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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Examination of effects of aerosols on a pyroCb and their dependence on fire intensity and aerosol perturbation using a cloud-system resolving model" *by* Seoung Soo Lee et al.

Anonymous Referee #2

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First of all, we appreciate the reviewer's comments and suggestions. In response to the reviewer's comments, we have made relevant revisions to the manuscript. Listed below are our answers and the changes made to the manuscript according to the questions and suggestions given by the reviewer. Each comment of the reviewer (in black) is listed and followed by our responses (in blue).

Review of: "Examination of effects of aerosol on a pyroCb and their dependence on fire intensity and aerosol perturbation using a cloud-system resolving model"

Authors: Seoung Soo Lee, George Kablick III Zhanqing Li

Recommend major revisions.

General comment:

This manuscript examines the impacts of fire intensity and aerosol concentration on the strength of convection, microphysics processes, and upper level moisture through simulations using spectral-bin microphysics. I find that much of the paper is not particularly novel since it's fairly well known as this point that aerosol effects tend to be muted with increasing strength of convection. However, details regarding the impacts on microphysics processes are insightful. I found the most novel portion of the paper to be the final result regarding the fact that when a weak fire produces weaker aerosol emissions, the results tend to be muted. I think the paper needs to focus more heavily on the more novel aspects of the work.

In general, the paper needs to be greatly shortened. It is far longer than a typical journal article and needs to be made more concise, particular since it's a follow-on study. There are many places in the paper where the language is too "wordy". Many sentences and statements are written in a way that is difficult to read and can get in the way of representing the scientific results. Examples are given below in the specific comments section of this review.

We, authors, revised the manuscript based on this comment particularly by focusing on shortening the manuscript. To shorten the manuscript, we removed unnecessary text and figures not only by the following comments below but also by our own decision.

Specific comments:

Title: The title is too long. You could remove "using a cloud-system resolving model".

Done.

1. Lines 63-64: The changes in cirrus clouds altering the radiation budget has become a true but very common motivating factor for cloud microphysics related research as it applies to climate change. Your main motivation here is that pyroCbs with high aerosol loading can change climate. However, pycoCbs are a subcategory of deep convection that comprises a very small percentage of actual deep convective storms and cirrus anvils. As such, I think you need to improve your motivating statements for this work.

The following is added:

(LL65-74 on p3)

The level of our understanding of impacts of pyroCbs on water vapor and cirrus clouds in the UTLS over the global scale is very low and studies to improve this understanding has been going on (Fromm et al., 2010). However, this paper does not focus on these pyroCb impacts at the global scale. Instead, this paper aims to gain a process-level understanding of mechanisms that control impacts of individual pyroCbs on water vapor and cirrus clouds in the UTLS. The examination of these mechanisms can provide useful information to parameterize interactions among pyroCbs, water vapor and cirrus clouds in climate models. Hence, this examination can contribute to studies that try to improve our understanding of the global-scale impacts of pyroCbs on water vapor and cirrus clouds by using climate models.

2. Line 114: Here you state that using a CSRM allows you to have “confident information” on aerosol effects. I think this assumption is a bit premature given that you have not yet discussed the model you are using or the microphysics parameterization and its capabilities. Some microphysics schemes do not necessarily provide “confident information”. Perhaps you should first offer some assessment of your choice of model schemes being used.

Text pointed out is revised as follows:

(LL120-140 on p5)

These simulations are for a case of a pyroCb which is identical to that in Kablick et al. (2018), and performed by using a cloud-system resolving model (CSRM) which is able to resolve cloud-scale dynamic and thermodynamic processes. By resolving these processes that play a critical role in the development of clouds and their interactions with aerosols, we are able to obtain information on aerosol effects on the pyroCb development and its impacts the UTLS water vapor and cirrus clouds, and on associated dynamic and thermodynamic mechanisms. This information is likely to be more confident than that from a model that does not resolve but parameterize those cloud-scale processes. The basic modeling methodology in this study is similar to that used by Kablick et al. (2018). However, this study uses a more sophisticated microphysical scheme, i.e., a bin scheme, rather than the two-moment bulk scheme used by Kablick et al. (2018). Through extensive comparisons between various types of bin schemes and bulk schemes, Fan et al. (2012) and Khain et al. (2015) have concluded that the use of bin schemes is desirable for reasonable simulations of clouds, precipitation, and their interactions with aerosols. This is because the bin scheme explicitly predicts cloud-particle size distributions, while the bulk scheme prescribes those size distributions. The bin scheme also uses collection efficiencies and terminal velocities varying with varying cloud-particle sizes to emulate this variation in reality, while the bulk scheme in general uses fixed efficiencies and terminal velocities, which are not able to consider the variation of collection efficiencies and terminal velocities in reality. This makes the bin scheme more sophisticated than the bulk scheme.

