We would like to thank the referees for a thorough review of our manuscript and their comments, which will improve the paper. We would also like to thank the referees for interesting suggestions for future work. Below we describe how we plan to incorporate particular reviewers' comments into the revised version of the paper.

Referee 1

This paper essentially provides a precis on the recent improvements made to the Weather and Research Forecasting-SFIRE modelling system and provides a brief overview of the application of the system to a fire danger forecasting system in Israel. Overall the paper reads satisfactorily but suffers from an assumption that the reader understands many of the acronyms and terms used in the paper from previous WRF-SFIRE papers. Similarly there are also a large number of statements, particularly in the first half, that need to be supported by appropriate references.

We will explain the acronyms and add references.

Additionally, many of the new additions to the system do not provide sufficient information for proper peer-review if they haven't already been reviewed. As a result, the statement 'This paper consolidates for the first time in journal form new developments in the SFIRE software system' worries me greatly if the authors are seeking to validate their work in a peer-review journal in this manner.

This paper is an updated reference, meant to summarize the new developments since the previous reference paper (Mandel et al. 2011) in perspective, and to complement them by some new results. It is not an attempt at an overall validation. We will identify any illustrations (as opposed to validation) as such. Some specific aspects of the model deserve a more focused treatment and in-depth validation on their own. Therefore, in the meantime, two papers specifically on the validation of the fuel moisture model with data assimilation and the coupling with a chemical transport model, respectively, were submitted for publication - Vejmelka et al. (2014) and Kochanski et al. (2014) [in references to this response].

Of considerable concern is the fuel moisture model that is 'tested' against an historical fire for which there is little detailed observation of spread and no direct measurements of fuel moisture content. The performance of the fuel moisture model is then inferred from incomplete data. A simple analysis of the data provides indicates that the fuel moisture model lags considerably behind what would be expected of fine fuel moisture contents driving fire behavior and subsequently the fire behavior lags even further behind what would be expected. The fuel moisture model needs to be properly assessed and validated against complete independent data.

The fuel moisture model used in this is study has been validated against observations from 45 stations, collecting data over a period of two years, and the model statistics is submitted for publication in Vejmelka et al. (2014) [in the references to this response]. Due to the limited

amount of available fuel moisture data available during the simulation period for our model domain, a complete fuel moisture model validation is outside of the scope of this paper. However, we plan to add additional plots showing time series of the simulated and observed 10h fuel moisture for the stations located within our computational domain.

The fuel moisture reported in the paper is the the overall fuel moisture, being an integrated value, consolidating 1h, 10h, 100h and live fuel moisture across the whole fire domain. For each fuel type, contributions from 1h, 10h and 100h fuel classes differ. So for instance for the grass fuel types, indeed the 1h fuel moisture contributes in 100%, however for coarser fuels, as much as 48% comes from 100h fuel moisture. As 1h, 10h and 100h fuels are simulated with progressively increasing time lags, the overall fuel moisture in general is responding to environmental changes slower than the fine fuel moisture. Out of the 13 fuel classes represented in the model, 10 have non-zero contributions from 10h fuel, and 9 from 100h fuel (Albini, 1976, Table 4). Therefore, the integrated fuel moisture content is expected to lag behind the fine fuel moisture. Nonetheless, we will verify if the fuel moisture time scale used in this study is appropriate to realistically model fuel moisture fluctuations. We will also clarify in the text the meaning of the reported fuel moisture.

Once this is done, the fire behavior prediction then needs to be assessed and validated.

We were able to collect one fire perimeter contour showing both branches of the Barker Complex fire, and we compared it to our simulation in Fig. 7. A more complete validation (on a different fire) is planned in future.

The English grammar is proficient but there is frequent occurrence of clumsy or poorly phrased grammar that needs to be revised. Despite the title, there is no mention in the abstract of the Israel fire danger system. One wonders whether the title is overly specific in this case, given that the operational use in Israel is only 40 lines long. Perhaps something along the lines of 'Recent advances and applications of WRF-SFIRE'?

We will change the title as indicated and mention the Israel system in the abstract.

Specific comments:

P1760: L1 Insert 'have' before 'made' L5: 'WRF-SFIRE' needs to be defined. L6: Issue with phrasing here 'fireline intensity' is not a new concept. I think you mean 'a new interpretation of fireline intensity'.

The new fireline intensity is actually different; among other things, it depends also on the speed of burning, and the has different units than Byram's fireline intensity. But this is a more complicated issue, which deserves a separate treatment. We will omit the new fireline intensity and leave it to a separate paper in future.

L20: The small scale processes involved in wildland fire state well before flames appear occurs at the scale of molecules in the thermal degradation chemistry. See Sullivan and Ball (2012) for an overview of the chemistry involved. Also see the review of wildland fire modelling series by Sullivan (2009a,b,c) for discussion of the complexity of the problem.

