Nat. Hazards Earth Syst. Sci. Discuss., 2, C1624–C1631, 2014
www.nat-hazards-earth-syst-sci-discuss.net/2/C1624/2014/

Natural Hazards and Earth System Sciences
Discussions

Interactive comment on "Resolving vorticity-driven lateral fire spread using the WRF-Fire coupled atmosphere-fire numerical model" *by* C. C. Simpson et al.

C. C. Simpson et al.

c.simpson@adfa.edu.au

Received and published: 21 July 2014

The authors would like to thank the anonymous referees for taking the time to provide comments on this paper. In particular, Referee 2 highlighted an error in our discussion of the method used to determine the mid-flame height winds. We apologise for this error in the model description, which has now been corrected.

We have gone through the manuscript and attempted to address each of the comments made by the referees. We have also made a number of editorial changes to the manuscript to improve readability, but without changing the scientific arguments.

The most important of these editorial changes are discussed below as general author comments, but not every change is addressed for brevity.

Response to comments from Referee 1:

R1. Abstract: L5: hard to reference a work with no references (because abstract), you may remove the sentence of state that "Numerical studies suggest that fire channelling".

A1. We have removed any reference to previous work in the abstract, including the sentence suggested.

R2. L14: please put also an idea of the vertical grid spacing near the ground, of prime importance here too.

A2. We have now specified in the abstract that the horizontal and vertical grid spacing are varied together between 25 and 90 m, which should be sufficient detail for the abstract. Outside of the abstract and in the second sentence of subsection 2.2 we make it clear that the horizontal and vertical grid spacing are varied together. In the 1st sentence of the 4th paragraph in subsection 2.2, we now clearly state that the vertical grid is non-stretched i.e. constant throughout the depth of the model domain, including close to the surface. We also comment on the slight change in the vertical grid spacing with time due to the movement of the model top.

R3. 3501 L5: please be more specific, Sullivan's review papers are not on the steady state- ness. You can maybe make a difference between the "potential ROS models" steady by nature and the other models usually more complex based on local energy balance.

A3. In the 1st paragraph of the Introduction, we have made it clearer that Sullivan's papers provide a review of wildland fire spread models, rather than on the steady state nature of some of these models.

R4. 3502 L15: Do not forget Clark/Packam/Jenkins/Coen that pioneered the work, and

had somewhat similar studies (especially the one you reference (nr3)) as this paper back in 96.

A4. We have included references to the two Clark et al. 1996 papers in the Introduction, and the Coen 2005 paper is referenced in section 2. It's not clear which Packham and Jenkins papers (they are co-authors on the Clark 1996 papers) you refer to, so these have not been included.

R5. 3504 L21: why 6.1 m ? is it for all locations ? is the wind speed taken at the 2d horizontal location of the mid/flame too ?

A5. Referee 2 has pointed out that in the version of the WRF-Fire we are using, the use of wind reduction factors was removed. We have therefore modified the description of the wind interpolation in the methodology. In terms of the horizontal location, the winds are interpolated horizontally to the fire mesh. We do not believe it is necessary to provide this particular detail, as the paper by Mandel et al. 2011 that describes the process in detail is referenced and can be consulted for further details on some aspects of the model.

R6. 3505 L1: I believe the vapour concentration is specifically fuel moisture dependent.

A6. We believe that you are correct. We have removed the comment that the latent heat flux is fuel type dependent.

R7. 3505 L6: Fire to atmosphere coupling is switched of by (. . .) I believe fire is still driven by the wind.

A7. Correct, the fire is still driven by the wind. Upon re-reading the original manuscript, we appreciate that our phrasing may have caused confusion. We have included the phrase "fire to atmosphere coupling" instead of "two-way coupling" in numerous instances throughout the paper, which will make this point clearer.

R8. 3505 L21: Which filter is used ? Smagorinsky ? there are some in WRF as I recall.

A8. I assume this should refer to page 3503 L21, rather than 3505 L21? As described later in the second paragraph of the model configuration section, a 1.5 order TKE closure is used for the eddy coefficient namelist option (i.e. km_opt = 2). The authors intend to include namelist files as supplementary material to improve reproducibility of the results.

R9. 3506 L4: is it the same TStep for all resolutions ? then scales up ?

A9. It is the same time step for all resolutions. I'm not clear what is meant by "scales up"? Again, an included namelist file as supplementary material will make this work easier to reproduce.

R10. 3507 L25: please reference the choice of factor (0,46) and height.

A10. With regards to the modified description of the wind interpolation, this comment is no longer relevant. However, we have attempted to explain our use of a mid-flame height and roughness length for this fuel type i.e. they are default values, and the choice of these values is still an active research question.

R11. 3508 L2: Fig 2 is rather important, but rather small, could you maybe make C25 larger, one thing that would clarify the understanding is to have time isocontours on one of the VDLS working sim, to picture a bit better the fire dynamics, even in Fig 5 it is not clear.

A11. Yes, we agree that Figure 2 was very small and it may be necessary to split this Figure into two separate ones. However, we have left it as is for now and will discuss this issue with the copy editors directly when appropriate. Depending on how the final figure size works out, we will consider adding time isocontours to one of the panel plots.

R12. 3512 L2: this erratic number sequence is because of intermittence, please explain a bit more clearly (as demonstrated by strong variations observed in the coupled factor ?)

A12. This erratic number sequence arises due to the strong variation seen in the effect

of coupling on the lateral spread. We have included a comment to this effect into the paper.

