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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.

Anonymous Referee #1

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General comments,

This paper presents a kind of numerical sensitivity analysis of the WRF-Fire tool, focused on one kind of fire spread that appears to be clearly due to coupled fire/atmosphere effects. The goal is to see If and what kind of parameterisation can reproduce this effect. Overall, although I am not a native English speaker, the written language reads satisfactory, with no . The paper is well structured, with all figures correctly explained and clear (sometimes rather dense / small). I also found the paper to be well referenced with no obvious additions required to my knowledge.

The study seems to be well performed, with a reasonable choice of parameterisation and resolution for the comparison. It would maybe be interesting to see if coarser resolutions would still have performed well on larger domain/mountains, but this is discussed in the conclusion where the link is made between the number of cells and eddy size. There are no very new findings or methodological breakthrough in this paper, but this study may be somewhat a reference that may help to compare codes on these highly idealized scenarios and therefore important to the research field and to understand better the strengths and weaknesses of coupled atmo-fire tools and WRF/Fire in particular.

I only have minor (some are small details) comments and believe the paper may be published if those are found relevant and implemented in a revised version.

Detailed comments:

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

3501 L5: please be more specific, Sullivan's review papers are not on the steady stateness. 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.

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.

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

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 ?

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

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

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

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

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.

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 ?)

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

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

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