We will modify this part to note the molecular scale processes of thermal degradation and add the suggested references.

L21-23: This discussion focuses on the atmospheric component of turbulent processes and ignores the critical ingredient of heat transfer processes to unburnt fuels that drive fire spread.

The heat transfer from the fire directly to unburned fuel is parameterized in the spread rate calculation (Rothermel 1972). Radiative transfer over longer distances is currently not considered, but suitable models can be supported in future. Convective heat transfer through the atmosphere can affect fire spread through increased drying in the fuel moisture model, but his effect is of a lower order.

We tried to focus on fluid dynamics processes as the ones affecting both mixing and turbulent heat transfer. We purposely ignore the radiative processes and conduction within the fuel bed to establish the general context of the interaction between larger-scale and small-scale processes.

L23-25: This explanation of the importance of larger scale weather contributions is rather weak and unconvincing. What is the importance of cascading eddies in regard to fire behavior?

A chemical reaction rate is affected by concentrations of reacting species. In case of the gas phase oxidation, these concentrations are generally affected by local mixing. The oxidation reactions are generally so fast, that their rates are mixing limited. According to Kolmogorov hypothesis, the energy content of the eddies responsible for small-scale mixing is controlled by larger-scale eddies, as the energy propagates from larger to smaller scales. Consequently, any mixing-limited chemical reaction in the atmosphere is ultimately affected by large scale processes providing energy for turbulent mixing.

Change 'lager' to 'larger'.

P1761: L2: Explain here why fuel moisture, driven by atmospheric conditions, is important for what you're doing.

We will move here the first sentence from Section 4 and expand: "Fire spread rate depends strongly on the moisture contents of the fuel, therefore modeling fuel moisture is important." We will incorporate a shorter version of the following paragraph from the introduction of Vejmelka et al (2014): "The behavior of fire is highly sensitive to fuel moisture content (FMC), which is defined as the mass of water per unit oven-dry mass of fuel. Fuel moisture affects the burning process in at least three ways (Nelson 2001) [in references to this response] : it delays ignition,

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decreases fuel consumption and increases particle residence time. With increasing fuel moisture content, the spread rate decreases, and eventually, at the extinction moisture level, the fire does not propagate at all (Pyne et al. 1996). The moisture content depends on fuel properties and on atmospheric conditions. The fuel moisture content of live fuels exhibits predominantly a seasonal variation driven by physiological regulatory processes. In contrast, the fuel moisture content of dead fuels is influenced by a variety of weather phenomena such as precipitation, relative humidity, temperature, wind conditions, solar radiation and dew formation. For a recent review on modeling processes affecting fuel moisture in dead fuels, see Matthews (2014)."

We appreciate bringing Matthews (2014) to our attention.

In Rothermel's spread rate model, for example, fire spread rate depends on fuel moisture ratio through the moisture damping coefficient (Rothermel, 1972, Fig. 7).

Diurnal variations in the dead fuel moisture, often disregarded in simulations of short fires, become important in the case of prolonged fires. These fires stay active over periods of days or even weeks, over which fuel moisture conditions can significantly change. Even though one can imagine particular meteorological conditions with negligible daily fluctuations in the temperature, wind, relative humidity and consequently also the fuel moisture, in general, the diurnal variations in the fuel moisture content affect fire activity. For that reason, simulations of multi-day wildland fires, such as presented in this study, require estimates of moisture content changing during the fire event.

L5: The refs here are only two examples of the type: Insert 'e.g.' before 'Linn'.

OK.

L6: Computation time is just as important in regard to operational applicability, not just costs. L9: 'is practically impossible'. You just said on L5 that it was technically feasible. Which is right and clarify what you mean.

We will clarify that such simulations take too long.

L10: Another example of poor phrasing here and throughout: 'allows to' should generally be 'enables' or, in this case, the more active '...model captures a practically. . .'

We will make the suggested change.

L12: Provide a ref for this first sentence. How do winds drive the fire propagation?

Winds drive fire propagation through the acceleration of fire spread. We will modify the sentence similarly as:

"Is has been recognized that the wind speed affects the fire rate of spread (e.g. Rothermel (1972), Albini (1981, 1982), Beer (1991)." [in references added to this response]

L14: In wildland fuels, the key ingredient is in fact carbohydrates, not hydrocarbons.

We will correct this.

L16: What are 'fire storms' and how are they generated? Provides refs. L18: "'it's own weather"

We will add the following sentence to clarify that:
"The fire-induced convection may lead to formation of pyro-cumulus cloud and conflagration strong enough to generate its own wind system (firestorm). See Fromm et al. (2006) and Rosenfeld et al. (2007) [in the references for this response], for an example of a pyro-convective system generating tornadic winds.

Provide a ref. L22: Define 'simple fire spread model' How simple? How complex? Does it matter?

We will remove the world 'simple'.

Again poor phrasing with 'allows to', change to 'captures'. L27.

