Authors' Response
Pages 1-3: Response to Anonymous Referee #1 (RC3)
Pages 4-9: Response to Anonymous Referee #2 (RC2)

Response to Anonymous Referee #1 (RC3)

Thank you for your additional comments to our previous response (in italic font). Our responses to your comments are below (in bold blue).

1. It would be helpful if the reviewer could provide more details on the data sources and their availability, so that we could further examine the quantitative differences between FINN v1.5 and GFED.

> My previous figure is based on the output of FIRECAM (https://globalfires.earthengine.app/view/firecam) except for FINN2.4. I also made similar figures by myself, and I can not find a problem in my procedure. Please check the attached file (info_for_figure.zip). I am afraid that the authors have forgotten to divide by 30 (days) for the figure of FINN1.5.

Response: Thank you for sharing the files. We have carefully looked into the codes and realized that our previous figure for FINN was showing the mean PM2.5 emission rate averaged among fire cells (1 km^2 per cell) within each 0.1° × 0.1° grid. However, there are a lot of cells that do not have any fires (i.e., simply no FINN data as there is no fire), which should also be included when calculating the 0.1° × 0.1° grid average. GFED provides this grid-mean data, so in order to have a fair comparison, we have re-calculated the PM2.5 emission rates in [ton/month/each 0.25° × 0.25° grid] as shown below; both of the plots match what was provided by the reviewer. Although this figure is not included in the paper, we appreciate the reviewer's comment, which has led to the clarification of the quantitative difference between FINN and GFED for the month of September in 2015.

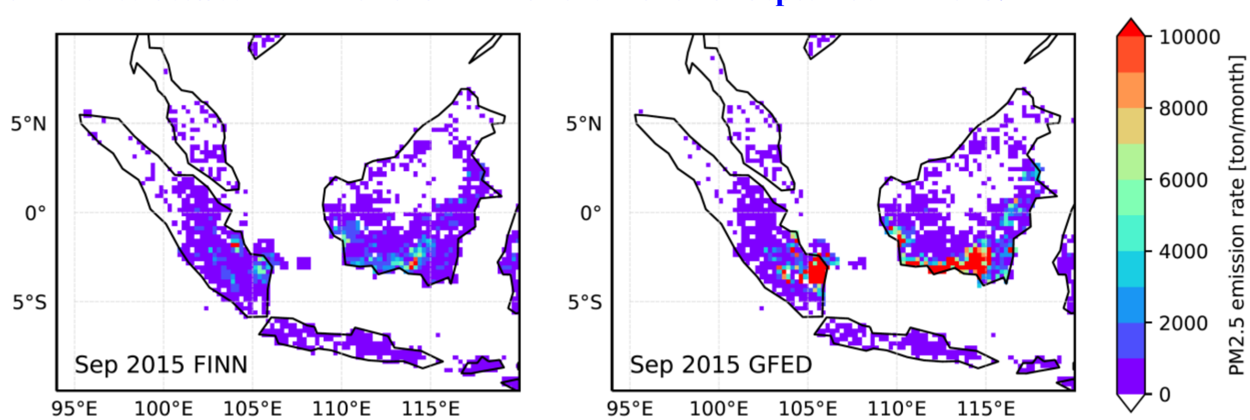


Figure: Estimated PM2.5 emission rate [ton/month] in each 0.25° × 0.25° grid for the month of September in 2015, according to (left) FINN v1.5 and (right) GFED.

2. We deem that the ratio of 5:1 is not an uncommon parent-to-nested ratio and has been used in similar studies. It is not entirely clear to us what/which incorrect feedback (e.g., relevant to aerosol-cloud interaction?) was being referred to, and indeed, we would greatly appreciate it if the reviewer could provide some relevant publication on this matter. Even if the indexing was slightly off, we do not believe that it has an impact significant enough to invalidate our scientific findings from our simulations.

> I am afraid that the authors have not checked the release note of WRF version 4.2 which I have shown, but detailed information related to this problem can be found at: https://github.com/wrf-model/WRF/pull/1100 I recommend the authors to conduct a sensitivity test by yourself whether this problem might affect your result or not, by using corrected source code (share/interp_fcn.F).

