

Author's comments to anonymous Referee #1

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

- I understand from the title that the conceptual model is tested in the paper rather than developed. Where has the conceptual model been developed originally? Perhaps this could be stated more clearly.

The conceptual model has been developed based on published studies, which found mineral dust particles within smoke plumes and/or suggested that fires should be considered as a source of mineral dust. Building on this, we framed the model and applied LES to test it. In order to address the reviewer's concern we clarified this in the paper.

- There seems to be an important difference between the schematic shown in Fig. 1 and the results of the LES experiments: In the LES experiments, the increased wind speeds (and increased turbulence) occur downwind from the fire while in the figure, the “intensive turbulence” is depicted upwind/behind of the fire (also described in the text in See also the p. 3 1.20-27). This difference is critical, because – if the fire moves with the wind as is suggested in the Figure and text – the areas with high winds downwind of the fire might still be covered by vegetation, as the fire has not passed these areas. In that case, the vegetation cover might prevent particle entrainment. The same applies to areas with high winds further to the side (the “vortex trail”, Sec. 5, Fig. 8). I think it is important to discuss this in more detail or to highlight that the paper focuses solely on the aerodynamic aspects of the dust emission potential of wildfires, while the surface conditions remain undiscussed.

Thanks for pointing this out. Yes, it is correct that the outcomes of this study are slightly different from what the conceptual model is postulating – in particular regarding the location of the region of the highest wind speeds. The zone of convergence forms actually in front of the fire (downwind of the fire), however, the increased horizontal winds (as well as the increased turbulence) triggered by the fire updraft and the resulting confluence are also present within and upstream of the fire area. To illustrate this in a clearer way, we have replaced Figure 8 (former Fig. 7). The new figure shows PDFs calculated additionally for the direct fire area. A comparison between the fire-related impacts on the near-surface winds between the fire area and the boxes A-C is given in Table 2 using the relative fractions of exceedance of a horizontal threshold velocity. Soil conditions suitable for aeolian erosion and thus entrainment of mineral dust into the atmosphere can be expected as at least within the fire area a partly or complete removal of vegetation can be assumed. Additionally, we decided to adapt Fig. 1 in a way, which corresponds more with our approach and the results. However, the schematic remains an idealized picture of the real processes.

- On page 2, lines 23-27, the authors state that fire can affect the physical and chemical soil properties and conclude that this leads to “enhanced dust emission potential”. How exactly does fire affect soil mineralogy, texture, and grain-size distribution? I could imagine that there are effects that enhance and others that reduce the dust emission potential and that the effect is not as clear as suggested. This relates to my previous comment about a more detailed discussion on the surface conditions.

It is correct that the surface conditions during and also after a fire event are an important aspect especially regarding to the dust emission potential and that lots of different effects play

a role, e.g., how the fire affects the soil conditions and finally the erodibility of the soil. However, most of the results presented in the literature suggest that the impacts of the fire on the soil surface increase the soil's vulnerability to dust mobilization. Since the impacts of the fire on the surface are important for the conclusion drawn from our findings, we have included a more detailed discussion concerning the fire impacts and its impacts on dust emission.

- I have the impression that there is a confusion of concepts regarding dust emission in the paper. It is not clear which process the authors assume to cause dust emission in the context of wildfires – sandblasting (saltation bombardment) or direct aerodynamic uplift. In Section 1.2, the authors give “threshold values for dust emission” (I suggest “threshold values of wind speed needed for dust emission to occur” or comparable), a concept that is usually applied for saltation rather than direct entrainment. Then in Section 5, particle settling velocities are estimated for particles ranging from small clay to large sand-particles to investigate whether such particles would remain suspended once entrained, suggesting that direct entrainment is the process considered. Otherwise, the settling-velocity criterion would not be needed, because the larger particles could also entrain dust through impactation on the surface if the fire-related vertical velocities are too weak to overcome the particles' settling velocities. Clarification is needed on the processes considered as well as on their relevance given the turbulent conditions around the fire and the (unknown) surface conditions. As a side note, Section 1.2 does not seem to be well embedded between Sections 1.1 and 1.3 and the transition to Sec. 1.3 is somewhat abrupt.

Thanks for pointing this out. We agree with the reviewer's point of view. The processes, which might play a role in fire-related dust emissions, were not well clarified. Especially in presence of the fire-induced turbulence the direct aerodynamic uplift seems to play a non-negligible role. However, also the saltation bombardment contributes to the total dust emission flux – in particular in areas where the wind field is predominantly influenced by the enhanced horizontal wind velocities rather than only by the turbulent updrafts. To address the reviewer's comment, we have now included a more detailed description of both processes in the introduction part of the paper. We further have revised the discussion part accordingly. The transition between the single parts of the introduction has been smoothed, too.