We will make the suggested changes

Ref for 'level-set method'.

We will insert reference to Osher and Fedkiw (2003) [in references to this response].

L28: Insert 'the' before 'fire component'.

We will correct that.

P1762: L7: Define 'CAWFE'

CAWFE is the Coupled Atmosphere-Wildland Fire Environment. It consists of the Clark-Hall atmospheric model coupled with fire spread implemented by tracers (Clark et al., 2004).

L8 Change to 'fire spread model of Rothermel (1972)'. L9: Change to 'Support for alternative fire spread models (e.g. Balbi)'. L19: Change to 'for the first time various new developments' L20: Insert 'completed' before 'in'. Insert 'by' before 'Mandel' L21: Delete 'scattered in presentations and conference abstracts'. This implies that these "improvements" to SFIRE have not undergone peer-review prior to this submission and raises doubts as to the appropriateness of the process.

We will make these suggested changes.

L24: Is the 'real test case' an attempt at validation or is it just an example?

The validation itself is outside of the scope of this paper (see our response to the third comment on page 1). We present here an example, to illustrate using the fuel moisture model in fire spread simulations.

L26: Does 'GIS' need to be defined?

We will spell out Geographic Information Systems (GIS).

P1763-64: *We will omit the new fireline intensity from this paper, and leave it to a separate paper in future.*

L16-22: What is meant by 'simulated fire severity'? Severity is a measure of impact of a fire and depends on what is being impacted. It is difficult to see the utility of this quantity from this discussion, primarily because it is unclear and confusing. If you're calculating maximum fire spread in any direction, then this doesn't need a fireline as such, is this correct? Does the maximum fire spread consider all combinations of potential fire weather/fuel combinations at a point? If it doesn't then it can't be considered maximum potential rate of spread. Clarify.

Our users requested some measure how hard a fire would be to suppress if there was a fire in a given location. For the lack of a better word, we call this "fire severity". Specifically, they were interested in the spread rate. Since there is no fire yet, we have provided the maximum spread rate in any direction (which indeed does not need a fire line), for the given fuel data and the weather forecast computed by the system. We have also provided the fireline intensity calculated from the maximum spread rate in any direction, and the fire intensity. We do not say that we would consider various possible weather/fuel combinations, and we indeed do not.

P1765: L13: 'Hopefully'? Why hopefully? L26-27: '. . .the wind at the moment. . .' Do you mean as currently implemented, or the current wind speed driving the rate of spread in the simulation? Unclear.

We will rephrase this sentence:
"Replaying the artificial fire history enables gradual fuel burn and heat release into the atmosphere (the whole inside of the fire perimeter is not ignited at once). We do so, in order to estimate the amount and location of the available fuel, as well as to capture the fire-induced atmospheric circulation at the perimeter time."

P1766: L4: 'Here' Where? In Kondratenko et al 2011? Who's 'we'? Not the authors of Kondratenko et al 2011?

We will replace "Here" by "In this paper". "We" means the authors of this paper.

L10: Delete 'is'. L11: Insert 'an' before 'additional'. L13: What does 'plays the role of truth here' mean? Rephrase.

We will delete "which plays the role of truth here".

L16: Moisture content is generally singular in regard to fuel. Change 'contents' to 'content'. L23: See L16.

We will make the suggested changes.

L24: If this model and its parameters are for dead wood, how applicable is it to determining the moisture content of fine (1 hour) fuels driving the spread of wildland fire? See recent review by Matthews (2014) for a comprehensive coverage of the different fine fuel moisture modelling approaches employed around the world. This method is a gross simplification for the purposes of simulation.

The model is run for 1, 10, and 100 hour fuels. These all consist of dead fuel, just different sizes. It is currently applied to the grass component of 1 hour fuel also, even if that would be better treated as a separate fuel class. It does not apply to live fuel. The method is indeed a simplification, and we will consider further enhancements in future, as supported by available data and peer-reviewed literature.

P1767: L15: Is this calibration appropriate for the conditions being simulated? L19: The values for threshold rain intensity and saturation rain intensity seem rather low. How do they compare with other values using the same model?

We have verified that the fuel model setup provided realistic diurnal fuel moisture variations for 10 hour fuel matching without any visible time lag the 10h fuel moisture observations.

Wouldn't these depend on other climatic variables?

Like everything, this is indeed a simplification. We will consider further enhancements in future, as supported by available data and peer-reviewed literature.

P1768: L2: 'wk' is undefined.

We will explain that w_k is the proportion of fuel class k in the fuel.

L3: Delete 'with'. L4: Change 'contect' to 'content'.

We will do this.

L12: This is not clear. Shouldn't the averages of the state variables over two adjacent time steps be for example (E_n + E_n+1)/2? L15-16: Same as above?
$E_{n+½}$ is defined by averaging, which is indeed $E_{n+½}=(E_n + E_{n+1})/2$. We will add this formula to the existing verbal description, for more clarity.

