Atmos. Chem. Phys. Discuss., 13, C5543–C5546, 2013
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Atmospheric Chemistry and Physics Discussions

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ACPD

13, C5543–C5546, 2013

Interactive Comment

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 #2

Received and published: 6 August 2013

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 pre-

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Interactive Discussion

Discussion Paper

sented 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.

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.

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)?

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 all-sky 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).

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.

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

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not sure that the manuscript shows any evidence of that.

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 of pictures or text in a figure? I am guessing that if it does, this manuscript runs afoul of it.

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.

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?

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