Atmos. Chem. Phys. Discuss.,
https://doi.org/10.5194/acp-2017-1204-RC1, 2018

Atmospheric Chemistry and Physics Discussions
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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 #1

Received and published: 8 June 2018

Wagner et al. investigate the potential of wildfires to emit dust. Based on large-eddy simulation (LES) and predefined fire scenarios, the fire impact on the boundary layer wind speed and turbulent-kinetic energy is analyzed. Through comparison of wind speeds in the lowest model layer with an assumed wind speed threshold for dust emission and through estimation of particle settling velocities, conclusions about potentially fire-caused particle lifting and particle suspension are drawn.

The topic of the paper is very interesting and novel. However, there are in my opinion several aspect that require further consideration before the paper can be finally published. Please see my specific comments below.

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Discussion paper

- 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.

- 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 l.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.

- 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.

- 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 impaction 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 seems to be well embedded between Sections 1.1 and 1.3 and the transition to Sec. 1.3 is somewhat abrupt.

- 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.

- 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 positions.

- 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.

- 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”.

- 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?

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

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

- 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.

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

- 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.

- 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”.

- 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.

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

- 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-file, etc. more easily than through comparison to Table 1.

- 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. . .”

- 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?

- P. 4 l. 35: “particle formation”?

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

- 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.

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

- 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.

- 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?

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

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

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

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

Please also note the supplement to this comment:
https://www.atmos-chem-phys-discuss.net/acp-2017-1204/acp-2017-1204-RC1-supplement.pdf

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