L21: Why couldn't the model be validated against direct measurements of fuel moisture content, rather than trying to interpret fuel moisture model performance filtered through fire spread simulation (and the huge number of assumptions and unknowns embedded therein)? Fuel moisture content is one of the easiest aspects of wildland fire to study because it doesn't need a fire. This model must be validated using direct FMC measurements.

We agree completely. This was done in the meantime in Vejmelka et al. (2014).

P1769: L11: One of the many unknowns in this case was the precise ignition locations. Another it seems is the final perimeter itself. L22-25: This is pure speculation because you could not compare it against actual fire behavior, only the simulated perimeter that had the diurnal fuel moisture changes fed into it.

We show fire perimeters simulated with constant fuel moisture (two different values) and with the variable fuel moisture estimated by the fuel moisture model coupled with the weather component of the system. We show results from these three numerical experiments overlaid over the observed fire perimeter (green contour in Fig. 3 b). We do not claim that the run with the fuel moisture model captures the actual fire behavior. We claim that it better represents the final fire perimeter and allows to capture diurnal variations in the fire activity.

Figure 8 illustrates some significant problems with the fuel moisture model as implemented. See discussion below on the figure (P1791). L25-26: You haven't shown that the fuel moisture model 'renders' the diurnal variations in fire activity or that it improved the total simulated fire area. You may infer this but you haven't shown it. Over 4 days of fire spread there is plenty of opportunity for over and under predictions to cancel each other out. A very weak test.

We show results from a sensitivity test in which the only parameter changed in the simulation, is the way how the fuel moisture used in fire spread computations is represented. The differences shown in Fig. 7 cannot be attributed to anything else. As discussed earlier we will also present time series of the simulated and observed fuel moisture to show how the model rendered fuel moisture fluctuations.

P1770: L1-4: This is all unvalidated. L16: How are these 10 h fuel FMC values applied to 1 h fuels? It's not clear how these are modified in the simulation.

Fuel moisture is simulated independently in three classes - 1h, 10h and 100h. They are integrated for each fuel type according to relative mass contributions derived from Albini 1976. See also our response to the comment on the first page. Here we can only demonstrate the model's ability to render the 10h fuel moisture since no other fuel moisture data is available within the fire domain.

P1771: Will the results gathered here for the 10 h fuels be much different when applied to 1 hour fuels? The equilibrium times of 10 h fuels mean that they will miss much of the impact of rapid changes in atmospheric conditions such as cold fronts and troughs, etc.

As stated above, the 10h fuel moisture is not applied to 1h fuel. Each is computed separately, so that each fuel class responds to the atmospheric conditions at a different time scale. Because of shared adjustment parameters for the equilibrium, changes made by data assimilation to the 10h fuel equilibrium are transferred to 1h and 100h fuel equilibria as well, even if approaching the equilibria is still determined by the respective 1h and 100h time lags.

P1774: L9-11: Doesn't the chemical emissions of a fire depend on the intensity of the fire in question? Are the emission factors given only fuel-type specific?

The emissions are fuel specific and defined per mass of burnt fuel, so the fuel consumption rate (mass/unit time) is used for computation of the emission fluxes. This is admittedly a simplification in the current version. We will consider supporting emission factors dependent on fire intensity and other fire characteristics in future, as much as it can be justified by available data and peer-reviewed literature.

P1775: L4-10: How is this validated?

The aerosol formation has not been validated yet. So far the simulated PM2.5, NO, Ozone and plume height has been tested against observations, see Kochanski et al. (2014).

L13: Where are the fire danger maps given in Section 2? In this context, how is fire danger defined? It appears all you are considering is potential fire behavior but this does not tell you anything about potential for fires to break out, their potential to spread, or their potential to do damage, which is what most definitions of fire danger incorporate. See Chandler et al 1983.

We'll replace this by "potential fire behavior".

L14: Where are the GIS interfaces given in Section 6?

We'll replace this by "GIS data sources and conversions".

 L17: Define NWS.

National Weather Service.

L19: How are the forecasts downscaled?

The forecasts are dynamically downscaled by nesting 444m grid within the 1.33km one, similarly as shown in Fig. 7a.

L21-22: What are the high-resolution forecasts of fires based on?

The high resolution forecast of the fires are based on the high-resolution local weather forecasts.

Potential fire behavior? How is this defined? Worst possible given the coincident forecast weather or the worst possible given the potential combinations of forecast weather?

We will clarify "potential fire behavior" as the fire behavior if there was a fire, given the forecast weather, fuel and topography data, and forecast fuel moisture. We will clarify "worst possible" as using the largest spread-rate in any direction.

P1776: L1: Insert 'via a web interface' after 'interactively'. L10: 'output every 24 h'. Unclear here. Do you mean the moisture model is run once a day or every operational 1.333 km system's output for a period of 24 hours?