- It is not clear to me why the authors use wind speed to estimate the dust emission potential and not friction velocity. Friction velocity is directly related to surface drag, which is what drives particle entrainment, and is normally available from model simulations. In that case, the threshold friction velocity could be determined using a physics-based relationship (e.g. Shao and Lu, 2000) and there would be no need to use an empirical wind speed threshold. Depending on the vertical wind shear, the use of friction velocity could lead to different results regarding the areas of potential emission.

As we use ideal surface conditions that are constant in time and space here, we decided to estimate the potential for wind erosion via the surface wind speed. For cases representing real surface conditions (i.e. vegetation cover), indeed, the dust emission flux will be calculated using the wind friction velocity, which is dependent on the surface wind speed and the roughness.

- The passage from p. 4 L. 1 to p.5 line 10 seems more general and introductory compared to the previous paragraphs that describe the fire-processes that could potentially cause dust emissions. I would suggest to restructure this part and to move the mentioned passage to an earlier position.

Thanks for your suggestion. We have restructured the paragraph to ensure a more logical order.

- General comments: Figure/results are discussed in great detail in the paper, which is good. Sometimes, however, this seems to lead to a repetition of aspects, e.g. increasing turbulence though the fire plume leading to peaks in wind speed etc. I believe it would be beneficial to go through the paper with a special emphasis on conciseness of the presentation.

Thanks for pointing this out. The manuscript was shortened where possible and appropriate.

- P. 8 l. 30-31: “Zones of strong convergence along with an acceleration of the horizontal wind” – the text and figure suggests that there is directional convergence and speed divergence, which would be “confluence” rather than “convergence”.

Changed to “confluence”.

- P. 10, l. 23: The weakening of horizontal wind speed with distance from the fire does not seem to occur between Box A and B. Can you comment on that?

The weakening occurs here only on the lowest model levels, whereas at higher levels (actual level depends on the mean ambient wind velocity) the weak horizontal winds are increased because of the downstream transport of fire-generated momentum respective turbulence and a downstream tilt of the fire updraft zone. Although the momentum is first mainly related to an upward motion, the subsequent generation of turbulent vortices leads also to an increase of the horizontal wind components. We have clarified this in the corresponding paragraph.

- P. 11 PDFs: I understand that the PDFs are calculated using different numbers of time steps. Does this effect the results?

Indeed. We have adapted the number of timesteps used for the calculation of the PDFs. The idea behind that is that the fire-induced turbulence needs some time to get transported downstream and with increasing distance to the fire area, the air masses remain longer uninfluenced. While the fire impacts on the wind in and around the fire area (e.g., box A) instantaneously, it takes some time until the areas further downstream (boxes B and C) become affected. Therefore, the number of time steps was reduced with increasing distance to the fire area. A consideration of all time steps since fire ignition would consequently reduce the fraction of time steps with wind speeds above the threshold especially in the “remote” box C.

- P. 12, l. 28: The numbers given here are specific to the case and should therefore not be given in such a general statement.

Removed.

- P. 18, L. 3-6: In my opinion, this paragraph contains several statements that are too strong. First, “This study gives a first introduction into the dust emission process during wildfires” should in my opinion rather be “This study investigates the potential of wildfires to created aerodynamic forces strong enough to emit dust” or similar. Second, “Further quantification” should be changed to “Quantification” given that no quantification has been done yet. Finally, while I understand that the estimation of fire-related dust emissions on continental and even

global scale and the study of their impacts are the eventual goal, I believe that it is a long way until then and that a study at local/regional scale would be the next step. The goal of an inclusion in large-scale models can (and should) be kept as a motivation, but I would suggest in a more moderate/realistic way.

Thanks for your suggestions. We agree, the statement reads very ambitious. We have rewritten the paragraph and explained possible applications of our findings in a more detailed way underlining the potential of the process and the results obtained from this study.

Minor comments (please see also annotated supplementary pdf for grammar and typos):

Many thanks for your very helpful corrections, which we have all taken into account.

- At several locations in the paper, the authors state that the numerical experiments are designed to “prove” the conceptual model. I believe this should be reworded to “test”, because the outcome of an experiment should be open.

Changed to “test”.