Response: We have run a NOFIRE simulation with changes in the file (share/interp_fcn.F) incorporated for the first 3 days of September 2015. Differences (NOFIRE - NOFIRE$_{new}$) in temperature at the lowest model level is shown below; as is clear, the difference is very small, which indicates the negligible impact of the code change to our simulations results and therefore our findings.

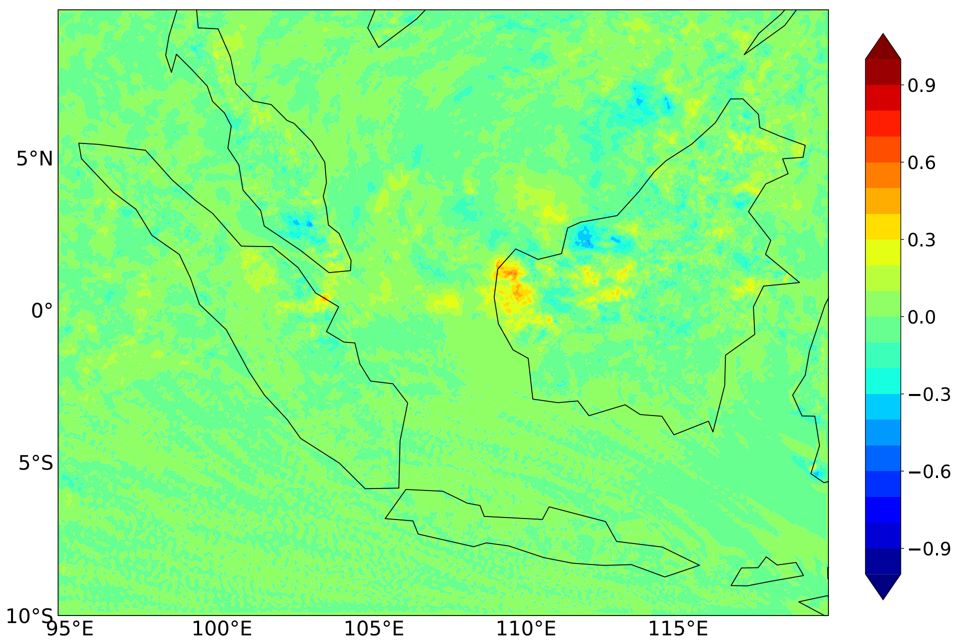
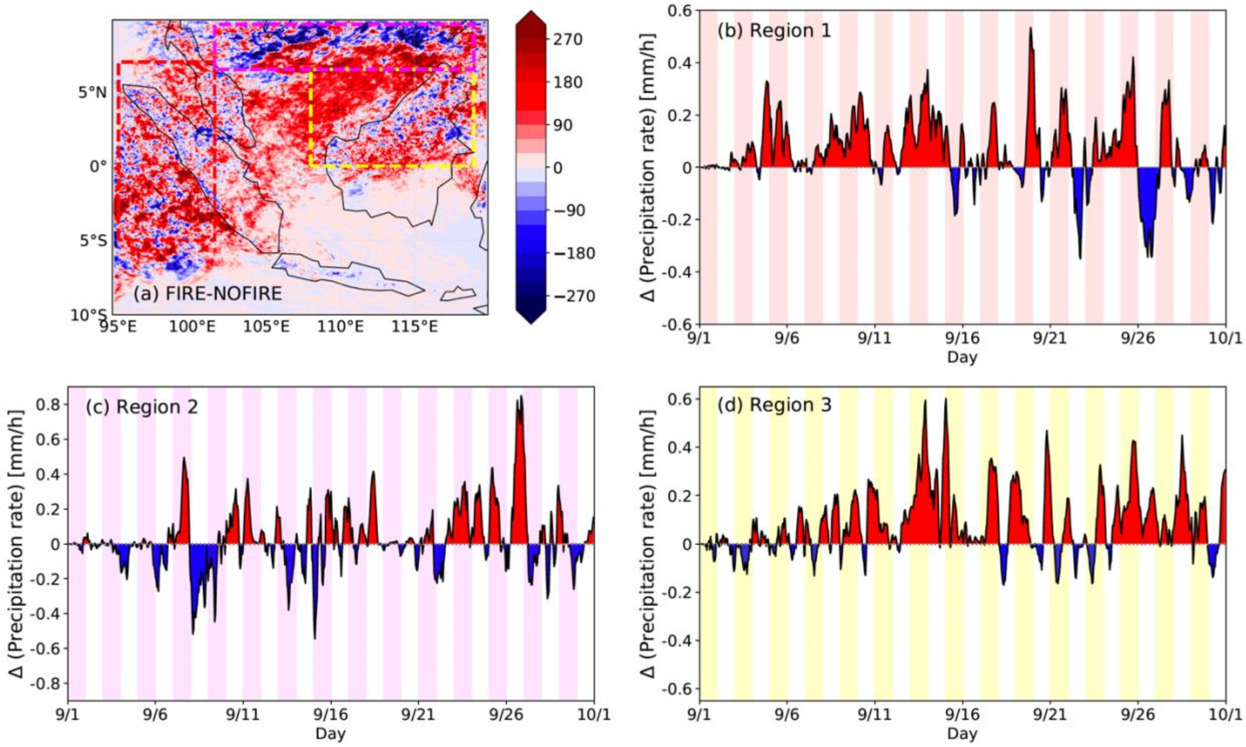