The moisture model with time step 1h is run once day from a 24h forecast at 1333m resolution.

L17: Change 'air support' to 'aerial suppression'. L27: If worst case what is the value of fuel moisture used?

We will replace "worst case" by a reference to the largest rate of spread in any direction.

L29: What about areas outside of these landmarks? Are people expected to interpolate between these values? If so, is this meaningful?

The code provides values at all nodes of the grid. Our users were interested in values at several select points only.

How are these predictions of 'fire danger' validated?

The validation of the behavior of a potential fire is the same as for the fire forecast itself.

P1777: How applicable to the fuels of Israel is the Rothermel model and its operational fuel models?

The system supports rate of spread correction factors in order to enable applying the modified rates of spread as suggested by Carmel et al. (2009) [in references to this response] (Fuel model 1 and 4 suppressed by a factor 2, fuel model 10 suppressed by a factor of 4)

L5: 'is the only one running operationally'. Do you mean in Israel or around the world? It is not the only fire danger system running operationally in the world.

As far as we know, WRF-SFIRE is the only coupled atmosphere-fire system running operationally.

L12: Change to 'Mandel et al. (2009, 2011a).

The citations are generated by the Copernicus BibTeX and LaTeX style files, which are out of our control.

L12-14: This is not a sentence. L15:Phrasing. 'allows now for modelling fire spread,'. It did this before. Insert 'dynamic' after 'account'.

We will make these changes.

P1778: L1: What do you mean 'renders' smoke?

We will change it to:
The updated version simulates also fire smoke. Depending on the users' requirements, smoke can be simply treated as a passive tracer advected...

L2: Change 'it' to 'smoke'. L5: Change to 'not only allows the study of'. L10: PhrasingâA˘T˘'rendering' L16: Phrasing –'allowing for' L17: Change 'the ignitions' to 'incorporating ignitions'.

We will make the suggested corrections.

What do you mean by 'arbitrary'? As I understand it, it's not arbitrary but a fire perimeter at a given time.

We will clarify this sentence:
The original ignition mechanism allowing for point and line ignitions has been to support igniting fires from user defined perimeters (e.g., remotely sensed).

Phrasing 'allows for'. L18: Change 'a need for' to 'the need to'. L21: What does 'selected other components of the Earth system' mean? Clarify.

We list them already: fuel moisture and chemistry models. Coupling Earth science modeling softwares is the goal of many projects, for example, the Earth System Modeling Framework (ESMF).

L24: Ultimate you have not quantified fire danger as a whole but only one aspect of it is potential rate of spread. Without potential for fire occurrence and potential for damage, this is not an assessment of fire danger.

We will omit references to fire danger.

P1779: L1: What other coupled fire-atmosphere models have been implemented operationally if WRF-SFIRE is 'one of the first'?

Just being cautious. We are not aware of any. We will replace by "apparently the first".

P1786 (Figure 3): The vertical axis label for 3b appears to read 'Idnition'. What do the colors mean on the right vertical axis? There are numbers but no axis title.

We will explain that the false color is the same as the vertical axis. This is how MATLAB does it, for easier 3D visualization.

Where are the two peaks in (b)? It is difficult to see these. Perhaps highlight them.

We will add arrows pointing to the peaks.

P1787 (Figure 4): What is 'tign' from the title for 4b?

Time of ignition. This will be changed to "fire arrival time".

Why are the times for all the figures in UTC? If the objective is for an operational tool, output should be in local time so direct comparisons can be made against measurements in the field, particular those of fire behavior and perimeter location.

Time is specified in UTC in WRF. UTC is more natural for scientific studies, particularly with simulation domains spanning multiple time zones. We will use local time for figures from operational application, but then some figures will be in UTC and some in local time.

Can't you quantify how similar or different the two wind fields in a) and b) are? 'quite close' is highly subjective and not very informative.

We will calculate the difference in standard norms. We will also show the difference of the wind vectors. Here is the preliminary figure:

