Dear Editor,

We would like to thank for reviewers' valuable comments and suggestions. Their comments are addressed as shown below. Hope they find our revisions useful. Thank you very much.

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Regards,
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Reviewer 1:

We thank the reviewer for very helpful comments. Your comments and suggestions are addressed accordingly. Thank you very much for your effort.

In this study, the authors employed WRF-Chem to study the influence of anthropogenic aerosols on a relatively-heavy rainfall event. They showed that aerosol enhanced precipitation in southern part of the domain and aerosol– cloud interactions (ACI) is the main reason for the response. They further did sensitivity studies and found that re- mote aerosols contributed more than twice the precipitation increase compared with local aerosols. By further increasing emission by 10 times, their figures showed that more significant decrease and increase of precipitation in the respective cloud regimes (I did not use the wording from the authors because I do not agree with it).

Major comments:

1. The authors missed some key points when interpreting their results. The simulated cloud system seems like a cold front system meeting with warm and moist air. The cloud regimes should be very different over the code side of the frontal system compared with clouds at the convergence zone and warm side of the system. This key message should be considered when analyzing aerosol-cloud interactions since ACI strong depends on different cloud regimes. Decrease and increase in precipitation are seen over the different parts of the domain (Figure 3d) but the authors ignored the decrease part which is at the cold side of the system but focused on the increased part. When further increasing emission by 10 times, there is enhanced decrease (Figure 12b) but the authors still ignored it. Another misleading analysis is that the authors used the domain averaged vertical cross section plots and viewed then as single deep convective cell to discuss the ACI effect. The low-level clouds shown in such plots might not be vertically connected with the

higher-level clouds. For example, shallow clouds could mainly occur in the northern part of the red box area used for the analysis and deep convective clouds could mainly occur in the southern part. Increasing aerosols suppresses shallow convection, which would be different from the story that the authors described in the paper.

Response: We thank the reviewer for very helpful comments. The background circulation pattern at 500 hPa is characterized by ridge in north and trough in south over Asia (Figure R1). This pattern is favorable for persistent meeting between cold air from the north and warm moist air from Bay of Bengal and South China Sea, resulting in intensive convergence near the surface (Figure 1b) and torrential rainfall over Guangdong Province. The cloud top temperature average over the land in domain 2 is lower than –15 °C almost everywhere with minimum reaching about –35 °C (Figure S1b), indicating strong convection. Moreover, cloud ice, over the region with both decreased and increased parts, extends up to 16 km shown in Figure S11 and Figure S8, respectively. Further inspection of cloud evolution within the red box shows that the cloud regimes are consistent within the increased area used. We divided the red box area in 22°–24°, 112°–115° into a north box in 23°–24°, 112°–115° and a south box in 22°–23°, 112°–115°. Shallow clouds occur in both the northern and southern parts of the red box area (Figure R2). Figure R3–R6 show the differences in microphysical and dynamic variables due to aerosols. Their similar patterns in Box_N and Box_S suggest that the processes and related physical mechanism within the red box are consistent with each other.

Thanks for your comments regarding the mechanism of decreases in precipitation. We choose another region in 24°–25°N, 110.5°–112.5°E over the northwest corner of domain 2. The analysis is added in the paragraph six in the discussion section. Thank you again.

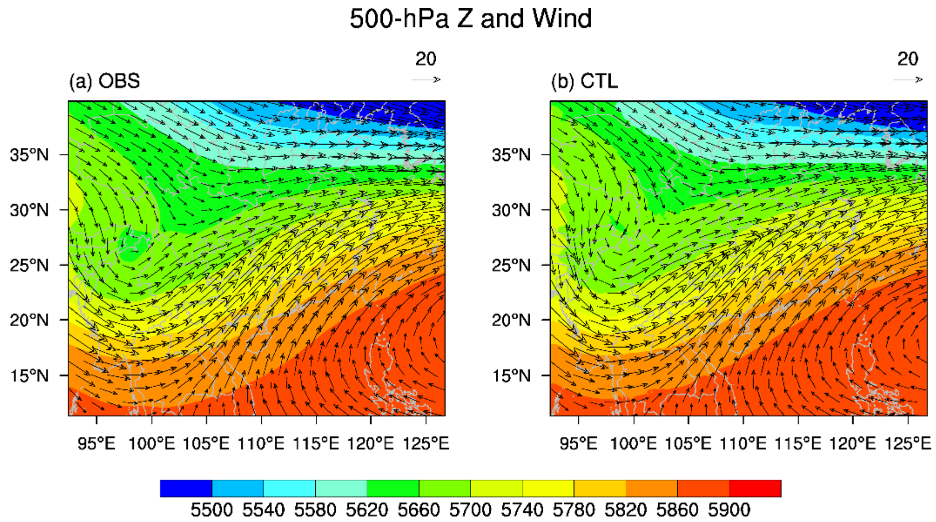

Figure R1. Spatial distribution of 3-day averaged 500-hPa wind (vector; unit: m s^{-1}) and height (shading; unit: m) during December 14–16, 2013 for (a) OBS from ERA-interim and (b) CTL from control simulation.