Figure: Differences (NOFIRE - NOFIRE$_{new}$) in temperature at the lowest model level, averaged over the first 4.5 days in September 2015.

3. I am also wondering why the variation is centered at 0.2mm in Figures 8c and 8d. Did you edit the value by adding some offset without explanation? If so, it degrades the reliability of the results in this paper.

Response: No, it was simply a y-axis label error. Thank you for pointing that out, and the corrected figure below will be used in the revised manuscript.



<u>Figure 9 (formerly Figure 8)</u>: (a) Difference (FIRE-NOFIRE) in accumulated precipitation [mm] over the month of September. (b-d) Time series of regional mean precipitation rate differences (FIRE-NOFIRE) in (b) Region 1, (c) Region 2, and (d) Region 3.

Response to Anonymous Referee #2 (RC2)

Thank you for your comments and suggestions. Please see our responses to your comments and questions in bold blue below.

This study uses the Weather Research and Forecasting model coupled with Chemistry (WRF-Chem) to understand the impacts of these fire particles on cloud microphysics and radiation during the peak biomass burning season in September over the Maritime Continent region with 4-km grid spacing. The authors show a clear sign of precipitation and cloud top height enhancement by fire particles. Such a study is certainly of great interest to the community and fits well to the ACP scope. Long-time cloud resolving model simulations covering a large region for aerosol-cloud interaction study is precious. The analysis in the current paper is not that comprehensive and strict yet. I think major changes are needed in terms of determining what mechanisms are responsible for the convective invigoration before the study can be accepted as a publication in ACP. Hope my specific comments below help the authors improve the paper.

Specific comments:

- Method section, the Quick Fire Emissions Dataset (QFED) is a relatively new fire inventory that usually gives much high emission of fore aerosols than FINN. From Figure 6, the model underestimates AOD a lot. I think QFED would help. I understand it is challenging to rerun the model simulations with the new emission. Adding some discussion about this would be fine.

  Response: Thank you for your suggestion. We have added a discussion on this topic at the end of Section 2.

  Page 6 lines 113-119:

  "It is worth noting here that this choice of fire inventory may have a significant impact on the simulated results; for instance, the Quick Fire Emissions Dataset (QFED; Darmenov and da Silva, 2015) provides a relatively large amount of particle emissions from fires compared to FINN (e.g., Liu et al., 2020; Pan et al., 2020). On the other hand, the recently published version of FINN (version 2.4) may include an improvement to the FINN dataset that leads to its large difference from version 1.5 that this study utilized. While the improvements of fire inventories are still ongoing and their comparisons are beyond the scope of this study, the potential impact of using different inventories needs to be kept in mind."

  Page 16 lines 288-289:

  "Furthermore, do our simulation results depend on the fire inventory used for the simulations?"