Maximum wind difference is 11 m/sec
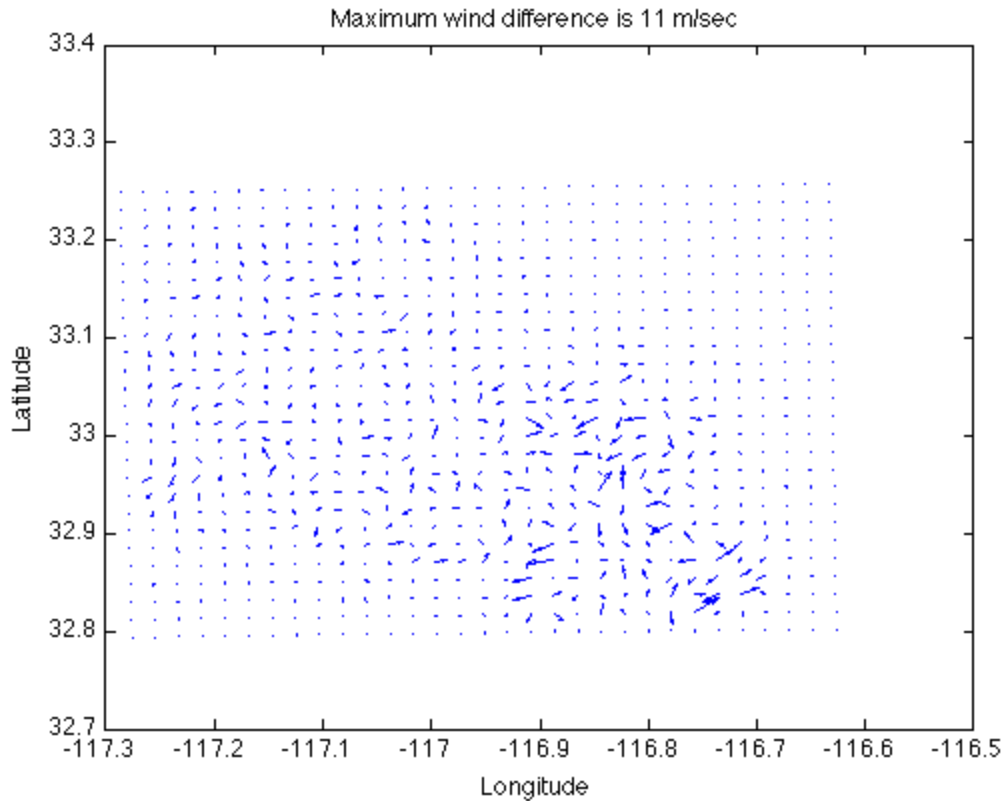
P1788 (Figure 5): Again, why display fire output in UTC? Mis-spelled 'simulation' in second line. Again, can't you quantify the differences and similarities between the two simulations? See Filippi et al (2014) for some metrics of comparison. In particular for a simulation, final fire shape alone is an insufficient metric as arrival time is also very important.

This is derived from WRF, which uses UTC. We will change to local time and add a comparison of fire arrival times.

P1789 (Figure 6): The function displayed in (b) is very different to the one shown in (a). The linear nature of the time-lag model suggests that it will under-predict Van Wagner and Pickett for nearly any rain rate and starting moisture content.

Figure (a) is from the Van Wagner and Pickett formulas, which have a number of different cases and correction factors added over time. We use the fitted coefficients, which result in (b), as the default, but the user is free to modify those coefficients. We believe that any differences are well within the region of uncertainty. Statistical studies for specific locations (Vejmelka et al., 2014) [in references to this response] result in different values in any case.

P1791 (Figure 8): The 'diurnal' oscillation of the moisture content curve does not appear to follow the actual daylight hours of the day and the subsequent maxima and minima of the rate of spread prediction appears to even further removed from what would be expected of wildland fire behavior. Assuming that the fire occurred in the Pacific Daylight Time zone (UTC -7 hours), the FMC maxima and minima seem to be around 9.00 am and 5.00 pm. Sunrise at this time year for Spokane (about 150 kms away) is 6.25 am and sunset 7.05 pm. This means that after 2.5 hours of sun, the modeled moisture is still dropping whereas in reality the MC of fine fuels follows the almost immediate rise in relative humidity following dawn.

The reason for the lag as described earlier is the fact that the integrated total fuel moisture is plotted here with contribution from other fuel classes as well, not only the fine fuels moisture.

Similarly, the minima of FMC normally occurs 3 or so hours after solar noon (in this case noon is 12.45 pm). A minima at 5.00pm is also rather late. These lags are prob- ably driven by the use of parameters and coefficients derived from 10 hour fuels rather than 1 hour fuels. This is also evident in the values of the maxima and minima ˘T˘an absolute variation of only 9% (in some cases as low as 2% appears to grossly underestimate the true variation in moisture content particularly in late summer.

The diurnal variations of integrated fuel moisture has smaller amplitudes than the fine fuel moisture, as it contains contributions from slowly varying coarse fuel classes, responding to the atmospheric conditions at longer timescales than one hour.

These lags in moisture content seem to be exacerbated in the lags in fire behavior. The period of active fire spread appears to commence at between 10.00 am and 1.00 pm and cease between midnight and 01:00 am. Of course this is dependent on other environmental variables but also seems to be well behind the expected diurnal variation in fire behavior. Most fires will not continue spreading so actively 5-6 hours after sunset. This is a significant issue and needs better testing and validation before it can be deployed.

We have compared the time series of the computed 10h fuel moisture with available observations in order to assess the time lag and have found no indication of the fuel moisture model output (10hr) lagging behind 10-hr observations.

P1792 (Figure 9): It is difficult to see the point of this figure without a significantly better graphic. What do the colors mean?

