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Atmospheric Chemistry and Physics Discussions

Interactive comment on "The Importance of Plume Rise on the Concentrations and Atmospheric Impacts of Biomass Burning Aerosol" *by* Carolin Walter et al.

Anonymous Referee #3

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GENERAL REMARKS The paper presents interesting results on how different treatments of plume rise of fire emissions impact on aerosol concentrations, and radiative impact. The paper shows the significant impact of fire emissions on the atmosphere's radiative budget. The paper would be worthwhile for publication in ACP if the following major remarks were properly taken into account, in particular: âĂć a sound argumentation on the choice of several parameters (in particular fire intensity), âĂć a better description on how optical properties and aerosol-cloud interactions are calculated in the model.

MAJOR REMARKS: Section 2.1, page 5, lines 13 – 20. The parameters used in this study to obtain the lower and upper bounds of the plume height need to be much

better justified, in the context of available fire studies. When reading the paper, one could think that the two upper and lower values given for fire intensity (30 and 80 kw/m2) represent a common range of observed values. But then these values are not really used n a statistical sense, but rather in a deterministic way to calculate lower and upper plume heights for a given fire. Isn't there a conceptual mismatch. Also the values chosen for the limiting vertical velocity and default fire size need justification. For all values, how would altering them with respect to their estimated uncertainty ranges alter the results of this paper. Some sensitivity tests would be welcome here.

Section 2.4, page 7: This section is difficult to read, because the aim of the argumentation is not clear from the beginning on. The last sentence, that the authors were unable to perform Mie calculations for this study, and thus took values for diesel soot instead of wood soot should be put right in the beginning of the section. Potential implications of this approximation should be discussed all along the paper, in particular in section 3.5 (radiative effects). Fire aerosol is also constituted of organic aerosol. Which optical properties are adopted for organic aerosol ? Is internal or external mixing assumed for different fire aerosol components ? This should be stated. Only one reference for one wavelength is given for the single scattering albedo of diesel and wood. I guess that there are much more results available in literature. Please synthesize. Optical parameters of soot have been shown to change with plume age (for example review of Bond et al., 2013). This effect is not considered in the present study. This point should at least be discussed. Please also discuss, how specific information on size distribution would ideally be used for Mie calculations, and how this was handled in the present study. Again, what is the expected error ?

Additional section 2.5: Please describe, how aerosol microphysics interactions are treated in the model, which processes and parameterizations are included? This is crucial for enabling the reader to understand results presented in Section 3.5 (Aerosol radiative impact).

Section 3.4 : The arguments given for stating that the VARHEIGHT simulation is the best are to some extent convincing. Nevertheless, the given data set is quite restricted, are there more observations available ? For instance in-situ PM measurements at surface sites? MODIS or POLDER AOD fields ? Is it possible to put the discussion on a more quantitative basis (for example by calculation of correlation coefficients between simulations and observations? It should be mentioned while discussing results in section 3.4, that differences between simulations and observations could be due also to errors in fire intensity and emissions. In how far do such errors prohibit from drawing conclusions on the different plume rise schemes. Overall, section 3.4 is quite difficult to follow, may be it is possible to simplify, and not give all numbers. Those could be grouped together in a table.

Section 3.5 would be strengthened, if simulated effects on short wave radiation, temperature and cloud cover could be substantiated by observations, for the given case study. This should be possible from meteorological in situ and satellite observations. Without observations, this section remains rather speculative.

MINOR REMARKS : Page 3, lines 7-9: Is this rapid transport to Europe due to prior vertical lifting into the upper troposphere with stronger winds. Please make this link clear in the revised text. Page 3, lines 19-23: are these arguments valid for specific cases or are they more general, please make this clear. Page 4, lines 11-19: please better argue, why this study is new with respect to older work . Page 4, model description: Is secondary aerosol formation from biomass burning emissions included in the model ? This process is for example shown to be important for Russian fires in summer 2010 (Konovalov et al., 2015). Page 5, Section 2.1: is lateral detrainment in the convective fire plume is apparently not considered ? Page 9, line 18: 'in sufficient agreement' agreement with what ? Page 11, line 32: A median mass diameter above 1μg/m3 seems large to me. It is for instance larger than the accumulation mode in which most mass of continental aged pollution aerosol is concentrated. Is there an explanation, why this is different for fire aerosol.

Tables : It would be worthwhile to add a table with emission factors for different model

species.

Figures :

Figure 2: please specify the Figure legend, for which parameter the diurnal cycle is shown ?

Figure 4: What is the meaning of the red points ? I guess the smoke area is in grey, while clouds are white. To be completely clear, this could be mentioned in the legend. How does observed smoke region compare to that simulated with different plume height options. Does such a comparison allow state on benefits of different plume height treatments?

Figure 5: It is difficult to make the "geographical" link between figures 4 and 5. In figure 5, please indicate latitudes and longitudes, or make appear the domain of fig. 4 in fig. 5.

Figure 7: How are colors attributed, it is not very quantitative ? This is mentioned in the main text, please recall it in the figure legend.

Figure 10: Aren't there any observations of short wave radiation available in the modelling domain? Is figure 10 contained in Figure 13, or is it different. It could be justified, but please indicate it.

TECHNICAL, EDITING REMARKS : Page 2, line 7: 'they developped' -> 'Konovalov et al.' or 'the authors' Page 3, line 18: 'Another simulation . . ..' In the same study/reference ? Page 3, line 18: 'the same' Which ? Page 6, line 18: 'the wind speed in the boundary layer is usually higher . . ..' Add 'usually'

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Bond, T. C., Doherty, S. J., Fahey, D. W., Forster, P. M., Berntsen, T., DeAngelo, B. J., Flanner, M. G., Ghan, S., Kärcher, B., Koch, D., Kinne, S., Kondo, Y., Quinn, P. K., SaroïňĄm, M. C., Schultz, M. G., Schulz, M., Venkataraman, C., Zhang, H., Zhang, S., Bellouin, N., Guttikunda, S. K., Hopke, P. K., Jacobson, M. Z., Kaiser, J. W., Klimont, Z., Lohmann, U., Schwarz, J. P., Shindell, D., Storelvmo, T., Warren, S. G., and Zender, C. S. : Bounding the role of black carbon in the climate system : A scientiïňĄc assessment, J. Geophys. Res.-Atmos., 118, 1–173, 2013.

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