Reply for comments of

Mesoscale modeling of smoke transport over the Southeast Asian Maritime Continent: Coupling of smoke direct radiative effect below and above the low-level clouds

Cui Ge, Jun Wang, Jeffrey Reid

We thank the reviewers for the constructive comments to improve this manuscript. Item-by-item replies are provided below; text in bold italics shows reviewer's comments.

Reviewer 2:

This paper presents results from a series of regional climate simulations for the Southeast Asian Maritime Continent using WRFChem designed to explore the response of clouds and atmospheric dynamics to the direct radiative effect of smoke aerosols from biomass burning in the region. The paper describes novel and interesting results that are deserving of publication in ACP, however the manuscript itself is rather difficult to follow and requires substantial editing before it will be suitable for publication. The number of individual figure elements is immense and the text is correspondingly dense. Indeed, the dynamics of the system the authors discuss is complicated, however presented in its current form the paper struggles to clearly convey its main conclusions. Compounding the difficulty is considerable poor English grammar that requires substantial editing by a fluent English writer. Nevertheless, I am certain that the authors of this paper can work carefully to clarify the key dynamical processes at work in their results and improve the precision of the language used to describe them.

Thanks to reviewer for the constructive comments. This time we worked to clarify the key processes, and also a native English speaker helped us with the English grammar.

As examples of this lack of clarity in the manuscript, I offer two specific examples of important points that left me confused:

1) Is the modification of the land-sea breeze a key element of the more general results shown in figure 4 and summarized in the conceptual model? In particular, is modification of the convergence/divergence of the land-sea breeze system necessary for the weakened subsidence in the 3-6km altitude attributed to aerosol absorption? Or does the weakened subsidence merely reflect enhanced buoyancy in the 3-6km layer.

We think the modification of the land-sea breeze is one of the key elements, because over the coastal region, land (sea) breeze always interplays with other meteorology factor. And the reviewer is right about the weakened subsidence in the 3-6km altitude reflect the enhanced buoyancy in the 3-6km layer. In the 6 paragraph of section 3.2, we discussed it as "It should be noted that dynamics and radiative effects are coupled; the warming by smoke particles confined over the smoke source region in the morning (10:00 LT) can result in local convergence and produce an updraft (buoyancy) above PBLH, which in turn transports more smoke particles above, and thus renders a positive effect." About the enhanced updraft in the middle atmosphere, we don't think it is due to the change of sea breeze. While we think for the surface convergence/divergence over the south part of Borneo at daytime/nighttime, the change of sea/land breeze is a main reason. And now we make several changes in the manuscript to void misleading reader.

2) Is the change in free-tropospheric precipitable water a direct consequence of the changes in the vertical motion induced by aerosol radiative effects (as argued on page 15460), or related to larger-scale regional dynamics (as argued on page 15457, line 6)?

The free-tropospheric precipitable water itself should relate to larger-scale regional dynamics that talked in Reid et al. 2013. If we compare the preciptable water with the one from the same time period of other year, we can find the large scale dynamics impact on that. While here, Fig. 4 shows the difference due to the aerosol radiative effect, so we think the change in free-tropospheric precipitable water is a direct consequence of the changes in the vertical motion and entrainment of drying induced by aerosol radiative effects (please see replies to the Q1 raised by the first reviewer).

Other items that require clarification:

3) Key finding number 1 (from the enumerated list in section 7; also mentioned in the abstract) is that low-level cloud enhances atmospheric absorption by smoke. This is most likely true, but not quantitatively demonstrated in the manuscript. What the figures show is that in all-sky conditions the top-of-atmosphere radiative forcing is positive. All this means is that the extinction by aerosols makes the scene darker when viewed from above than it would be in the absence of the aerosols. The difference in the TOA forcing between all-sky and cloud-free conditions discussed in the manuscript could be entirely a consequence of the difference in albedo of the scene beneath the smoke, even with the same magnitude of atmospheric absorption. Indeed, it is likely that enhanced reflection from the cloud layer enhances absorption in the atmosphere because of the additional component of upward reflected sunlight passing back through the smoke layer. But this is not quantified in the authors' analysis. This could be demonstrated by showing the difference in the atmospheric absorption between allsky and cloud-free conditions, in which the conclusion could stand as is (assuming the calculation backs it up). Or this conclusion should be reworded to say that net absorption of the surface/atmosphere column is enhanced because the smoke

resides above a bright surface (i.e. were it not for the clouds a large component of the solar radiation would have been absorbed by the surface regardless of the aerosol load).

