General comments:

In this work the authors present the latest evolution of the fire-atmosphere coupled model s WRF-SFIRE. The improvements have been made on: the fire line intensity estimation, the initialization method that can now start with an already mature fire front, the implementation of a moisture model together with its Data Assimilation scheme, the development of GIS tools to process input and output data, and a coupling with the chemistry of WRF-Chem. A section also shows the use of the model in an operational level within the Israeli national fire forecasting system.

The manuscript is well structured and easy to read. However the level of validation of the different new modules added to the WRF-SFIRE system is relatively weak, e.g. the moisture model is validated with only one poorly documented fire scenario, the efficiency of the new fire line intensity is not clearly shown, and if mentioned, no results are shown on the coupling with WRF-Chem which could be of great interest for the fire community.

Another concern is the explanation behind the new estimation of the fire line intensity which I found rather confusing. As stated by the author the byram's Fire line Intensity (FI) I is defined as

I = R H w

where R(m/s) is the Rate Of Spread (ROS) of the fire line, H (J/kg-1) is the heat of combustion, and w(kg/m2) is the fuel consumed per unit area. w is then defined as

w = w0 beta

where w0 (kg/m2) is the initial fuel load, and beta is the fraction of fuel mass burnt during the residence time of the flame. Beta is not a constant, and several model exist to estimate the fuel consumption, see de Groot 2009 or Ottmar 2014. The value of 0.9 that the authors mentioned seems to refer to the combustion efficiency which is a measure of the local fire regime (ie flaming or smoldering). The consumed mass is generally a function of the fine and coarse fuel moistures, bulk density, duff depth, canopy structure and density, degree of decomp, mineral content, …

Rothermel (1972) in his derivation of ROS, defines the intensity reaction as the heat release rate per unit are of the fire front. It has the dimension of $J/m^2/s$. It seems that this is the quantity the authors want to estimate.

In conclusion, I will recommend to clarify section 2, the definition of a fire line intensity which does not have the right dimension is a bit confusing, the addition of more fire scenarios in the validation process of the moisture models would also help to improve the quality of the manuscript.

Specific comments:

Title:

The mention of Israli fire forecast system is not relevant with the content of the manuscript.

Section 1:

P1762 – 5: “However the operational development … number of processors” this does not need to be mentioned here.

Section2:

See above. Furthermore, all along the manuscript terms are used with different name than the one usually found in the literature. For example the Rate of Spread is named “fire spread rate” (e.g. P1763; l9).

P1764-l7: e(-tau/Tf), why tau? The “fuel burn time”, is this the flame residence time?

P1764 – last sentence: fire danger map, fire severity and the use of ROS. This is not really clear how the final product is developed. May be an example of a mapped product could help here, and some more careful explanation on the derivation of this product.

Section 4:

P1766 - first sentence: May be the dependence of ROS to the fuel moisture could be formally developed here, so that it emphasizes the need of a new moisture model.

P1767 – l18&22: 1h, 10h, .. fuels are not defined.

P1768 – Eq 6: mention that M = m(t)

P1768-l16-20: The exponential notation is a bit confusing as it is not present in equation (3). Then a Taylor expansion is used for short time step and the resulting expression is exact for arbitrary large time step. It is a bit confusing.

P1768 – last sentence: the module of WRF are not explained, defined or referenced.

P1769-l29: may be it could be mention here again that the comparison is done 108h after ignition.

Section 5:

P1770-l22: Nk is not fdefined

Section7:

I am not sure this section brings a lot to the manuscript. It is of great interest for future use of WRF-SFIRE, however I think further work would be needed (e.g. a more convincing result that Figure 9) to include it in a peer-reviewed journal.

Section 8:

P1775-l17: NWS is not defined

Figures:

Figure 8: Only 2 simulations are shown here while 3 are reported in the manuscript.

Figure 9 could be improved, we cannot really see anything, it is mainly just a black background. Or it could just be removed (see above).

Reference:

Rothermel, R. C. (1972). A mathematical model for predicting fire spread in wildland fuels. (Research Paper INT-115). Intermountain Forest and Range Experiment Station, Ogden, Utah, 84401.

de Groot W.J., J. Pritchard, T.J. Lynham Forest floor fuel consumption and carbon emissions in Canadian boreal forest firesCan. J. For. Res., 39 (2009), pp. 367–382, 2009

Roger D. Ottmar, Wildland fire emissions, carbon, and climate: Modeling fuel consumption, Forest Ecology and Management, Volume 317, 1 April 2014, Pages 41-50, ISSN 0378-1127