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Interactive comment

Interactive comment on "Simulating Forest Fire Plume Dispersion, Chemistry, and Aerosol Formation Using SAM-ASP version 1.0" *by* Chantelle R. Lonsdale et al.

Anonymous Referee #2

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The manuscript by Lonsdale et al. describes a coupled plume-scale process model that combines the System for Atmospheric Modelling (SAM) and the Aerosol Simulation Program (ASP). Although both SAM and ASP have been developed and extensively used previously, their coupling is a new step. The coupled SAM-ASP model is undoubtedly a useful tool that can help the atmospheric community in studying the near-source smoke plume chemical and physical evolution that cannot be adequately represented in regional and global models. However, I find that the manuscript is too short, with multiple important points not being sufficiently addressed and explained. Furthermore, the presumed advantages of an explicit simulation of the dispersion of a smoke plume compared to a previous single-box model simulation are not convinc-

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ingly demonstrated. My specific comments and recommendations for improving this manuscript are provided below.

Specific comments

Introduction: I suggest that the authors better explain the place of their modeling tool among plume models that were available previously. A useful (albeit somewhat outdated) review of such models can be found in Goodrick et al. (2013). I also suggest that anticipated effects of unphysical mixing of biomass burning emissions within grid boxes of 3D CTMs on simulations of air pollution be explained in more detail specifically in the case of particulate matter (based, e.g., on the findings by Bian et al. (2017), Hodshire et al. (2019) and Konovalov et al. (2019)): while the authors gave some idea about these effects in the case of ozone, they did not provide any hints on how unphysical mixing can affect PM simulations.

Sect. 2: I suggest that the title and structure of this section be revised by taking into account that the goal of this manuscript is not to introduce several "models" but rather only one coupled model (SAM-ASP). I suggest specifically that Sect. 2 be entitled as the present Sect 2.3, while Sects. 2.1 and 2.2 that describe the modules (previously developed) of SAM-ASP be merged, and the present Sect. 2.3 be appropriately renamed.

Sect. 2.2: This section is a way too short. It would be helpful if the authors provided information about specific turbulence and cloud parameterizations, a possibility to model aerosol-cloud interactions, limitations associated with basic physical assumptions involved in the model, model grid and typical temporal resolution, an algorithmic language used, etc.

Sect. 2.3: This is, in my understanding, the key section of this paper, and as such, it is also a way too short. In this section, I would expect to find many technical details, such as algorithmic languages used in the model code, a numerical solver, system requirements, availability of parallel computing algorithms, flexibility of the SAM-ASP configuration, etc. I suggest that this section be extended accordingly. Could the authors also explain if the current version of SAM-ASP can be used to simulate aerosol-cloud interactions, if (and how) the wind shear is taken into account in the current Lagrangian configuration of the model, and how the mass emission fluxes can be converted into the initial conditions? I also recommend that Figure 2 from Sakamoto et al. (2016) (to which a reader is referred) be reproduced (possibly with revisions) in this paper.

Sect. 3: Can the authors consider moving the content of this section to Sect. 4?

p. 7, l. 15: Do the authors mean that the emissions are initially distributed evenly between 1200 and 1400 m? If so, could the authors comment on why, according to Fig. 2, the plume is located between 900 and 1300 m after just one hour? Is it initially propagating downward?

P. 7, l. 15, 16: I suggest that the authors explain their choice of the initial horizontal width of the plume. I see that according to Fig. 1, it was about 5 km, while the corresponding scale of the fire (covering 81 hectares) was ~ 1 km. Was the fire rectangular?

p. 7, l. 28, 29 and Fig. 2: Can the authors provide NEMR for OA with respect to CO? This will make the results for OA more consistent with the results for the gaseous species and also give to a reader a clue about the OA initial concentration (which determines the OA gas-particle partitioning).

p. 7, l. 7. Can the authors explain how they estimated the age uncertainty?

p. 8, l. 1. Can the authors discuss possible reasons for the underestimation of dispersion in the first two hours of their simulation? Does this bias depend on any options used in the SAM configurations?

p. 8, l. 23-28. The authors found that the behavior of OA at the edges of the plume is different from that near the core. However, I wonder if this difference is important when evaluating the average NEMR across the plume? Fig. 4 seems to suggest that the edge effects could indeed be significant (with respect to the evolution of the average

NEMR), but the firm conclusion is hardly possible as the CO dispersion rates in the box model and SAM-ASP are very different. I suggest therefore that the authors make an additional experiment where the CO dispersion rate in the box model is adjusted to that in SAM-ASP. A positive outcome of such an experiment will make the paper much stronger.

Sect. 5: Conclusions look unusually too concise for a GMD paper and should be considerably extended. It should be made clear, in particular, that when compared to observations, the simulation with SAM-ASP did not show any significant differences with respect to a much simpler box model simulation.

Sect. 6: In my understanding, GMD authors are normally expected to provide free access to their modeling tools. But in this case, the access is to be granted by a person who is not even a co-author of this paper. Can the authors consider providing easier access to their model?

Minor comments

p. 2, l 15: I suggest using "reviewed" instead of "described".

p. 2, l 32: CTMs do not "make" emission estimates but only use them.

p. 2, l. 26: I suggest removing the word "size".

Sect. 2.1.2: I suggest moving the description of the settings specific for the numerical experiments performed with SAM-ASP in this particular study to Sect. 4.

p. 7, l. 33: "PAN, NOx. . ."=> "NEMRS for PAN, NOx, . . ."

Fig. 3: The figure caption should mention that the box model results are adopted from Alvarado et al. (2015) (if this is so).

p. 11, l. 4, Bian H.,. . ., 2017: Is it the correct reference?

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