- Section 3.1 and Figure 5, the simulation without fire already substantially overestimated precipitation in Region 2 and 3. Considering fire aerosols makes the simulation further deviate from the observations. The reason for the poor performance of the WRF model simulations might be discussed. Did you evaluate water vapor, T, and SST with observations? For maritime conditions, ECMWF reanalysis data is generally better for

initial and boundary conditions than NCEP FNL since it assimilated water vapor data from satellite over the ocean. Also, literature studies with WRF documented various reasons for overestimating precipitation such as overestimated surface latent heat fluxes, and the problems with the physics scheme used such as MYNN PBL and Morrison 2-moment microphysics in your case. The relevant literature studies should be discussed to provide potential reasons for such a large discrepancy between model and observed precipitation.
<u>Response</u>: We think that the largest contributor to the discrepancies is SST (i.e., its temporal spatial evolution in the model), as it has a strong influence on convective activities over the tropics. We have added the discussion on this at the end of the first paragraph in Section 3.1.

Page 7 lines 132-142:
"The discrepancies in the absolute values shown in Figure 5a-c may be largely due to the lack of ocean dynamics that can lead to more realistic sea surface temperature (SST) distributions. As can be inferred from the effects of ENSO on the amounts of precipitation over MC, SST has significant impacts on the convective activities in the tropics (e.g., Graham and Barnett, 1987; Woolnough et al., 2000; Tompkins, 2001). Indeed, MC lies over the tropical warm pool where the Earth's highest SST is observed. Sabin et al. (2013), for example, analyzed the observed data of SST and convective activities around the warm pool and found a tight connection between the two, especially between 26°C and 29°C. Estimating SST over MC also faces an additional challenge: the complicatedly distributed while nearly equalized coverages of land and ocean in the area. It hence requires a high-resolution ocean model to accomplish (e.g., Wei et al., 2014). Therefore, the lack of realistic temporal and spatial variations in SST may be one of the reasons why the simulated amounts of rainfall are off, while the spatial distributions and the overall temporal evolution are reasonably well reproduced. Current WRF/WRF-CHEM does not have the capability of coupling with an ocean model, which would remain as one of their desirable future developments."

- L135-140, the underestimation of AOD can be also because of the largely overestimated precipitation which usually scavenges aerosols efficiently, besides the underestimated fire emissions from FINN. Vice versa, the underestimation of AOD can also be one of the reasons for the largely overestimated precipitation because large AOD can suppress convection and precipitation through aerosol radiative effect as shown in many literature studies. It is a little surprising that you did not find fire aerosol impact through aerosol radiative effect. Did you look at the clear-sky temperature changes? The robust way is to do a sensitivity test by turning off the aerosol radiative effect in the radiation scheme.
  <u>Response</u>: The point is well received. Following your suggestion, we have looked at the clear-sky radiation differences. This additional analysis revealed a few key features, including the reduction of ground-level temperature in FIRE due to the aerosol radiative effect. Although this effect does not seem to have contributed to the invigoration of convection (i.e., it rather stabilizes the atmosphere) and therefore our overall conclusion remains unchanged, we have updated Figure 8 with additional plots and added its discussions in the main text.

  Page 11: New Figure 8

Page 7 line 161 - Page 9 line 176:
"Firstly, their impacts on radiation are discussed. Figure 8a-d shows the mean changes in incoming (ground-level) and outgoing (top-of-the-atmosphere) shortwave radiation under clear- and all-sky conditions, respectively. It is clear from this figure that the inclusion of fire particles reduced the solar radiation reaching the ground by scattering and/or absorbing. Such a radiative difference indeed led to lower temperature near the ground in FIRE, by a degree or so, as shown in Figure 8e. The location of this strongest cooling effect coincides with that of the largest reduction in incoming insolation on the ground, implying their connection. At the top of the atmosphere, outgoing shortwave radiation increases in FIRE particularly over the seas where the surface is dark, due to scattering by fire particles. As a result, albedo increases in the FIRE run (Figure 8f), although this increase is partly due to the increased cloud optical depth (Figure 8g); the aerosol direct and indirect effects both worked to increase the overall reflectivity, even though their timing or causal relationship remains uncertain. The reduction in outgoing longwave radiation (OLR) in FIRE (Figure 8h) indicates that cloud top heights increased on monthly average. This reduction in OLR suggests that convection was stronger and clouds developed taller in the FIRE run. Although this is contrary to what can be expected from the aerosol radiative effect that reduced the surface temperature and worked to stabilize the atmosphere, we show next that convection became stronger in the FIRE run and increased the amount of rainfall."

