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

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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 1:

General comments:

This study uses the online-coupled WRF-Chem model to simulate radiative impacts and atmospheric feedbacks of biomass burning smoke over the Southeast Asian Marine Continents (MC). Although this is generally a model sensitivity study without much evaluation by observations, they found a suite of interesting mechanisms that smoke aerosols affect radiation budget, atmospheric boundary layer processes, meso-scale circulations (land/sea breezes), and aerosol vertical distribution. The results seem to be plausible in general and will add useful piece to the discussion of this interesting topic. Generally authors have done a nice job in presenting and interpreting complex results. I would recommend the paper be published in ACP after they further improve the paper. Here are some comments for them to consider when doing revision.

Specific comments:

1. Discussion on PBL temperature and moisture changes induced by smoke radiative

effects has focused on perturbations of surface energy budget and atmospheric radiative heating. What is missing in the interpretation is the role of entrainment processes near the top of PBL. Yu et al. (2002) look into the contribution of entrainment processes based on idealized PBL simulations.

In section 3.2, we added the discussion about entrainment of drying as follows. "The perturbation of entrainment of drying could be another reason for the change in water vapor. Yu et al. (2002), based on the idealized simulations from a high-resolution and one-dimensional boundary layer model, found that aerosols with strong absorption elevated above PBL can lower the top of PBL and hence reduce the entrainment heating and moisten the PBL. Yu et al. also suggests the implications of these results could be on the cases such as smoke from biomass burning or mineral aerosols from a dust storm. It is good to see Yu's conclusion performed well in our study. Over fire area during daytime (Fig. 4d and h), decreased PBLH can reduce the entrainment drying and increase PW within PBLH. Right above PBLH (about 1km), because the warmer air induces a slight updraft, the entrainment of drying could be enhanced and so PW would be decreased. When the smoke aerosol became more absorbing (with OC/BC is 3.5), such perturbation of PW within or above PBLH during the daytime became more prominent as shown in Fig. S2 a and b." Fig. S2 was added in supplementary online material.

2. The study proposes a conceptual model based on a month-long simulation for 2006. Will the conceptual model still hold for other years? It is reasonable to expect that smoke radiative effects and atmospheric feedbacks may change from year to year. One example is observed different changes of cloud fraction associated with smoke in Amazon (Koren et al., 2004; Yu et al., 2007). I would suggest that some discussion be added in the paper on

possible interannual variability of smoke radiative effects and atmospheric feedbacks. Readers should find the paper more insightful if it has a discussion on how the smoke impacts in MC region may be similar to and/or different from that in other regions, such as Amazon (e.g., Bevan et al., 2009; Wu et al., 2011; Zhang et al., 2008).

Now we add some discussions about some related studies in Amazon and also some possible interannual variablility. And many thanks for all the references recommended by the editor.

We added the following in section 3.1,

"Similar results were found over the Amazon region during the dry season (Koren et al., 2004), where satellite data showed that scattered cumulus cloud cover was reduced by smoke particles, and this response can reverse the regional smoke instantaneous forcing of climate from -28 Wm^{-2} in cloud-free conditions to 8Wm^{-2}."

In section 3.3, we added the following,

"The change in cloud fraction is consistent with past studies. For example, the dominant effect of the aerosols to reduce clouds and precipitation in the afternoon was found in Wu et al. (2011) when they studied the biomass burning event in the dry season of South America. Koren et al. (2004) reported that scattered cumulus cloud cover over the Amazon region can be reduced by 38% due to smoke semi-direct effects. Zhang et al. (2008) did ensemble simulations about the impact of biomass burning aerosol on land-atmosphere interactions over the Amazon, and found cloudiness decreases in early afternoon due to the absorption of solar radiation by smoke aerosols."

In section 5, we added the following,

"In addition, large inter-annual variations of fire activities were found over the southeastern Asia region, although high cloud cover in this region is persistent even in the dry season (Reid et al., 2011). Hence, we expect that the conceptual model in Fig. 12 can be generalized for other years in this region, although year-by-year variations of smoke can be more likely than that of clouds to lead to variations of the strength of each process in the conceptual model. Caution needs to be taken in applying this conceptual model to other tropical biomass burning regions such as the Amazon forest where significant change in cloud cover and resultant change in smoke radiative effects can be found between drier and wetter years (Yu et al., 2007)."

3. The paper in its current writing presents monthly average results first and then shows a case study for October 31 2006. I don't find any significant value this case study (section 5 with Figure 12 and 13) adds to, except that much larger magnitude of perturbation was induced by smoke in this case than monthly mean. They may want to consider moving the case study to supplementary online material (SOM) and summarizing major points in the main text. If they prefer to keep the case study, I believe it is more appropriate to first present the case study in detail and then briefly show the monthly mean.