3. Introduction: The introduction appears quite short on references. Other work has been done on pyroCbs and several additional relevant papers should be referenced.

The following references are added:

Fromm, M., D. T. Lindsey, R. Servranckx, G. Yue, T. Trickl, R. Sica, P. Doucet, S. Godin-Beekmann, et al. (2010), The untold story of pyrocumulonimbus, B. Am. Meteorol. Soc., 91 (9), 1193, doi:10.1175/2010BAMS3004.1.

Peterson, D., M. Fromm, J. Solbrig, E. Hyer, M. Surratt, and J. Campbell (2017), Detection and inventory of intense pyroconvection in western North America using GOES-15 daytime infrared data, J. Appl. Meteorol. Clim., 56 (2), 471-493.

Pumphrey, H., M. Santee, N. Livesey, M. Schwartz, and W. Read (2011), Microwave Limb Sounder observations of biomass-burning products from the Australian bush fires of February 2009, Atmos. Chem. Phys. , 11 (13), 6285-6296.

4. Line 160: What is your size distribution of IN? You discuss this here but there's no other mention of heterogeneous ice nucleation in the paper that I recall seeing.

The following is added:

(LL251-258 on p9)

Those studies have indicated that in general, median aerosol diameter and standard deviation of the distribution range from ~0.01 to ~0.03 μm and from ~2.0 to ~2.2, respectively, for aerosols that act as CCN. By taking the approximate median value of each of these ranges, median aerosol diameter and standard deviation of the adopted unimodal distribution of aerosols as CCN are assumed to be 0.02 μm and 2.1, respectively, for the control run. Following Seinfeld and Pandis (1998) and Phillips et al. (2007), for aerosols that act as IN, median aerosol diameter and standard deviation of the unimodal distribution are assumed to be 0.1 μm and 1.6 that are typical values in the continent.

The heterogeneous ice nucleation is calculated by using temperature and supersaturation in addition to IN size distribution. This is reflected as follows:

(LL180-182 on p7)

In these parameterizations, contact, immersion, condensation-freezing, and deposition nucleation paths are all considered by taking into account the size distribution of IN, temperature and supersaturation.

5. Line 199: At 150/cm3 this seems overly clean for representing a continental type case. Can you justify your choice here?

Yes, it is rather low concentration for the continental type case. However, this concentration is frequently observed in remote continental sites including the forest site adopted in this study, according to Pruppacher and Klett (1997), Seinfeld and Pandis (1998), Reid et al. (1999), Andreae et al. (2004), Reid et al. (2005), Luderer et al. (2009). These references are included in text

6. Section 2: Please provide more description of how aerosols are treated in the model. Are they transported around the domain after initialization? Is there nucleation scavenging and precipitation scavenging? Does the fire continue to act as an aerosol source after initialization or do the initial aerosols just get depleted? Each of these effects can impact the interpretation of the results. A fire continually producing aerosols

over time.

The following is added:

(LL263-274 on p9-10)

Aerosols are diffused and advected by air flow in clouds. After activation or captured by precipitating hydrometeors, aerosols are transported within hydrometeors and removed from the atmosphere once hydrometeors that contain aerosols reach the surface. It is assumed that in non-cloudy areas, aerosol size and spatial distributions are set to follow the background counterparts which are set at the first time step. In other words, once clouds disappear completely at any grid points, aerosol size distribution and number concentration at those points recover to the background counterparts. This assumption has been used by numerous CSRM studies and proven to simulate overall aerosol properties and their impacts on clouds and precipitation reasonably well (Morrison and Grabowski, 2011; Lebo and Morrison, 2014; Lee et al., 2016). This assumption means that a situation where fire continuously produces aerosols to maintain the initial background aerosol concentrations is adopted by this study.

7. Section 2: What data was used to initialize and nudge the simulations?

The following is added:

(LL197-205 on p7)

Balloon soundings of winds, temperature and dew-point temperature were obtained every 6 hours from Ft. Smith observation station, which is located near the forested site, as described in Kablick et al. (2018). The sounding data at 12:00 GMT on August 5th are used to prescribe the initial atmospheric condition. Using the sequential soundings, at each altitude, temperature and humidity tendencies are obtained. These tendencies represent the impacts of synoptic- or large-scale motion on temperature and humidity with the assumption that sounding data represent the synoptic conditions, following Grabowski et al. (1996), Krueger et al. (1999) and Lee et al. (2018). These tendencies are horizontally homogeneous and applied to the control run every time step by interpolation.