- L157-158, incorrect statement. The increase in rainfall and cloud top height are only the indication of invigoration, but this could be achieved through mechanisms other than what was proposed in Rosenfeld et al. 2018 which is through enhanced ice processes. For example, the enhanced latent heat from condensation as suggested in Sheffield et al. 2015 (JGR) and Fan et al. 2018 (Science). Also, the microphysical effect of aerosols can be an important factor contributing to the increased cloud top height as shown in Fan et al. 2013 (PNAS).
<u>Response</u>: We agree, and indeed, this sentence has been removed from the main text as it was no longer necessary in the revised manuscript. However, we have elaborated our explanations on how convection gets invigorated, as explained in our responses to your comments below.

- L172-174, The mass for each hydrometeor is increased. Also, precipitation is increased. This means that the conversion of water vapor to condensed phase is enhanced a lot. Both condensation and deposition play a role in this increase. Those two processes generally dominate the latent heat release and need to be explained. The increase of condensation heating can play a much larger role in invigorating convection than the same amount of latent heating from ice-related processes as shown in Fan et al. (2018) and Lebo et al. (2018, JAS). Often the increase in condensation heating is larger than the other processes in magnitude. Therefore, I think more analysis is needed to figure out whether warm-phase invigoration through condensation also contributes to the invigoration or not, besides the cold-phase invigoration as described in Rosenfeld et al. 2018. If you do not have outputs of condensation and deposition rates, you may do restart runs for a selected short time period to output them to look at. Another option is to look at the vertical profile of supersaturation change from the nofire to fire cases for the convective updraft cores only (such as use W > 5 m/s) to see where the maximum reduction in supersaturation occurs.

Response: Thank you for your insightful comment. With additional outputs of (i) activated droplet number [# kg^{-1}], (ii) freezing rate [# $kg^{-1}s^{-1}$], (iii) cloud droplet effective radius r_{c_eff}, and (iv) ice crystal effective radius r_{i_eff}, we have estimated the latent heat release associated with droplet activation and freezing (figure below); in the calculation, we have assumed all the new droplets/ice crystals to have the same radius r_{c_eff}/r_{i_eff}, due to which these estimated amounts are the maximum values (i.e., newly formed droplets/ice crystals must be smaller than r_{c_eff}/r_{i_eff}). According to these estimates, the increase in latent heat release from droplet activation is much larger than that from freezing. We have added this figure and discussion in the revised manuscript.