We will replace Fig. 9 by the following:
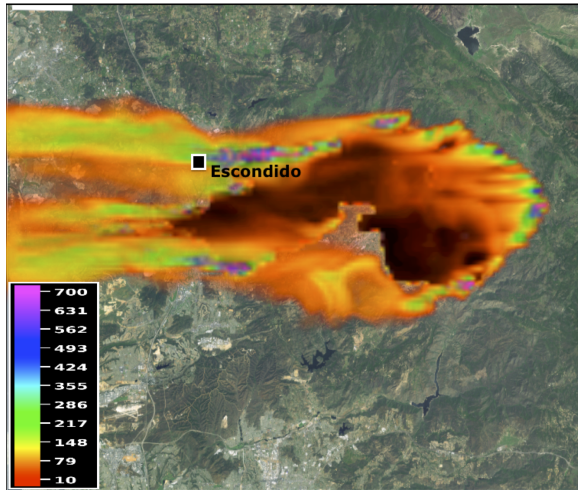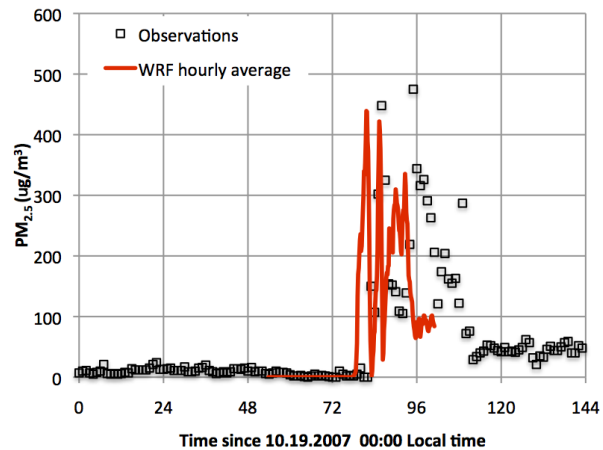
(a)   (b)

An example of the PM 2.5 concentration field in ug/m3 simulated for the Witch and Guejito fires in California using WRF-SFIRE-CHEM. a) Simulated PM2.5 on 10.22.2007 13:00 local time (hour 85 since 10.19.2007 00:00) with the location of the Escondido air quality station, b) Simulated hourly-averaged PM 2.5 (red line) and observations (black points) from Escondido air quality station, marked as black-filled square on panel a).

References

Chandler, C., Cheney, P., Thomas, P., Trabaud, L. and Williams, D. 1983. Fire in Forestry 1: Forest Fire Behaviour and Effects. John Wiley & Sons, New York, 450 pp.

Filippi, J.-B., Mallet, V. and Nader, B. 2014. Representation and evaluation of wildfire propagation simulations. International Journal Of Wildland Fire 23, 46-57.

Matthews, S. 2014. Dead fuel moisture research: 1991-2012. International Journal Of Wildland Fire 23, 78-92.

Sullivan, A. L. 2009a. Wildland surface fire spread modelling, 1990-2007. 1: Physical and quasi-physical models. International Journal of Wildland Fire 18, 349-368

Sullivan, A. L. 2009b. Wildland surface fire spread modelling, 1990-2007. 2: Empirical and quasi-empirical models. International Journal of Wildland Fire 18, 369-386.

Sullivan, A. L. 2009c. Wildland surface fire spread modelling, 1990-2007. 3: Simulation and mathematical analogue models. International Journal of Wildland Fire 18, 387- 403.

Sullivan, A. and Ball, R. 2012. Thermal decomposition and combustion chemistry of cellulosic biomass. Atmospheric Environment 47, 133-141.

Referee 2

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.

In the meantime, some of us completed papers on validation of the moisture model (Vejmelka et al., 2014) and coupling with WRF-Chem (Kochanski et al., 2014). We will refer to those papers as well as include results of some additional validation studies in this paper. We will omit the new fireline intensity here and leave it to a separate paper in future.

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=RHw

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

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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 area of the fire front. It has the dimension of J/m2/s. It seems that this is the quantity the authors want to estimate.

No, the new type of fireline intensity is different from the reaction intensity or from Byram's fireline intensity. However, we will omit the new fireline intensity here and leave it to a separate paper in the future.

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 Israel fire forecast system is not relevant with the content of the manuscript.

We will change the title.

Section 1:

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

We will move this note to Section 8.

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).

We will follow established glossary, using firewords.net and other sources, rather than using synonyms.

P1764-l7: e(-tau/Tf), why tau?

Thank you. This should have been t, not tau.

The "fuel burn time", is this the flame residence time?

No, this is not flame residence time, rather the characteristic time of the burning reaction - the time it takes for the fuel fraction to decrease from 1 to 1/e.

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.

We will provide maps as illustrations.

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.