Now we move Figure 12 and 13 to supplementary material, and summarized major points in the main text.

4. For many figure captions, they show the perturbation as a difference between "aerosol" and "non-aerosol". What does this really mean? Does "aerosol" refer to "aerosol with feedback" and "non-aerosol" to "aerosol without feedback"? Now that they have Table 2 listing the experiments for this study, it would be easier for readers to follow if they can clearly state in caption and/or main text which experiment(s) have been used to generate

the panels.

‘aerosol’ and ‘non-aerosol’ mean ‘simulation with considering aerosol radiative interaction’ and ‘simulation without considering aerosol radiative interaction’. To be easier for readers to follow, we clarified this, and replaced ‘aerosol’ with ‘Ra’, and replace ‘non-aerosol’ with ‘non-Ra’ through out the manuscript and the figure captions.

5. The paper has 14 figures. However in most cases, each figure has several panels with baseline and perturbation results mixed. The size of figure is quite small in many cases. All these make reading less pleasant. I would suggest that they move some panels of less significance to SOM or even remove some. For example, they may consider moving f, g, h, i, and l panels in Fig. 1 to SOM. In Fig. 4, m, n, o, and p panels can be removed. For Fig.5, you can either keep a-d or e-h panels. For Fig.7, they may consider keeping just those panels associated with low-level cloud and surface winds.

We removed some panels of less significance and also some figures to supplementary online material (SOM). 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.

Technical corrections

I would suggest that they have a native English speaker to read through the paper carefully and correct some errors.

This time, a native English speaker helped us with the manuscript.

p.15444, l23: add “by” after “is reduced”.

Thank you. We added it.

p.15445, l11: "the decreased sea breeze". Not clear.

'the decreased sea breeze' is changed to 'the weakened sea breeze'.

p.15445, l22: add "by" after "characterized".

Thank you, we added it.

p.15445, l25: add near-surface" before "PM10"

Thank you, we added it.

p.15446, l4: spell out MODIS.

We spelled out it as "Moderate Resolution Imaging Spectroradiometer (MODIS)"

p.15446, l20-21: spell out ENSO, ITCZ, and MJO.

Thank you, we spelled out all the terms in the manuscript.

p.15447, l3: spell out CALIOP.

Thank you, we spelled out CALIOP as the Cloud-Aerosol Lidar with Orthogonal Polarization.

p.15448, l18-20 and elsewhere: please make sure to use "OC/BC ratio" or "BC/OC ratio" consistently throughout the paper.

Thank you, now we corrected all of them as OC/BC ratio throughout the paper.

p.15448, l27: change "the seasons most significant events" to "the most significant events during the season".

Thank you, we changed it.

p.15449, l8-10: what are refractive indices for OC and BC in other wavelengths? DO you consider absorption in the UV by OC?

We checked the code again, and now we updated for the information of refractive indices. "According to the database compiled by Barnard et al. (2010), and also as described in Zhao et al. (2010), the shortwave refractive index for BC and OC is not wavelength dependent in this study. The refractive index of BC in this study is assigned the value of 1.95 + i0.79 for both shortwave and longwave. The refractive index of OC (dry) is 1.45 for shortwave, and for longwave it is in the range of 1.22-2.50 (real part) and 0.01-0.5 (imaginary part).” And in the optical module of WRF-chem, the absorption in the UV by OC is not considered.

p.15449, l12: you need to explain “hydroscopicity” or what “0.14” means.

Now we explain it in the manuscript with ‘The hygroscopicity (size growth factor) is assumed to be 0.14 for OC and a very small nonzero value (10^{-6}) for BC (Ghan et al., 2001a), and hence the wet mode radius for BC can be diagnosed from RH, hygroscopicity and other related parameters.’

p.15449, l19: delete “overwhelm”.

We deleted it. Thanks.

p.15450, l2: “Wang, 2013” should be “Wang et al., 2013”.

We corrected it.

p.15450, l1: do you assume the emissions are uniformly distributed in 0-800m layer?

We added ‘and within this injection height the emissions are uniformly distributed’

p.15450, l9, “luck” should be “lack”.

We corrected it.

p.15451, l7-11: What is the remaining 10% of the total smoke particle mass? How do they account for its radiative properties in the model? When I assumed that 100% particle mass is POM and BC, I got the respective BC/particle mass ratio of 6.25%, 16.01%, and 3.77%, 6.25% for the baseline, S1, S2, and S3 experiment, which is somewhat different from that shown in your Table 2. I finally realized that the difference could be reconciled by taking

the "90%" into account.

We added 'The remaining 10% of the total smoke particle mass is not considered in the simulation since the uncertainty of the optical property of those masses could be quite large.'

p.15451, l25-26: while they may use all-sky and clear-sky difference to explain WRFChem and MODIS AOD difference, it is also necessary to remind readers that their WRF-Chem simulations only consider biomass burning smoke. What is contribution by non-smoke aerosols in the region?