Now we reworded this conclusion as the reviewer suggested in the abstract and conclusion part. This is indeed a better presentation, and we think now it is consistent with our description about Fig. 1 in section 3.1.

4) The figure captions for the panels showing differences induced by the aerosols say "aerosol minus no-aerosol". But this cannot be the actual methodology used because one of the figures shows the change in PM2.5 mass between the simulations. Therefore there must be some aerosol in the "no-aerosol" case. I presume that the authors meant the difference between a simulation applying radiative interaction with aerosols and a simulation without radiative interaction. The manuscript needs to be clear about this and use precise language throughout to describe exactly what difference is depicted.

Now we changed '$V_{fd} – V_{non\text{-}fd}$' to '$V_{Ra} – V_{non\text{-}Ra}$' which Ra means aerosol radiative interaction. Also, please see replies to Q4 raised by the first reviewer.

5) In a related note, the word "feedback" is often misused in the literature and so it is throughout this manuscript as well. A feedback occurs when a specific change in a system leads to a response that further modifies the original change. So in table 2 where a row labeled "feedback" seems to mean that the radiative interaction with aerosols is on or off (although again, this requires clarification), this word is being misused. In fact, the radiative interaction is merely that, not a feedback. The response may induce a feedback, but that is internal to the dynamics, not a switch that the authors can turn on and off. It could be argued that figure 4 b,f,j and n depict a feedback where the radiative interaction with aerosols modify the aerosol distribution. I would probably be willing to let that slide,

but in a strict sense, I'm not sure that even qualifies as a true feedback. A true feedback would be where the addition of radiative interactions with smoke aerosols changed the amount or the radiative effect of the smoke aerosols. I'm not sure that the manuscript shows any evidence of that.

Thanks for the reviewer, that's right when we talk about aerosol feedback, it actually should be 'aerosol radiation interaction', and more specifically it is direct radiative effect in our study. Now we corrected this throughout the manuscript, include title, captions of the figures, and Table 2.

6) The figures containing more than 4 panels are entirely illegible when the paper is printed out. Maybe this is not an issue for an electronic journal where one can zoom in on the figures on the computer screen. This should be an issue that the editor of the journal should weigh in on. Does the journal have a policy on the minimum size o fpictures or text in a figure? I am guessing that if it does, this manuscript runs afoul of it.

Thanks. We removed some panels of less significance, also we moved some figures to supplementary online material (SOM). We removed some panels of less significance. For example, we moved 3 panels of Fig. 1 to Fig. S1 (in SOM). We removed m-p panels from Fig. 4. For Fig. 5, we removed a-d panels. And in Fig. 7 we only keep those panels associated with low-level cloud and surface wind. Also we moved Figure 12 and 13 to supplementary material, and summarized major points in the main text.

7) Figure 5 shows a change in PM2.5 but does state at what elevation this concentration is evaluated. Is this PM2.5 changes in the boundary layer? Or in the 1-2km layer? Or the 2-3km layer? This is obviously crucial to the clarity of the argument since the authors are

arguing that smoke absorption substantially redistributes the smoke concentration vertically.

Now we clarify the $PM_{2.5}$ is ‘surface $PM_{2.5}$’ in the caption of Fig. 5.

8) There is an interesting difference between the vertical redistribution of aerosols and the redistribution of moisture discussed by the authors. This is interesting because crudely speaking, both constituents are emitted by the surface and mixed vertically by turbulence. Thus the notion that the moisture is trapped by enhanced stability of the boundary layer while the aerosols are not seems to rely critically on the injection height of the smoke. Is there independent validation of the injection height from in-situ or remote sensing observations? Is there an uncertainty range of that injection height? If one were to set up a sensitivity study using different injection heights within the range of observational uncertainty would the differences between smoke mixing and moisture mixing be robust?

About the smoke injection height, we described it in the introduction part. In the paper of Wang et al., (2013), we did sensitivity experiments about the smoke aerosol injection height for the same time period of this paper. And we found a good agreement between simulation from WRFchem and satellite/ground-based observations in terms of surface $PM_{2.5}$ mass, aerosol vertical profile, and smoke transport path when FLAMBE emission is injected within 800 m above surface. So based on Wang et al. (2013), we use 800m as the smoke injection height in this paper. About the change of precipitable water, the change of entrainment of drying induced by aerosol radiative effects is another reason (please see replies to Q1 raised by the first reviewer).