Atmos. Chem. Phys. Discuss.,
https://doi.org/10.5194/acp-2017-1204-RC2, 2018

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Wildfires as a source of airborne mineral dust – Revisiting a conceptual model using Large-Eddy simulations (LES)" *by* Robert Wagner et al.

Anonymous Referee #2

Received and published: 11 June 2018

This article investigates the impact of a high sensible heat flux (attempting to represent a wildfire in a simplified manner) to promote the presence of the required conditions for enhanced dust emissions. The authors perform 9 large-eddy simulations (LESs) varying the strength of the ABL wind and the distribution and magnitude of the imposed heat flux (steady in time and space). The LES runs are properly designed, and although periodic boundary conditions are applied in the horizontal direction, the analysis is restricted to a relatively small time window over which the atmospheric conditions near the heat source remains unaffected by the periodic flow. The authors confirm/verify that the strong heat flux results in the formation of a convergence region, in which probability of higher velocities is increased, and therefore the potential

for dust mobilization (based on thresholds velocities). The manuscript is overall well written and organized, although language can be improved in a few places. However, there are several aspects that need to be addressed before the manuscript can be considered for publication in Atmospheric Chemistry and Physics.

General comments

1) The aim of the paper is unclear, beyond demonstrating that a significant heat flux applied over a specific region does create localized convergence. This is a known fact, which has been shown already in the literature. The authors should be more specific and clear on what the objective and novelty of their research is. While the authors present systematic description of the differences among the 9 cases, there is no specific findings besides "bigger and more intense fires will lead to stronger convergence and therefore larger velocities at the surface".

2) The focus of the analysis seems to be on the near-surface velocities (and probabilities of exceedance of a certain threshold). While this is one aspect, the problem is more fundamentally governed by turbulent processes, so there are other magnitudes that should be incorporated to the analysis. For example the authors should look at vertical fluxes of horizontal momentum, and potential temperature, as well as turbulent kinetic energy. At the end, dust mobilization is tied to turbulent mixing, since there are not going to be steady uniform horizontal and vertical components to mobilize the dust. In that regard, it is expected that enhanced TKE regions will highly correlate with the results presented in Fig. 10.

3) The authors attribute the more efficient penetration of the heat plume into the free atmosphere to the ambient wind conditions, and they in all cases find the fire propagating above the ABL irrespective of the strength of the imposed heat flux. This is likely to be also influenced by the strength of the stabilizing capping inversion. The authors should at least acknowledge this aspect and include information about their setup (currently missing). In fact, there are many real fires in which the plume remains within the

ABL.

4) Introduction is excessively long. 1.1 can be removed, as well as most of 1.3 (first long paragraph). It is a nice review of related matters, but not really pertinent to the specific research carried out by the authors.

Minor/specific comments 1. Page 1, line 6: “highly”. 2. Page 3, line 9: “rapidly”. 3. Page 3, lines 9-10: ABL typically presents shear. It would be perhaps more correct to say: "possible increasing wind shear". 4. Page 3, line 20: “partial”. 5. Page 4, lines 1-30: This paragraph is too long and not specifically related to the work presented herein. It should be significantly shorten and focused into what is relevant to the paper. 6. Page 6, line 20: If the code is fully compressible, this should be more like 0.02 s. Also, what do you mean by "initial"? Is it adaptive time scheme? 7. Page 7, line 10: How is wind at 10 m specified? Is it through a geostrophic forcing? Please explain. It would be good to include the geostrophic wind speed so the reader can have a better understanding about the strangth of winds above the ABL (as well as within the rest of the ABL). Also, including ABL height in the table would be useful. 8. Page 9, lines 2-4: The authors do not discuss the potential temperature distribution of the incoming ABL (in particular strength of capping inversion). This will have a strong impact on whether the fire-generated updraft can reach the free troposphere. The authors should include this information and incorporate to the analysis/conclusions sections of the manuscript. 9. Page 9, line 35: “perpendicularly”. 10. Page 11, lines 3-4: "spreading" may not be the best word to use. It can be confused with "fire spread", which is not the case here. Please find a better word. 11. Page 11, line 4: “near-surface”. 12. Page 11, line 18: Is that the right quantity? Vertical wind or TKE will likely be better estimates of dust mobilization at the surface. 13. Page 11, line 28: Is this the expected behavior in the surface layer where structures are going to be predominantly anisotropic? 14. Page 11, line 34: Last 2 lines in page 11 should either go with previous or following paragraph, not as a stand-alone paragraph. 15. Page 13, lines 15-20: Any explanation/speculation of why that happens? 16. Page 13, lines 26-27: Horizontal component, correct? This

is clarified layer in the discussion but it should be explicitly mentioned when reference to the threshold velocity is first made.

Interactive comment on Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2017-1204, 2018.