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Interactive comment

Interactive comment on "Physical parametrisation of fire-spotting for operational fire spread models: response analysis with a model based on the Level Set Method" *by* Inderpreet Kaur et al.

Anonymous Referee #2

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General comments: The paper outlines a physical parameterization of the effects of spotting and atmospheric turbulence on the fire propagation. The main concept utilized in this approach is to treat them as random processes and apply the correction to the level set function used to trace the fire progression. The presented method is an extension of the original approach focussed on the turbulent effects and published by the authors previously. The subject is definitely of a great importance from the fire modeling standpoint and fits the journal well.

Specific comments on the structure of the paper: The paper is generally well written, but its organization should be revised. There are three elements that are missing in the

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Discussion paper

paper. First is a clear description of the physics of the process being modeled, as well as approximations and assumptions necessary to formulate the model. The second one is a simple description of the input into the model, the output from the model and what the model does in practical terms. In the current form, all the equations need to be deciphered and linked together in order to reconstruct the proposed data flow. There are four distinct processes that should be clearly outlined, and framed by the corresponding input and output parameters: -Firebrand generation -Firebrand lofting -Firebrand horizontal transport and mass loss -Secondary ignitions

The third missing element is a detailed description of the modeling setup used in this. Without that, is not clear how the results presented in the paper have been obtained, and their replication is impossible. Also, the choice of parameter values used in the study should be discussed (e.g. the diffusion coefficient).

Specific issues: The main issue that needs to be addressed is the model formulation in terms of the firebrand lifting. In the proposed approach Wang (2011) formula is used (see eq. 5 on p 5). Unfortunately, this formula is not used as intended. Contrary to what is suggested, it represents the downwind propagation distance as a function of the loftable height being a function of the firebrand size. 'H' in equation 5 has nothing to do with the maximum plume top height provided by the Sofiev scheme. It is a maximum loftable height for a firebrand of radius r, (which is a function of the up-draught flow), not the plume top height which defines at what height the vertical in-plume velocity goes to zero. Using the plume top height in this context is incorrect because firebrands do not behave as air parcels due to their mass and non-zero fall speed. I think this problem may have led to doubtful conclusions presented in section 6.

A logical solution would be to use the resolved velocity profile from the coupled fire-atmosphere model to compute the maximum loftable height based on the updraft strength at any given height. Alternatively, a simplified 1D column model (for instance Freitas model 2007, 2010) could be used to compute the vertical profile of the updraft velocity needed to assess the lifting height.

The aforementioned issue is related to the choice of the host fire model used for the simulations. The paper could be strengthened by implementing the proposed method into an actual coupled fire-atmosphere model like WRF-SFIRE, or FOREFIRE. There are two reasons for that. One is the practical aspect of the presented work, and the other one is the physical representation of the spotting process itself. The coupled fire-atmosphere models offer many advantages over uncoupled models especially important from the standpoint of the fire-spotting modeling. They resolve convective updrafts uplifting firebrands, they render the flow downwind from the fire transporting the firebrands, and they account for the interactions between the secondary spot fires and the main fire front. All these aspects are missing in the actual analysis presented in the paper, but when added they would significantly improve the scientific content of this paper as well as its broader impact. The WRF-SFIRE mentioned by the authors multiple times, or any other community open source fire model would be a great choice for a host-model assuring broader application of the presented method, reaching beyond theoretical discussions.

I would also suggest reorganizing the discussion of the model limitation following the general comments. For instance how the vertical wind shear is taken into account? How the wind modification by the fire itself is represented, do the secondary ignitions interact with the main fire front etc.

Adding a table with the description of all the symbols used in this study would be very helpful. Its lack makes following the model description very difficult.

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