8. Line 235: Your assessment of "good agreement" between the model and satellite image is based on very little comparison. Can you provide more convincing evidence that these simulations are well representing this pyroCb event?

We made an additional comparison of cloud macro-physical properties, as represented by LWP, IWP, cloud-top and cloud-base heights, between the observation and the control run.

The following is added:

(LL284-297 on p10)

The averaged liquid-water path (LWP) over areas with non-zero LWP in the control run is 960 g m^{-2}, while the averaged ice-water path (IWP) over areas with non-zero IWP in the control run is 202 g m^{-2}. These simulated LWP and IWP are ~10 % different from the satellite-retrieved counterparts. In this study, for the calculation of LWP (IWP), we only considered droplets (ice crystals); drops with radii smaller (greater) than 20 µm are classified as droplets (raindrops). Stated differently, droplet mass but not rain mass is used to obtain liquid-water content (LWC) and LWP, and the mass of ice crystals but not the mass of snow aggregates, graupel and hail is used to obtain ice-water content (IWC) and IWP. The averaged cloud-top height and cloud-base height over the period between when the pyroCb forms and when the pyroCb disappears is 10.3 km and 3.6 km in the control run, respectively, and these simulated top and base heights are ~7% different from the satellite-retrieved counterparts. This indicates the overall cloud macro-physical structures, as represented by LWP, IWP, cloud-top and cloud-base heights, are simulated reasonably well as compared to the observation.

9. Lines 341-342: Experience has shown that what you choose as your lower threshold for averaging cloud water or LWP impacts the interpretation of the results. What do you consider the lower threshold given that models can provide very small numbers of LWP that are non-zero? Including tiny values of LWP in an average can impact trends in results.

In this study, all the LWP values which are not zero are considered for calculation related to LWP. Stated differently, as long as LWP is greater than 0.00 g m^{-2}, LWP is considered for the averaging process. For the calculation of LWP, we need to obtain LWC and it is true that results can be dependent on how to set up the threshold for LWC. Note that when LWC is greater than the threshold value, the LWC is used to calculate LWP and the averaging process. To respond to this comment, we vary the LWC threshold from 0.0 g m^{-3} to 0.01 g m^{-3} through 0.005 g m^{-3}; note that

The threshold value of 0.00 g m^{-3} is adopted by this study, and in addition to this threshold value of 0.00 g m^{-3}, the threshold values of 0.005 g m^{-3} and 0.01 g m^{-3} are also frequently adopted by previous studies for LWC and LWP calculations. We find that this variation of the LWC threshold does not change the qualitative nature of results in this study.

10. Lines 386-389: It seems to me that these two sentences about cloud-ice mass density are just stating that there is cloud-ice in the pyroCb and no cloud-ice outside of it. It seems unnecessary to state this since non-cloudy areas imply a lack of cloud/ice. Perhaps you can be clearer on what the intent is for these sentences.

The corresponding sentence is removed and related text is revised as follows:

(LL434-444 on p15)

The altitude of homogeneous freezing is at 9 km , so cirrus clouds which are composed of ice crystals (or cloud ice) only are between 9 km and 13 km. Between 9 km and 13 km, there are the presence of cloud ice and thus cirrus clouds in the control run, meaning that the pyroCb, which is simulated in the control run, produces cirrus clouds (Figure 6). The amount of cirrus clouds in the control run, as represented by the averaged cloud-ice mass density, ranges from 0.028 to 0.037 g m^{-3} between 9 km and 13 km (Figure 6). The averaged cloud-ice number concentration and cloud-ice size, as represented by its volume mean radius, between 9 km and 13 km ranges from 6 to 20 cm^{-3}, and from 10 to 20 micron, respectively. The altitudes between 9 km and 13 km correspond to a part of the UTLS below the troposphere. Henceforth, the UTLS cirrus clouds mean those clouds in a part of the UTLS below the tropopause.

11. Lines 390-391: What about considering homogeneous freezing? This process should be a major contributor to anvil ice mass and number concentration. I would expect ice numbers to be huge if a large portion of your aerosols are nucleated and transported aloft.

Ice numbers are shown as in our response to comment 12. To indicate the role of homogeneous freezing in ice number, the following is added:

(LL450-454 on p15)

However, mainly due to the larger aerosol concentrations, and associated greater homogeneous aerosol and droplet freezing, there is a large ~20-fold increase in cloud-ice number concentration and associated with this, there is a large ~2-fold decrease in cloud-ice size in the control run between 9 km and 13 km as compared to that in the low-aerosol run.