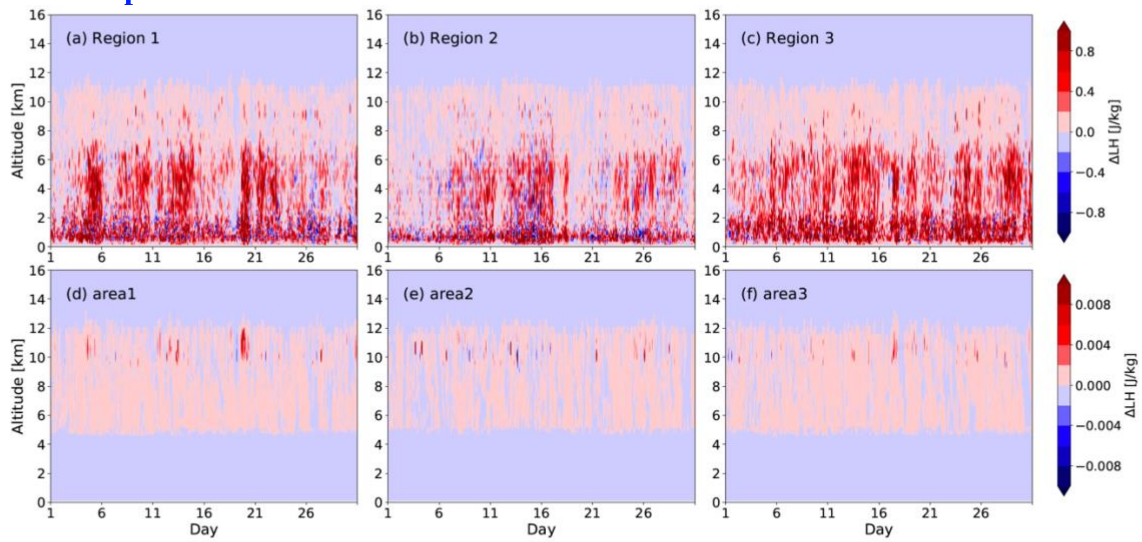


Figure A3: Differences (FIRE-NOFIRE) in the maximum amount of latent heat [Jkg^{-1}] released upon (top) droplet activation and (bottom) droplet freezing in (a,d) Region 1, (b,e) Region 2, and (c,f) Region 3. These were estimated from newly activated droplet number concentration [#kg^{-1}], time step [s], droplet freezing rate [#$kg^{-1}s^{-1}$], and cloud droplet and ice effective radii rc and ri. Since newly formed droplets and ice crystals are typically smaller than rc and ri, respectively, these are the maximum estimates. Note the difference in the scale of the color bars.

As for supersaturation, it was indeed reduced in the FIRE run, as shown in the figure below. However, the differences in latent heat release above show the comparison of the two processes (i.e., activation vs. freezing) more clearly, and therefore we did not include the following figure in the main text.

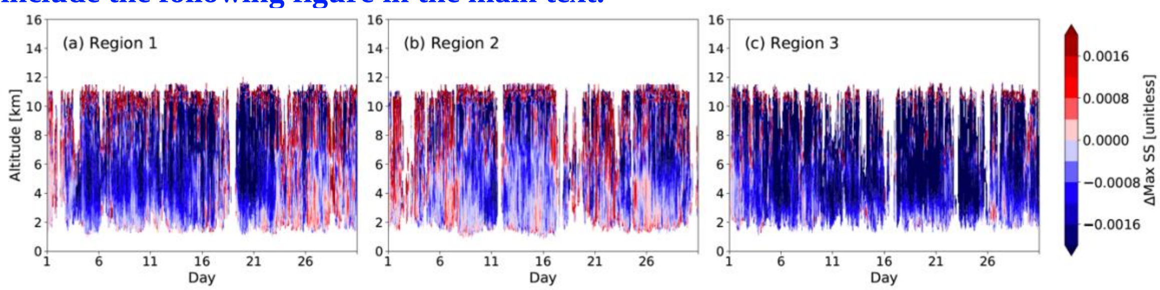


Figure: Time evolution of differences (FIRE-NOFIRE) in maximum supersaturation S_{max} averaged within each region, only sampled where updraft ≥ 5 ms^{-1} and $S_{max} > 0$.

Page 18: Figure A3

Page 12 line 209 - Page 13 line 214:

"As for the invigoration of convection signified by the increased rainfall and cloud top height, our analysis has revealed that the increased latent heat release is predominantly through increased condensation rather than increased freezing; Figure A3 shows the estimated amounts of maximum latent heat released upon droplet activation and freezing. According to these estimates, convection was likely invigorated more by increased condensation and less so by increased freezing. This result agrees with what was shown by Fan et al. (2018) and Lebo (2018)."

- Also, there is no support to say "surface rainfall seems to largely stem from melted snow and graupel". For the tropic convection, warm rain should have a significant contribution, particularly in the nofire case where background aerosols are low and the formation of warm rain should be quick. You may output warm rain and melted rain separately to verify this by restarting both simulations at a time of interest and running for a short time like 6 hours only (this way you can also address b better). With the addition of a large number of fire aerosols, warm rain may be severely suppressed which can lead to the dominance of melted rain, but this needs to be shown. To clearly show this, either through the comparison of warm rain and melted rain or autoconversion rate.

Response: The reasoning behind the statement has been clarified in the revised manuscript; Figures 11 and A1 qualitatively show a very strong correlation between total rain mass and snow/graupel mass. That is, their longitudinal/latitudinal patterns correspond very well with each other. Also, their absolute values are on a similar scale (i.e., ~0.01 gm^{-3}), as compared to those for cloud droplets (i.e., ~0.001 gm^{-3}). These are the reasoning that have been added to the main text.