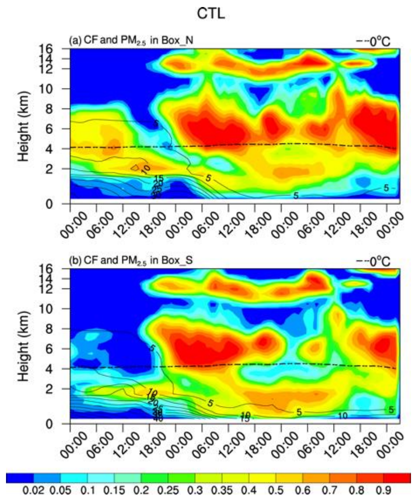

Figure R2. Time-height cross section of cloud fraction (CF; shading; unit: unitless) and $PM_{2.5}$ concentrations (contour; unit: μg m^{-3}) in (a) 23°–24°, 112°–115° (Box_N) and (b) 22°–23°, 112°–115° (Box_S) from control simulation. Dashed lines denote 0°C isotherm calculated as the averaged zero-layer height over the red box in Figure 3.

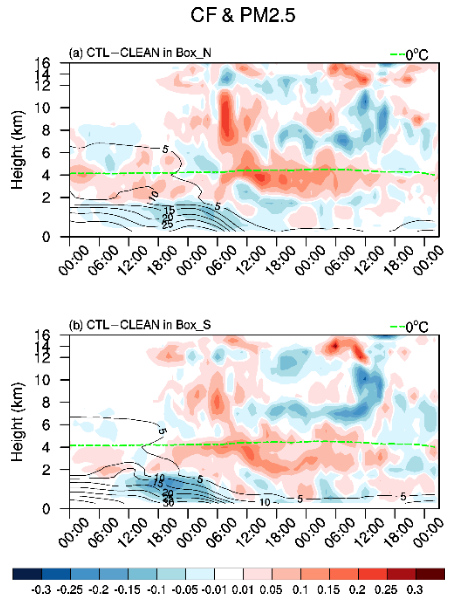

Figure R3. Differences with time (abscissa) and height (ordinate) in CF (shading; unit: unitless) and $PM_{2.5}$ concentrations (contour; unit: μg m^{-3}) between CTL and CLEAN (i.e. CTL minus CLEAN) for (a) Box_N and (b) Box_S. Only CF and $PM_{2.5}$ concentration anomalies that exceed 90% significance level are depicted with shading and contour. Green dashed lines denote 0°C isotherm calculated as the averaged zero-layer height over the red box in Figure 3.

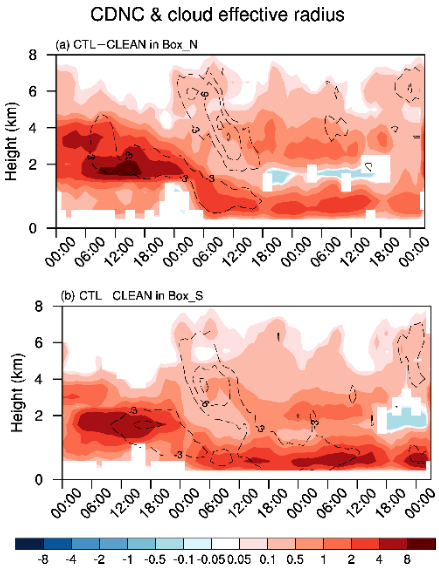

Figure R4. Differences with time (abscissa) and height (ordinate) in cloud droplet number concentrations (CDNC; shading, unit: 10^7 kg^{-1}) and cloud effective radius (contour; unit: μm) between CTL and CLEAN (i.e. CTL minus CLEAN) for (a) Box_N and (b) Box_S. Only anomalies that exceed 90% significance level are depicted with shading and contour.

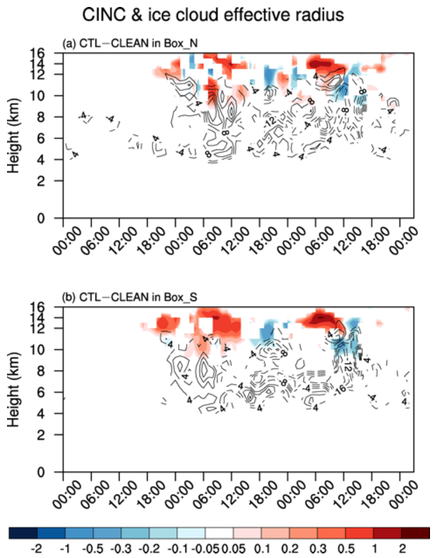

Figure R5. Differences with time (abscissa) and height (ordinate) in cloud droplet number concentrations (CINC; shading, unit: 10^5 kg^{-1}) and ice cloud effective radius (contour; unit: μm) between CTL and CLEAN (i.e. CTL minus CLEAN) for (a) Box_N and (b) Box_S. Only anomalies that exceed 90% significance level are depicted with shading and contour.