R13. 3513 L16: Increased heat release is linked to increased fire area here, it might be clearer to make the link directly.

A13. We have now attempted to make this link clearer at several points throughout subsection 3.5 e.g. "This increase in the power of the fire occurs due to an increase in the fire area [...]"

R14. 3516 L1: Is 2X2 grid cells only for c90 ?

A14. We have edited the first paragraph of the conclusions to make it clearer what resolution is required to model VLS: "This suggests that a horizontal and vertical grid spacing of 80 m or lower is required to model VLS."

Response to comments from Referee #2:

R1. Page 3500 lines 10 and elsewhere: Say here which grid spacing - apparently atmosphere, see page 3505.

A1. In the abstract we now have the following: "The atmospheric horizontal and vertical grid spacing are varied between 25 and 90 m [...]"

R2. Page 3502 line 9 and page 3504, line 20: The version of SFIRE available in WRF since 3.3 had the wind reduction factors removed. See http://www.openwfm.org/wiki/Fire_code_in_WRF_release for details, and the confirmation in Coen at al. (2013, page 18, column 1, bottom). The code with the wind reduction factors is WRF-SFIRE from openwfm.org mirrors (http://www.openwfm.org/wiki/How_to_get_WRF-Fire), described in Mandel et al. (2011), but that code is currently updated only to WRF 3.4. The version of the model needs to be clarified for the sake of reproducibility.

A2. The version of WRF-Fire used is that distributed with v3.5 of ARW (Advanced Research WRF). We therefore made an error in stating that wind reduction factors are

used, and we have modified the model description accordingly. In the first paragraph of subsection 2.1, we have attempted to clarify the version of the model used, and how it relates to CAWFE and other versions of WRF-Fire (or WRF and SFIRE). We apologize for the original error, we were not aware of this.

R3. Page 3504, line 12: Cite Rothermel (1972) only. It is unclear what modification of the fire spread rate equation is meant here. One significant modification from Rothermel (1972) is the use of the components of the wind and slope vectors normal to the fireline to drive Rothermel's formula. That, however, is due to Clark et. al. (1996a,b), not Mandel et al. (2011). Also, Clark et al. (2006a,b) should be cited for the concept of the two-way coupling of a fire spread model with an atmospheric model by the heat fluxes and the wind.

A3. We have removed the reference to Mandel from this line. We also now explain the modification to the treatment of the slope correction factor later in the paragraph and have dropped the initial use of "modified". In the first paragraph of 2.1, we now reference the CAWFE model through the Clark et al. 1996 papers and two other studies (I assume that you meant to refer to Clark et al. 1996a,b rather than Clark et al. 2006a,b? If not, then we will need further details to know which papers you refer to).

R4. Page 3505, line 21: It should be mentioned here that the simulations are not only idealized in the sense of disabling many schemes, but they are set up in on an ideal domain with a prescribed wind profile rather than a real terrain and data. There is a more detailed description of the ideal domain and the wind profile later.

A4. We have included the following sentence in this paragraph: "An idealised domain is used with a prescribed wind profile, rather than real terrain and weather data."

R5. Page 3507: It should be noted that the 6.1 m wind is obtained by an interpolation using the logarithmic wind profile (Mandel et al. 2011, sec. 5.2), and what roughness height was used.

A5. See reply to R2. We now state that the logarithmic wind profile is used and specify the roughness height.

R6. Page 3515: Please state the conclusion clearly: what mesh resolutions (horizontal and vertical) are needed for acceptable results?

A6. We have extensively re-written the first paragraph of the conclusions to state more clearly what mesh resolutions are needed for acceptable results "This suggests that a horizontal and vertical grid spacing of 80 m or lower is required to model VLS. However, given the sensitivity of the peak and average lateral ROS to the horizontal and vertical grid spacing, we suggest that a grid spacing of 30 m or lower is optimal for modelling VLS with WRF-Fire."

General comments:

1. Changed the naming convention for VDLS to VLS at the suggestion of a colleague. This does not reflect any change in our understanding of the nature of the dynamic fire spread, and is entirely editorial.

2. Introduced the acronym ROS for "rate of spread" or "spread rate". This acronym is widely used in the fire science literature.

3. Changed "uphill" and "downhill" to "upslope" and "downslope" for consistency with other work.

4. The domain-aggregated total heat release rate is now referred to as power of the fire, as this variable physically represents power (expressed in units of Watts).

5. The fire-induced vortices are now referred to as fire whirls, in line with the review paper by Forthofer and Goodrick 2011.

6. Removed the use of the term "5 min averaged". Put note alongside the description of Rothermel's equation that all rate of spread values presented are calculated using mean average over a 5 min interval unless otherwise stated. This improves the

readability of the results section.

7. Changed "large conflagration" to "conflagration" since the word conflagration already implies that the fire is large.

8. Replaced "perpendicular to the prevailing wind direction" with "transverse to the background winds", since prevailing does not necessarily imply the winds at the time.

9. We have since discovered that it was incorrect to state that the fire whirls "dissipate" on sub-minute intervals. The fire whirls can remain present over longer periods of time, but their physical characteristics do vary considerably on sub-minute intervals.

10. We have included several new references e.g. Weise and Biging 1997 and Countryman 1972 (in addition to those suggested by the reviewers).

Please also note the supplement to this comment:
http://www.nat-hazards-earth-syst-sci-discuss.net/2/C1624/2014/nhessd-2-C1624-2014-supplement.zip

---

Interactive comment on Nat. Hazards Earth Syst. Sci. Discuss., 2, 3499, 2014.