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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 #1

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Coupled atmosphere-wildland fire modeling with WRF-Fire version 3.3

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Summary:

The authors describe two-way coupling of a level-set method to track a fire front with the Weather Research and Forecasting (WRF) model. Such a model has the potential to offer real-time prediction (i.e. forecasting) for wildfires. The WRF horizontal wind field is sampled and logarithmically interpolated to 6.1 m above the roughness height (for consistency with the spread rate function) and this wind speed is used to parameterize a fire spread rate function, taken as a variant of the usual Rothermel (1972) model. The spread rate governs the evolution of the level set. The leading edge of the front is envisioned to ignite a surface fuel load. The heat and moisture fluxes decay exponentially in time as the fire front (zero level set) passes. The fire is therefore represented by a burning region instead of a 1D line. The feedback to the WRF model is accomplished by augmenting source terms in the potential temperature and moisture equations on the finest nested grid. The strength of the source terms decays exponentially in the vertical direction. The authors go on to discuss details of the input parameters for WRF and the direction of future WRF-Fire model developments.

General Comments:

The topic is of intense interest to the wildfire modeling community and the approach and tools documented in this paper represent progress toward the goal of obtaining accurate operational forecasts for the spread of wildfires. The main contributions of the work are the details related to calculation of the ignition time and fuel fraction. It is convenient to have the discussion of the level set algorithm and the WRF source terms within this paper to keep the document reasonably self-contained. The discussion of the software structure and the details of the WRF input settings, however, would be better suited for an online User Guide and Technical Reference Guide (which should evolve with the code). While these details are of course important for a reader to be able to reproduce certain validation tests, no validation is presented in this work, and so the pages of input details ultimately detracts from the main points of the article. Further, the tone of the article comes across as a set of notes to the authors themselves rather than a polished journal paper. I, therefore, support the publication of this article in Geoscientific Modeling Development, but I suggest the following comments should be addressed to improve the overall quality of the manuscript.

Detailed Comments (page numer-approximate line number):

1. 498-9: "and it uses the WRF parallel infrastructure for parallel computing." Omit. Not relevant.

2. 498-22: WFDS does not require a parallel supercomputer. It can be run on a pc or

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a Mac... and a supercomputer.

3. 499-7: omit "also"

4. 499-8: The authors often use the term "correct" cavalierly. It is one thing to get the "correct" result for a verification test like reproducing an analytical solution (I assume in this case it is an ellipse). It is a very different thing to the the "correct" wildfire shape. This is validation and no validation is done here. When we start talking about accuracy of the forecast we have to be picky about such things.

5. 499-14: I prefer the word "forecast" over prediction. There is no reason why a prediction needs to be faster than real time. But a forecast certainly must be.

6. 499-19: At this point in the document it would be good to discuss what you mean by "data". You have not yet pointed the reader to Table 1, which you do in Section 2.1. Just a sentence or two for those not "in the know" would be helpful here.

7. 499-28: Other communities will know "tracers" as Lagrangian particles and the level-set method (what you later call a "global" approach) as an Eulerian method. It would be good to orient the reader here.

8. 500-23: "was written in 2007", unnecessary.

9. 501-11: "from [a] logarithmic"

10. 502-10: It would be helpful to have one sentence explaining why the chaparral spread rate only depends on wind speed.

11. 503-14: Why do you call this a "density"? The units for (5) and (6) just indicate a flux.

12. 504-end of Section 2: This would be a good place for some discussion of the level of approximation being made with these spread rate functions. At least foreshadow the discussion in Section 11.3.

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13. 504-12: The word "subgrid" used here can be confusing terminology. In the LES community, "subgrid" means you have no data storage in that physical space. Here is seems you use subgrid to be the same as your "fire grid" which you use for interpolation purposes and computation of gradients, etc. And if this is not the case, then it needs clarification.

14. 504-21: Omit "Since".

15. 505-17: What is the relation of the fire grid to the subgrid discussed earlier?

16. 507: Do you really need more numerical viscosity when you are already using first-order upwinding? Is this because your dt is dictated by WRF and not the stability constraints of your level set PDE? We have had no stability problems with a similar approach using second-order TVD schemes (CHARM or Superbee). A couple other places in the paper you talk about using second-order schemes, one for interpolation of the ignition front and one for time advancement. But doesn't your level set solution degrade to first order because of your flux function? You should show a spatial convergence study to convince the reader your scheme is accurate, especially given that you later go on to show demonstrations with topology resolutions down to 3 m.

17. As discussed in the General Comments above, I would omit sections 7-9.

18. 523-Section 11.1: How do you plan to add smoke? I am surprised there is no mention of merging with WRF-IBM (Immersed Boundary Method) at some point. If you are trying to handle very fine scale resolution with steep topography angles, it is well-known that the scalar solutions near the surface will contain spurious oscillations due to the pressure-based conformal grid.

19. 524-Section 11.3: I know you said you do not yet handle crown fires, but is there any consideration for how you would handle spot fire ignition from lofted firebrands? Or how radiation effects would be accounted for in the spread rate function?

20. 527-Section 12: The final comment is a major one. The authors need to think

carefully about how they will validate this code. In fact, this should be its own section. One sentence, "Validation is in progress," is insufficient. If it is in progress then we are interested the plan and results to date.

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