We have additionally looked at the difference in autoconversion rate within updrafts ($\geq 5\ ms^{-1}$), for each of the three regions, as shown below. However, these figures do not show longitudinal/latitudinal distributions that are similar to those for rain mass or other hydrometeors (e.g., Figures 11 and A1) that were impacted by fire particles. Thus, we still think that the melted snow and graupel contribute significantly to rainfall, rather than the dominance of warm rain processes.

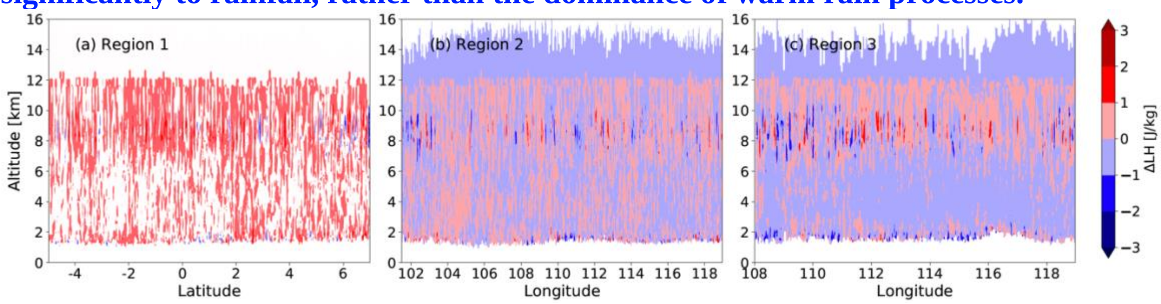


Figure: Changes (FIRE-NOFIRE) in the maximum amount of latent heat released upon autoconversion [Jkg^{-1}]. These were estimated from the autoconversion rate [$\#kg^{-1}s^{-1}$] and cloud droplet effective radius r_{c_eff}. Since newly formed droplets are typically smaller than r_{c_eff}, these are the maximum estimates.

Page 12 line 198-201:

"It is also clear that the longitudinal/latitudinal patterns of the rain and snow/graupel masses correspond very well with each other, implying the significant contribution of melted snow/graupel to rain mass. Furthermore, the absolute difference values shown in Figures 11 and A1 are on the order of 0.01 [gm^{-3}] for both rain and snow/graupel in all three regions, whereas those for cloud droplets are merely 0.001 [gm^{-3}]. Thus, surface rainfall seems to largely stem from melted snow and graupel."

- L178-182, I think the most important is the number of supercooled droplet is increased a lot, which allows more tomes of snow accretion and riming growth for both snow and graupel.
  <u>Response</u>: We agree, and the original wording was not clearly stating this. Therefore, we have changed the sentence so that the important contribution of increased droplets to enhanced snow & graupel formation is mentioned;

  Page 12 lines 206-208 (underlines what was added):
  "… the increased mass <u>and number</u> of cloud droplets in the FIRE simulation set a more favorable condition for efficient snow <u>and graupel</u> production in clouds, such as <u>through</u> droplet accretion by snow (Figure A2g-i)."

- L189, as mentioned earlier, this has been documented well in literature studies such as Fan et al. 2013.
  <u>Response</u>: We have added the following sentence to the main text;

  Page 13 line 222-224:
  "*Such aerosol-induced changes in stratiform anvil clouds, namely their extended lifetime and heightened cloud top, have been reported in previous studies (e.g., Fan et al., 2018), even though these changes can be independent of the invigoration of convection."*

- Conclusion, I suggest adding discussion about wildfire heat impacts, which is excluded in this study but can play an important role in changing low-level temperature and impacting convection. Zhang et al. (2019, GRL) presented a revised WRF-Chem with wildfire heat impacts considered.
  <u>Response</u>: Thank you for your suggestion. We have now briefly mentioned the potential importance of the heat effects, along with the reference to the paper, in the last paragraph of Conclusions in the revised manuscript.

  Page 16 lines 285-288:
  "Although it was out of the scope of this paper, recent studies such as Zhang et al. (2019) have shown a potential importance of heat effects of fires on convective clouds; they found strengthening of convection by the heat effects and therefore significant changes in subsequent cloud properties in their simulations. In the region of our interest, how much the heat effects of fires exert the invigoration of convection could definitely be examined in the future."