We will provide the following graph showing an example of ROS obtained from Rothermel's model by varying the fuel moisture contents (fuel model 3, zero wind, zero slope):
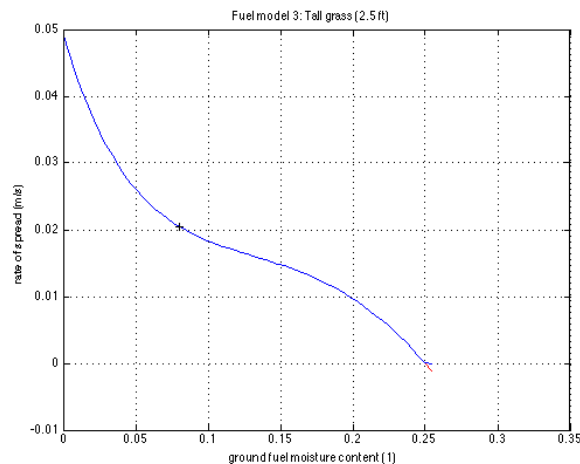
The dependence for ROS on fuel moisture contents in Rothermel's model is through an empirical quantity, the moisture damping coefficient , which cannot be derived from physical principles.

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

In NDFRS (wfas.net), the fuel classes are defined in terms of fuel diameter classified by the timelag: "A fuel's timelag is proportional to its diameter and is loosely defined as the time it takes a fuel particle to reach 2/3's of its way to equilibrium with its local environment."
Our usage seems to be well within the range of "loosely": 1-1/e = 0.63… rather than ⅔ = 0.66...

P1768 – Eq 6: mention that M = m(t)

We will note that M_n is numerical approximation of $m(t_n)$.

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.

The exponential is not a notation. The solution of (5) is exponential function of t. We have skipped this step, and just wrote the solution at the end of the time step. We will add the step now and some further detail to aid understanding.

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

We will add references to Janic (2001) and Kain and Fritch (1990). [in the references for this response]

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

OK.

Section 5:

P1770-l22: Nk is not fdefined

We will define N_k (the number of fuel classes in the fuel moisture model)

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.

In the meantime, the paper Kochanski (2014) was written with the validation. Please see above for the new Fig. 9.

Section 8:

P1775-l17: NWS is not defined

We will expand this acronym to National Weather Service (of Israel).

Figures:

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

We will add the missing time series for the fire simulated with the constant fuel moisture of 6.38%.

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).

Please see above for the new Fig. 9.

References:

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 fires. Can. 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

Additional authors' references to this response:

Albini, F. A. 1981. A model for the wind-blown flame from a line fie. Combustion and Flame 43: 155-174.

Albini, F. A. 1982. Response of free-burning fires to nonsteady wind. Combustion Science and Technology 29: 225-241

Beer. T. 1991. The interaction of wind and fire. Boundary Layer Meteorology 54: 287-308

Carmel, Y., S. Paz, F. Jahashan, and M. Shoshany, 2009: Assessing fire risk using Monte Carlo simulations of fire spread. Forest Ecology and Management, 257, 370–377

Fromm, M., Tupper, A., Rosenfeld, D., Servranckx, R., and McRae, R., 2006: Violent pyro-convective storm devastates Australia's capital and pollutes the stratosphere: Geophys. Res. Lett., 33, L05815.

Janjic, Z. I., 2001: Nonsingular implementation of the Mellor-Yamada level 2.5 scheme in the NCEP Meso model. NCEP Office Note No. 437, 61 pp.

Kain, J. S., and J. M. Fritsch, 1990: A One-Dimensional Entraining/Detraining Plume Model and Its Application in Convective Parameterization. J. Atmos. Sci., 47, 2784–2802

Kochanski, A. K., M. A. Jenkins, K. Yedinak, J. Mandel, J. Beezley, and B. Lamb, 2014: Toward an integrated system for fire, smoke, and air quality simulations. International Journal of Wildland Fire, submitted. 4th Fire Behavior and Fuels Conference Raleigh, North Carolina, USA February 18 - 22, 2013.

Nelson R. M., 2001: Water Relations of Forest Fuels, In 'Forest Fires'. (Eds. E. A. Johnson and K. Miyanishi) pp 79 – 149. Academic Press, San Diego.

Osher, S. and R. Fedkiw, 2003: Level Set Methods and Dynamic Implicit Surfaces. Springer, New York.

Rosenfeld, D., Fromm, M., Trentmann, J., Luderer, G., Andreae, M. O., and Servranckx, R., 2007: The Chisholm firestorm: observed microstructure, precipitation and lightning activity of a pyro-cumulonimbus, Atmos. Chem. Phys., 7, 645-659.

Vejmelka, M., A. K. Kochanski, and J. Mandel, 2014: Data assimilation of fuel moisture observations from remote automatic weather stations. International Journal of Wildland Fire, submitted. 4th Fire Behavior and Fuels Conference Raleigh, North Carolina, USA February 18 - 22, 2013.