Now we added more information as following. "Another reason for smaller simulated AOD is that only smoke particle emissions were considered in the model. Based on the model simulation by Xian et al., (2013) that considers non-smoke aerosol sources, we expect that the maximum contribution from non-smoke AOD on the average should be ~0.1 in our study."

p.15452, l1-4: it is better to define SWDRF here. e.g., based on what two simulations listed in Table 2. Does SWDRF include radiative perturbations induced by cloud feedbacks? State clearly what positive value means and what negative value means. Many studies define SWDRF with respect to net downward SW flux at TOA, which has a sign that is opposite to your definition.

Here we stated the definition of SWDRF in our study as following. "The SWDRF here is the net downward SW flux difference at TOA between the simulation which considers smoke radiative effect and the simulation that does not consider it (similar to the definition in Zhao et al. 2010)." And since SWDRF here include both direct and semi-direct smoke aerosol radiative effect, the perturbation of clouds could do some contribution to SWDRF, while the detailed study of that is beyond the scope of this study. And we know, also as the editor said that the aerosols usually exert a negative forcing at TOA because scattering solar radiation. So we give the explanation in the same paragraph of the manuscript. We think the net absorption of the surface/atmosphere is largely enhanced when smoke resides above clouds, hence it led to a warming effect at the top-of-atmosphere (TOA).

p.15452, l4: "different with usual case", what do you mean?

We removed 'different with usual case' since it is not clear. We want to say, the aerosols usually exert a negative forcing at TOA (also as the editor said) because scattering solar radiation. In this study we see a warming effect instead of cooling effect of aerosol.

p.15452, l9: the single scattering albedo of 0.9, at what wavelength? Do they have measurements from the 7-SEAS campaigns to evaluate the model result?

The single scattering albedo is for 600nm, and we added the information in the figure caption. Currently no measured data are available for us to evaluate the model results.

p.15453, l19: why do they find "It is interesting"? Doesn't this simply reflect the wellknown effects by clouds? Clouds reflect solar radiation to the space thus reduce the radiation reaching the surface.

We want to say, it is nice to see the impact of sea breeze on clouds and hence the distribution of GSW, while we did not make it clear. So now we removed 'it is interesting'.

p.15453, l21: "coast" should be "coastal".

We corrected it.

p.15454, l3: "different" should be "difference"

We corrected it.

p.15454, l16-19: is there any cloud spatial inhomogeneity that explains the patterns of TOA outgoing SW and GSW?

We believe cloud spatial inhomogeneity has certain impact on the patterns of TOA outgoing SW and GSW. While currently the radiation module used in this study did not consider cloud spatial inhomogeneity.

p.15455, l17: add "by" after "decreased".

We added it.

p.15456, section 3.2: Can they explain why PBLH is high over ocean near the northern boundary of the domain (Fig. 3a)? They try to link variations of PBLH with that of 2m air temperature. But it is more appropriate to link PBLH with surface sensible heat flux and the capping inversion.

Now we related PBLH with sensible heat, and also explain that 'And also the nearby ocean of the south Kalimantan has high PBLH due to less cloud cover and a warmer surface'. We do find the decrease of surface temperature and the increase of heating rate in the atmosphere due to smoke absorption, while in monthly average we didn't found the capping inversion in our study region. It could occur in certain vertical level during the big smoke event that we may do some analysis in our future work.

p.15456, l9: remove "It is interesting to".

We removed it.

p.15456, l13: add "layer" after "boundary".

We added it.

p.15456, l14: "efficient transport of heat in the atmosphere". Could they please elaborate the point a little bit?

Now we reworded it as 'efficient mixing of heat in the atmosphere above PBLH'.

p.15457, l4: "move" should be "moving".

We corrected it.

p.15457, l7: "suppress" should be "suppresses".

We corrected it.

p.15458, l6-9: I guess that the wind vector in Fig. 4 represents u-w wind speed. Please clarify in figure caption. Currently "wind speed" is causing confusion.

The editor is right, now we change it to 'u-w wind speed' in the caption of Fig. 4.

p.15458, l8: "transporting" should be 'transport".

We corrected it.

p.15459, l9: "alternation" may be better than "rotation".

We changed it.

p.15459, 2nd paragraph: where is Borneo? I don't see from Fig 5 that PM2.5 increases at 16 LT but decreases at 00LT. Maybe I missed something.

We added (the location of Borneo Island can be seen in Fig. 1c). And 'increase/decrease' is a typo, now we corrected it with 'decrease/increase'.

p.15459, l22: delete "from".

We deleted it.

p.15461, l19: "sunrises" should be "sun rises".

We corrected it.

p.15462, l17: "Korean" should be "Koren" .