12. Section 4: This section should probably contain some discussion of changes to cirrus cloud ice crystal number concentration. There could be quite an enhancement in cirrus crystal sizes and number which can strongly impact cloud top albedo. Given your motivation factor regarding radiation and climate change, this would seem relevant.

The following is added:

(LL440-442 on p15)

The averaged cloud-ice number concentration and cloud-ice size, as represented by its volume mean radius, between 9 km and 13 km ranges from 6 to 20 cm^{-3}, and from 10 to 20 micron, respectively.

(LL450-454 on p15)

However, mainly due to the larger aerosol concentrations, and associated greater homogeneous aerosol and droplet freezing, there is a large ~20-fold increase in cloud-ice number concentration and associated with this, there is a large ~2-fold decrease in cloud-ice size in the control run between 9 km and 13 km as compared to that in the low-aerosol run.

Line 439: You state “percentage increase in updrafts”? Are you referring to the number of updrafts or the updraft speed?

Here, percentage increase in updrafts means percentage increase in updraft mass fluxes. Since the updrafts mass flux is “updraft speed” times “air density”, and air density at each altitude does vary negligibly among simulations, differences in updraft mass fluxes are mostly explained by those in updraft speed. Hence, percentage increase in updraft mass fluxes means percentage increase in updraft speed with good confidence.

The corresponding text is revised as follows based on the comment here and the other reviewer’s comment:

(LL495-498 on p17)

Of interest is that the greatest percentage increase in updraft mass flux is in the case of weak fire (weak-low to weak runs), smallest in the case of strong fire (low-aerosol to control runs), and intermediate in the case of medium fire (medium-low to medium runs) (Figure 4 and Table 2).

The following is added:

(LL498-503 on p17)

Since the updrafts mass flux is updraft speed that is multiplied by air density, and air density at each altitude does vary negligibly among simulations, differences in updraft mass fluxes are mostly explained by those in updraft speed. Hence, it can be said that percentage differences in updraft mass fluxes mean percentage differences in updraft speed with good confidence.

13. Lines 462-467: This couple of sentences is an example where the main point could be made more concise.

Text pointed out here is revised as follows:

(LL512-514 on p17)

Associated with the greater increases in updraft mass fluxes, the percentage increases in the UTLS water vapor and cloud-ice mass (Equation 1) are greater in the case of weaker fire (Figures 5 and 6 and Table 2).

14. Section 4.1.2: This entire section needs to be re-written or removed. The discussion of (LWC/CNDC) is overtly long and could be greatly condensed. To this same point, figures 13 and 14 are not necessary. The discussion here refers to basic algebra that goes into unnecessary description for the anticipated audience.

This section is simplified as seen in the revised manuscript. However, following a comment by the other reviewer, summary is added at the end of this section about LWC, CDNC and R_v.

Further, the section on equilibrium supersaturation is also unnecessary; a very short refresher regarding supersaturation could be useful, but most of the potential readers do not need a full review of this. Referring to Rogers and Yao (1991) is adequate.

Following the comment here, the corresponding section is shortened.

15. Lines 743-746: This is an example that shows up numerous times where the lengthy wording interferes with reading the paper in a concise manner. Here you state, “are higher in the low-aerosol run than in the control run for strong fire intensity, in the medium-low run than in the medium run for medium fire intensity, and in the weak-low run than in the weak run for weak fire intensity. . .” This is very cumbersome to read and needs to be written in a concise way. When you see this type of monotonic behavior, you can simply state. This type of writing shows up many times in the paper and this represents just one example. Please examine the full paper for areas that can be written more concisely.

Based on the comment here, we revised the manuscript in a concise way, particularly focusing on expressions similar to those pointed out here.

16. Lines 743-760: It seems here that you're stating that larger Rv = more autoconversion in lower aerosol runs, and then in the next sentence it seems to state reduced Rv = less autoconversion in higher aerosol runs. You don't need to state both of these. One of them implies that the other must be true.

We just want to clarify that in the next sentence which is pointed out by the reviewer here, we do not talk about "reduced Rv= less autoconversion in higher aerosol runs" as phrased by the reviewer here. In the sentence, we talk about the variation of Rv and autoconversion with varying fire intensity. Hence, the two sentences pointed out here deliver two different types of information.

17. Line 960: The use of "enhancing difference" seems awkward. Directly state what the difference is (increase? decrease?)

The enhancing is replaced with "increasing"