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Geoscientific Model Development Discussions

Interactive comment on "Coupled atmosphere-wildland fire modeling with WRF-Fire version 3.3" *by* J. Mandel et al.

Anonymous Referee #2

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The present paper is dedicated to a coupled atmosphere/wildfire numerical model named WFR-Fire. The approach proposed in this paper, is based on the Weather Research Forecasting (WRF) atmospheric model, coupled with a level-set type method to track the time evolution of the position a wildfire front at landscape scale. From a general point of view the paper is well structured and contains many details concerning the numerical scheme used to solve the level set equation used to simulate the fire propagation and the manner used to couple the fire model with the atmospheric model WRF. One can regret that, much more space was reserved for the presentation of the model than for the discussion and the validation with experimental data. An re-equilibrium of the paper would be suitable before a publication.

p.498 (line 18): check the following expression 'reanalysis' or 'analysis'?

p.501 (line 21-26): In taking into account that, for many practical situations, the fire can propagate through a highly heterogeneous vegetation layer, the fuel properties must be understood as average fuel properties, or the authors must indicated how the local heterogeneity of the fuel are taken into account in WRF-Fire.

p.502 (line 10): The authors must explain why for Chaparral the slope of the terrain does not affect the rate of spread of the fire front. This affirmation is quite surprising. (line 12, eq.2): The units associated with formula (2) must be indicated. (line 22): At what height above the ground the wind speed U was defined? Does the wind speed U is defined as the 10m open wind speed? Does the wind speed U was affected by the presence of the vegetation (roughness effect)?

p.503 (line 11): How is evaluated the local value of the fuel weight (i.e. burn time)? What are the external physical parameters (wind, slope, air temperature, relative air humidity ...) which can affect the fuel burn time? This key point needs to be justified (using physical considerations) with more details.

p.504 (line 5-13): On what physical criteria, the grid size for the atmospheric domain and for surface fuel was chosen? What the typical values?

p.522 (line 15-26): Compared to the part devoted to the presentation of the model, the part concerning the validation and the comparisons with experimental observations is very short and can be extended, to consolidate the degree of confident of the model. The comparison between the numerical and the experimental temperature signals (see Figure 6), was not so good, especially just after the passage of the fire front (cooling phase). This point can be commented. Did the vertical wind profile (average value and turbulent kinetics energy) were identical for the simulations and during the FireFlux experiment?

p.523 (line 9): To analyse the results presented in Figure 10, a comment such as 'realistic fire and atmosphere behaviour was obtained.' is too short to be considered as a validation of the model. Is it possible to add more quantitative comparisons?

For all comparisons, how was the vertical structure of the atmosphere (stable, neutral, unstable)?

p.524 (line 18): What means the ARW core?

p.525 (line 12-14): In grassland, in dry conditions, the fuel moisture content (FMC) is quite low (it can be less than 10%), it is probably right to consider that the production of water can be considered as coming essentially from the combustion process, for other ecosystems (such as Chaparral for example), even during the dry season, FMC can be much larger (ranged between 30 and 70%), in this case it will be more difficult to neglect this flux. This point must be clarified.

p. 527: The conclusion is too short!

Interactive comment on Geosci. Model Dev. Discuss., 4, 497, 2011.