The typo has been corrected.

134-l1-5: Is it realistic to compare plume temperature close to the ground? Radiation might still be relatively important, and soot particles present in the plume are heated by the underlying combustion zone. The WRF-SFIRE system is not solving radiative transfer;

therefore the model is likely to underestimate temperature near the ground.

That is a great point. The model lacks the radiative component, which potentially could lead to the underestimation in the temperature near the ground. At the same time though, the updraft, and vertical heat advection is generally underestimated close to the ground since the first model level is located 2m above the ground. The underestimation in the vertical velocity may reduce the vertical heat advection and lead to higher temperatures at the surface, so these two effects may partially cancel out. Nonetheless, the simulated temperature at 2m above the ground should not be treated as realistically resolved. We show them mostly for the sake of completeness, and realize the model limitations in terms of resolving the near surface temperature. Therefore, we start our discussion from the 5/4.5m temperature plot, and we base our rate of spread computation on 5m rather than 2m temperature.

134-l28: consider revising, "and measurement" is "remarkable".

The missing word has been added.

135-l24: consider revising, "the fire" front "thickness"

We changed that part as suggested.

Section 5.3.3: I had difficulty to find information on the fire line ignition location in the WRF grid. As the x,y and z direction are used in this section, you might consider mentioning here the location of the fire ignition line and the fire front propagation direction, or wind direction.

The typo has been corrected.

140-l11: consider revising, "y-z cross section" instead of "x-y cross section "

The typo has been corrected.

140-l11: "x=465 m" is that the x-coordinate of the masts?

Yes it is, and we clarified that in the text.

141-l1: could you consider defining what is the "fire induced circulation"?

We rephrased the sentence and clarified it by removing this ambiguous part.

142-l26: ERR refers in the manuscript to value in MW and MW/m2. (see 141-l20)

Thank you for pointing this out; we corrected these errors.

144-l6: consider revising, "y-direction" instead of "x-direction"

The typo has been corrected.

146-l10 and around: such a statement is may be too general when only considering evaluation of the model on fire propagation over grass. If the objective of WRF-SFIRE is to be operational, situation of forest fire and crown fire have to be considered. In such a case, the fire behaviour is going to be relatively altered and radiation might play a more important role. Capturing the dynamics of the fire front propagation in an inhomogeneous forest of pine trees with a 10 m grid cell and no radiation might be much more challenging.

That is a good point, we commented on that at the end of this paragraph.

149-l15: As mentioned in the manuscript, the fire and plume behaviour near the ground is more affected than higher in the atmosphere. Therefore we can expect the simulated fire to be less realistic near the ignition. I understand here that the evaluation could not be made only on the ST data, as many *instrument failed during the burn, and that most of the available data are coming from the MT. Nevertheless earlier in the manuscript (i.e. in section 6), it would be interesting to have information of the variability of the steady state of the fire when reaching the ST.*

Absolutely, but unfortunately we do not have data that could be used to assess the rate of spread beyond the short tower. Therefore, we don't really know if what we see at the short tower is already a steady state or not. We hope future field experiments will shed more light on that.

Figures:

Figure 10: In the plot, both MT and ST are marked with a diamond, while the legend mentions a triangle for MT.
Figure 14: same as above Thank you for pointing this out, The

caption have been corrected.

References:

Mirocha, J. D., J. K. Lundquist, B. Kosović, 2010: Implementation of a Nonlinear Subfilter Turbulence Stress Model for Large-Eddy Simulation in the Advanced Research WRF Model. *Mon. Wea. Rev.*, **138**, 4212–4228. doi: 10.1175/2010MWR3286.1

Moeng, C. H., and et al., (1996), Simulation of a stratocumulus-topped planetary boundary layer: Intercomparison among different numerical codes, *Bull. Am. Meteorol. Soc.*, **77**, 261–278, doi: 10.1175/1520-0477(1996)077

Solomon, A., M. D.Shupe, P. O. G.Persson, and H.Morrison (2011), Moisture and dynamical interactions maintaining decoupled Arctic mixed-phase stratocumulus in the presence of a humidity inversion, *Atmos. Chem. Phys.*, **11**, 10,127–10,148, doi: 10.5194/acp-11-10127-2011

Wang, H., W. C.Skamarock, and G.Feingold (2009), Evaluation of scalar advection schemes in the Advanced Research WRF model using large-eddy simulations of aerosol-cloud interactions, *Mon. Weather Rev.*, **137**, 2547–2558, doi: 10.1175/2009MWR2820.1

Yamaguchi T., and Feingold G. (2012), Technical note: Large-eddy simulation of cloudy boundary layer with the Advanced Research WRF model, J. Adv. Model. Earth Syst., 4, M09003, doi:10.1029/2012MS000164