

Author's comments to anonymous Referee #2

The authors would like to thank the reviewer for the time spent on the manuscript and for providing constructive comments and suggestions. We have considered all comments carefully. Please find our detailed reply (italic) on the comments made below.

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”.

The aim of our paper is to show and to test the conceptual model describing how mineral dust particles can be mobilized and raised by fire-driven wind regimes and finally be injected into the atmosphere by the fire updrafts. Therefore, a detailed understanding of the fire impacts on the near-surface wind pattern is necessary because the near-surface winds are inevitable for particle mobilisation. To estimate the dust emission potential of typical grass-/shrubland fires and eventually the amount of dust emitted, the impacts of different fire setups have to be tested too.

In order to address the reviewer's concerns, we have strengthened the discussion and conclusion section aiming at highlighting the ability of wildfires to modulate the near-surface winds in a way stimulating dust particle mobilization and entrainment into the atmosphere. Hence, findings from this study contribute to the efforts on improving estimates on atmospheric aerosol loads and their feedbacks.

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.

We have predominately focussed on the near-surface wind velocities, since this quantity is crucial for the mobilization and emission of mineral dust particles – the main focus of the paper as mentioned in more detail above. TKE was indirectly taken into account since the turbulence finally governs the increased horizontal and vertical wind velocity near the surface. To address the reviewer's concern we now show vertical profiles of maximum TKE values for all case simulations (new Fig. 7).

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.

Thanks for pointing this out. We are aware that lots of the “real” fire plumes do not penetrate into the free atmosphere (see also the given references in Section 1.3) and that the likelihood that and how deep a fire plume penetrates into the free atmosphere depends on several

factors like fire properties as well as the current state of the atmosphere/ABL. We have rewritten the concerning paragraphs and weakened the statement in order to make clear that we discuss specific model results. We further highlight that the main focus of this study is on dust particle mobilization potential near the surface. To support this, we have removed Fig. 11 dealing with updraft strengths from the paper and have shortened the discussion thereon significantly.

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.

We have decided to provide the reader with a quite detailed introduction including a substantial review of the relevant studies as dust emission related to wild fires is often not considered. This applies all the more since the topic addresses aspects of quite different research communities (dust research as well as fire research) so that a comprehensive introduction part seems to be needed. However, we went carefully through the introduction part and shortened where it was possible and appropriated.

Minor/specific comments

1. Page 1, line 6: “highly”.

Changed to “high-resolution”.

2. Page 3, line 9: “rapidly”.

Changed.

3. Page 3, lines 9-10: ABL typically presents shear. It would be perhaps more correct to say: "possible increasing wind shear".

Changed.

4. Page 3, line 20: “partial”.

The concerning paragraph was removed during rewriting.

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.

Thanks for your suggestion, which we have considered carefully. However, we have the opinion that a detailed review of the important studies dealing with dust emissions related to wildfires should be given, although they are often related to a broader context (see also other comments to major comment #4). Nevertheless, we went through the paragraph and removed/shortened some details which we considered as redundant.

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?

Thanks for pointing this out. The word “initial” was a typo and is now corrected. For our simulations, we used a split-explicit Runge-Kutta scheme, where the sound wave part is

integrated with a smaller time step satisfying the CFL constraint for the sound speed. Applying the CFL criterion and using the highest occurring (updraft) wind velocity of 28.5 m/s (STRONG-FIRE case) in our simulations, we obtain with a spatial resolution of $\Delta x = \Delta y = \Delta z$ of 10 m and the time step of $\Delta t = 0.2$ s a maximum CFL number of $c = 0.57$, which is still below the threshold of 1.

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 strength of winds above the ABL (as well as within the rest of the ABL). Also, including ABL height in the table would be useful.

Thank you for your hint. We applied a time-height independent u-wind of 3 m/s („ambient mean wind velocity“ in the text) as initial condition. Since this should represent the geostrophic forcing and not the 10 m wind speed, we accordingly changed the labeling to u_g instead of u_{10m} . Vertical profiles of horizontal and vertical wind for selected areas of the domain are shown in Fig. 6. Additionally, we have now included a more detailed discussion of the “undisturbed” NO-FIRE simulation concerning the structure of the atmosphere and the height of the ABL before we discuss the fire impacts on the wind fields. Vertical profiles of the TKE, and temperature are now shown (new Fig. 5b). Since the ambient conditions are similar for all other simulations but case 1 and 3 for which the ambient wind velocity is different, we think it is sufficient enough to provide the information on the ABL height exemplarily for the NO-FIRE simulation which is the baseline for all fire simulations.

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.

Thanks for the suggestion. As suggested, we included a profile of the potential temperature and the TKE of the NO-FIRE simulation to give a more detailed overview of the structure of the ABL and their capping inversion. It is correct that the state of the ABL has a strong impact on whether the fire plume and the fire-induced turbulence can reach the free troposphere or not. However, the main focus of our study lies in the ability of fire-driven winds to mobilize dust particles from the surface and not in the injection of particles into the free troposphere. Because of that, we do not describe the upper ABL atmospheric conditions excessively. To clarify this, we went through the manuscript and revisited our statements on particle entrainment into the free troposphere to present more strongly the limitation of our findings from this idealized model approach.

9. Page 9, line 35: “perpendicularly”.

Thanks. However, the concerning paragraph was removed during rewriting.

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.

Changed to “distribution”.

11. Page 11, line 4: “near-surface”.

Changed.

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.

To our knowledge, the horizontal wind velocity is the appropriate quantity here and a very commonly used measure for identifying the threshold for dust mobilisation and finally the dust emission potential. There exist lots of studies (e.g., Bagnold, 1941; Marticorena and Bergametti, 1995; Kok et al., 2012), which explain and highlight the horizontal component of the wind vector (parallel to the surface) as the main driver for the mobilization of dust particles at the surface – in particular via saltation. It is correct that under turbulent conditions also a direct entrainment of dust particles into the atmosphere can take place where the vertical component plays a more important role. This is taken into account via the calculations of areas where suitable vertical velocities occur (Table 3 and Figure 9). Additionally, we include a more detailed explanation of the different dust emission processes in the introduction part of the paper and mention the processes involved explicitly during the discussion of the results.

13. Page 11, line 28: Is this the expected behavior in the surface layer where structures are going to be predominantly anisotropic?

The PDFs shown in Fig.7 consider only the horizontal wind velocity and no information is given concerning the wind direction.

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.

The paragraph is shifted to the following paragraph.

15. Page 13, lines 15-20: Any explanation/speculation of why that happens?

We contemplate that the long perpendicular (in y-direction, perpendicular to the mean ambient wind velocity) extent of the fire line leads to a much broader area of fire-induced turbulence behind the fire line. Although the total impact concerning the peak values is smaller compared to the rectangular fire setup, the long fire line preclude/impede a mixing of the fire-induced atmospheric pattern with uninfluenced non-fire flow, which means that the fire-induced patterns are much longer present in x-direction before a weakening and mixing of the impacts takes place. Thus, the creation of higher wind velocities is downstream of the fire area much stronger impacted than in fire area itself. We have included an explanation in the paper.

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.

Yes, the horizontal component. We have clarified the sentence.