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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Mesoscale modeling of smoke transport over the Southeast Asian Maritime Continent: coupling of smoke direct radiative feedbacks below and above the low-level clouds" *by* C. Ge et al.

Anonymous Referee #1

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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.

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).

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

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show the monthly mean.

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.

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.

Technical corrections

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

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

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

p.15445, l22: add “by” after “characterized”.

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

p.15446, l4: spell out MODIS.

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

p.15447, l3: spell out CALIOP.

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

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

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

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

p.15449, l19: delete “overwhelm”.

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

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

p.15450, l9, “luck” should be “lack”.

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.

p.15451, l25-26: while they may use all-sky and clear-sky difference to explain WRF-Chem 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?

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

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that is opposite to your definition.

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

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?

p.15453, l19: why do they find “It is interesting”? Doesn’t this simply reflect the well-known effects by clouds? Clouds reflect solar radiation to the space thus reduce the radiation reaching the surface.

p.15453, l21: “coast” should be “coastal”.

p.15454, l3: “different” should be “difference”

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

p.15455, l17: add “by” after “decreased”.

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.

p.15456, l9: remove “It is interesting to”.

p.15456, l13: add “layer” after “boundary”.

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

p.15457, l4: “move” should be “moving”.

p.15457, l7: “suppress” should be “suppresses”.

p.15458, l6-9: I guess that the wind vector in Fig. 4 represents u-w wind speed. Please

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clarify in figure caption. Currently “wind speed” is causing confusion.

p.15458, l8: “transporting” should be ‘transport”.

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

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.

p.15459, l22: delete “from”.

p.15461, l19: “sunrises” should be “sun rises”.

p.15462, l17: “Korean” should be “Koren” .

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

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

p.15465, l12: “Interesting” should be “Interestingly”.

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

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

p.15468, l16: “weak” should be “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.

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.

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

Fig. 8: specify the wavelength for SSA.

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

Fig. 12: what is shown in (o)? Is it the percentage change 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?

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”.

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Interactive comment on Atmos. Chem. Phys. Discuss., 13, 15443, 2013.