- P. 5 l. 15: It is stated that LES allows for “detailed process studies without interfering influences from the surrounding like topography or large-scale synoptic systems”. While this might be beneficial on the one hand, it is unrealistic on the other hand. I suggest rephrasing this as “detailed process studies in an idealized setup, i.e. without effect such as topography or larger-scale synoptic systems”.

Done. Thanks for your suggestion.

- In p. 5 l. 21 – 24: The authors contrast their approach/model to others like the WRF-Fire model, which can be used to study fire spreading and therefore includes an atmosphere-fire feedback. In the last sentence, it is explained that only the effect of fire on the atmosphere is considered in the present study (and not vice versa) and that therefore no atmosphere-fire feedback needs to be considered. The last part, however, is not currently mentioned, but should be added in my opinion and not left to the reader to conclude.

Thanks for pointing that out. We have rewritten and clarified these sentences.

- In my opinion, it would be better if Fig. 2 was true to scale.

Many thanks for this comment. We have carefully considered it, however, we have decided to keep the figure in its present form as the most relevant part of the analysis takes place within the first 1.3 km of the model domain so that this area should be represented more prominently compared to the rest of the model domain.

- If possible, it would be great to include a more meaningful labeling of each case in Figs. 3-5 and 8-11 that allows the reader to recognize the important aspect of each case like high-wind, large-fire, etc. more easily than through comparison to Table 1.

Thanks for the suggestion. We have included a new labelling of the cases as follows:

<i>old name</i>	<i>new name</i>	<i>description</i>
<i>case 0</i>	<i>NO-FIRE</i>	<i>control run (no fire)</i>

case 1	WEAK-WIND	weaker ambient wind conditions
case 2	REF-CASE	Reference case (moderate wind conditions, averaged fire properties)
case 3	STRONG-WIND	stronger ambient wind conditions
case 4	WEAK-FIRE	less intense fire
case 5	STRONG-FIRE	more intense fire
case 6	SMALL-FIRE	smaller fire area
case 7	LARGE-FIRE	larger fire area
case 8	ORTHO-FIRE	perpendicular orientated line fire
case 9	PARA-FIRE	parallel orientated line fire

- Abstract: “ – raised by strong turbulent winds related to the fire.” I believe that this cannot be determined for sure and that the sentence should therefore read “- likely raised. . .”

Changed to “most likely raised”.

- P. 4, l. 26: “such supergiant particles were present in all of the investigated fire sites”. Is it clear that the large constituents were “particles” rather than ash? For ash, the size is not as surprising, is it?

The cited publication (Radke, 1991) claims for particles but does not distinguish explicitly between soil remains and ash. However, also other studies have found coarse-mode particles with sizes of some hundreds of nanometers up to one millimeter from crustal/soil origin within smoke plumes. Nevertheless, we reword the paragraph.

- P. 4 l. 35: “particle formation”?

Changed to “particle aging processes”.

- P. 5 l. 30: atmospheric profiles of which quantities are specified?

I guess, P.6 l.30 was meant here. The initial profile consists of information on pressure, temperature, and humidity. We have included the information in the text.

- I suggest using “orthogonal” rather than “northerly”, “southerly”, etc. in the context of LES, as there is no need for the x-direction to be pointing toward the east.

Changed.

- While it is stated in the text that the fire temperatures listed in Table 1 correspond to the specified heat flux and do not directly translate into air temperatures, I recommend to also add a note in Table 1 explaining this.

We have decided to not show the additional temperature information in the table and explain the connection between heat flux and corresponding temperature only within the text.

- P. 8, l. 30: “small vortices” rather than “small turbulent eddies” to avoid confusion with small (diffusive) and large (energy-containing) eddies boundary-layer turbulence and large-eddy simulation.

Changed.

- P. 9 l. 4: “and causes strong turbulence around the area of the heated air” – I am not sure what is meant here. Perhaps downward mixing?

We clarified the sentence to “accompanied with downward mixing and a reallocation of the typical non-fire PBL structures.”

- P. 10, l. 31: “quickly turn to the normal non-fire behavior” – not clear in the context, can you reword this?

We decided to remove the whole sentence.

- P. 10, l. 34: “horizontally longer present” – suggest rephrasing.

The sentence was changed to: “Thus, the atmospheric patterns are vertically less impacted but the faster downstream transport of the fire-generated turbulence impacts on a much larger area in flow direction.”

- P. 12, l. 3-18: The discussion seems to be very long. Perhaps this can be shortened.

Done. The paragraph was shortened and adapted to the new Fig. 7 (PDFs of the fire area).

- P. 16 l. 2: What size is meant by super-micrometer particles?

We mean coarse-mode particles in general. We have clarified this.