We corrected it.

p.15464, l1: Could they explain why AOD changes slightly with OC/BC ratio?

We discussed it more with the following sentences. "When the OC/BC ratio changed from a smaller to a larger value, the total mass of OC and BC is unchanged, meaning scattering aerosols increased and absorbing aerosols decreased. When the OC/BC ratio changed from 17 to 3.5, both AOD and AAOD increased with the large value of 0.20 (for AOD) and 0.24 (for AAOD) around 20:00 LT (Fig. 9a)."

p.15464, l4: please be more specific about "the smoke source region".

Now we specified the 'smoke source area' in the 3rd paragraph of section 3.1 as "(the area where the monthly averaged AOD is larger than 0.5 in Fig. 1a)".

p.15465, l12: "Interesting" should be "Interestingly".

We corrected it.

p.15465, l26: "0.6km above ground", doesn't seem to be consistent with what Fig. 11a shows.

We re-writed this sentence as 'Most smoke aerosol can be found within 2km above surface.'

p.15468, l6: "As a result, PBLH decreases…" But this is not clearly shown in Fig. 4.

The nighttime PBLH decrease is quit small to see from the figure. Now we reworded it and also it is consistent with Fig. 5 as following "At night, the land surface temperature is decreased due to the smoke radiative effect during the day. Divergence occurs over the south part of Borneo (Fig. 5) with an enhanced land breeze, hence the downdraft near the surface is also enhanced. Consequently, $PM_{2.5}$ increases near the surface but decreases in the middle-to-upper part of PBL." Also please see replies for Fig.14 (raised by the first reviewer).

p.15468, l16: "weak" should be "weaken".

Now we changed 'weak' to 'weaken'.

Fig. 4: "5:00pm and 12:00pm": should be "5:00pm and 12:00am". Anyway it is better to use "17LT and 24LT" just for consistency.

Now we use LT instead of PM for consistency.

Fig. 5: Please explain what is "anomaly of surface wind" in (a), (c), (e), and (g)? The

wind fields at 16LT and 00LT are substantially different from the daytime and nighttime average, respectively. This needs some explanation.

To explain 'the anomaly', we add 'The anomaly of surface wind is the difference between the wind at certain local time and the wind of monthly mean.' to the caption of Fig. 5.

Fig.7: for (k) and (l), add a wind vector showing the magnitude of wind speed.

Now we added the wind vector to show the magnitude.

Fig. 8: specify the wavelength for SSA.

We added 'in 600 nm' for SSA in the caption.

Fig. 9: There is only one red dashed line in (b) – (h). Need to specify what it represents in caption.

To clarify the caption, we did the following edits in the caption of Fig. 9. :

In (b)-(h), the dotted red lines show variation of the variable (V) with OC/BC ratio is 10 and without consideration of smoke radiative feedback, and the 3 solid lines show the difference of the variable ($\Delta V = V_{areosol} - V_{non\text{-}aerosol}$) with different OC/BC ratio. OC/BC is 3.5 (Black line), 10 (Red line) and 17 (Green line) respectively.

Fig. 12: what is shown in (o)? Is it the percentage change of low-level cloud fraction?

Now Fig. 12 is Fig. S4 in supplementary online material. We added 'in percentage' to specify the figure, now it changed to '(o), The difference (in percentage) of low-level cloud fraction.'

Fig. 13: the caption for (g) is wrong. Could you please explain why T at 2200 m decreases when the smoke layer is more absorbing?

Now we move it to supplementary online material (SOM) as Fig. S5. We checked the plotting code for the figure and found we made a mistake, the ΔT should be $T_{oc/bc=10} - T_{oc/bc=17}$

while last time we use $T_{oc/bc=17} – T_{oc/bc=10.}$ While other figures in Fig. S5 are right. And we should notice that in Fig. S5, ΔT is $T_{oc/bc=10} – T_{oc/bc=17}$ instead of T_{Ra} - $T_{non\text{-}Ra}$. Here we want to see the relative change between different OC/BC ratios. When OC/BC ratio is 10, the smoke aerosol is more absorbing, so the radiative effect is more prominent compared the one with OC/BC ratio is 17.

Fig.14: Why does nighttime PBLH decrease? In the diagram, nighttime PBLH is similar to daytime value. Does this really make sense? My understanding is that nighttime PBL is much shallower than daytime PBL. Also using upward and downward arrow to describe change of land/sea breeze is confusing. They may simply use "weakened sea breeze", "strengthened land breeze".

The editor is right, the nighttime PBLH decrease is quit small to see from the figure. And it is right that nighttime PBL is much shallower than daytime PBL. We did some change through out the manuscript. And also we did the change on the figure according to the editor's suggestion.

References:

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Many thanks for all the references recommended by the editor. We try to digest all of them and also include the main points in the related discussions.