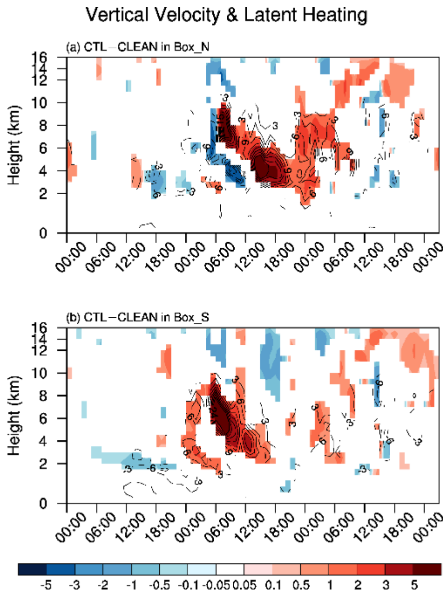

Figure R6. Differences with time (abscissa) and height (ordinate) in vertical velocity (shading, unit: cm s^{-1}) and latent heating (contour; unit: 3 K d^{-1}) between CTL and CLEAN (i.e. CTL minus CLEAN) for (a) Box_N and (b) Box_S. Only anomalies that exceed 90% significance level are depicted with shading and contour.

2. The authors did not present enough data to examine the things they claimed for. In a few places as detailed in the specific comments, the authors assumed the literature work applies well to this study without presenting the key results to prove the point. See specific comments #14, 18, 20, 21, and 22. There are many inaccurate or misleading statements. I noticed they are mainly related to the lack of expertise in cloud physics and weather area, such as #7, 10, 13, 18, 20, 21, and 22.

Response: We thank the reviewer for the thoughtful and thorough comments and suggestions. The comments are addressed accordingly as follows.

3. There are many inaccurate or misleading statements. I noticed they are mainly related to the lack of expertise in cloud physics and weather area, such as #7, 10, 13, 18, 20, 21, and 22.

Response: Thanks for pointing this out. The descriptions are corrected based on your comments. Please see the corresponding responses below.

Specific comments:

1. P2, the later part of the last paragraph discusses literature study about ACI, which does not include the most recent work on this topic from a Science article (Fan et al., 2018).

Response: Thanks for this suggestion. The paper is cited, and corresponding descriptions are added (P3 L3–5).

2. P2, L19: "the slowing autoconversion rate induced by aerosols forms airborne cloud droplets in clouds" is confusing. First, what is "airborne cloud droplets in clouds"? since it is in clouds, why call it "airborne"? second, how does autoconversion form cloud droplets?

Response: Thanks for pointing this out. The descriptions are corrected in our revised manuscript (P2 L31–32).

3. P5, L6, based on Fan et al. 2015, the factor used in the study is 0.3 (not 0.1).

Response: We adjusted the factor to 0.1 from 0.3 in Fan et al. (2015) to represent the background situation as the emissions in 2010 is much higher than that in 2006, which is revised in the manuscript (P5 L9–11).

4. P5, L12-14, not sure how IC, BC, and emissions were treated in both domains in both D1 and D2. If Dom1 and Dom2 are run at the same time, which means dom 1 provides IC and BC for domain 2, then how to change IC and BC in Dom 2 for D2? In addition, if emission does not change in Dom2, wouldn't the local effect be underestimated?

Response: Yes, domain 1 and domain 2 were run at the same time. The IC for domain 1 and domain 2 were provided from MOZART data. In D1 experiment, the IC, BC, and emissions were kept as same with the control run simulation for domain 1. Meanwhile, the IC and emissions were scaled by a factor of 0.1 for domain 2. In D2 experiment, the IC, BC, and emissions were scaled by 0.1 for domain 1. The IC and emissions were kept as same with the control run at the same time. The impact of boundary conditions provided to domain 2 was treated as effect of aerosols from outside of domain 2.

5. P5, L18-19: Which simulation is 10X based on?

Response: This simulation was based on the control run, which is revised in the main text (P5 L21–22).

6. Section 3.1: since there are 58 stations for PM2.5 measurements in Domain 2, why not use them to evaluate the control simulations since the aerosol property is important to aerosol impacts?

Response: Thanks for your question. We agreed this suggestion. Figure R7–R8 show the spatial distribution and time series of $PM_{2.5}$ concentrations during December 14–16, 2013, respectively, based on observation and control simulation. Over the delta region, higher aerosol concentrations occur in mega cities, while lower concentrations appear over their surrounding areas. The model underestimates $PM_{2.5}$ concentrations in the first two days with a more homogeneous pattern. This could be induced by either the relative coarse resolution of model or the pseudo surface (actually above the ground) due to model vertical layers design. The failure to get some hot spots near the estuary may be attributed to the uncertainty of emissions. In the time series, both the simulation and observation show a dramatically decreasing trend of $PM_{2.5}$ concentrations once the rainfall initiated. The model could generally replicate the spatial distribution and time evolution of $PM_{2.5}$ concentrations with some underestimation during the first two days. This bias may lead to an underestimation of the aerosol impact on rainfall.

The descriptions associated with the figure are added into the manuscript.

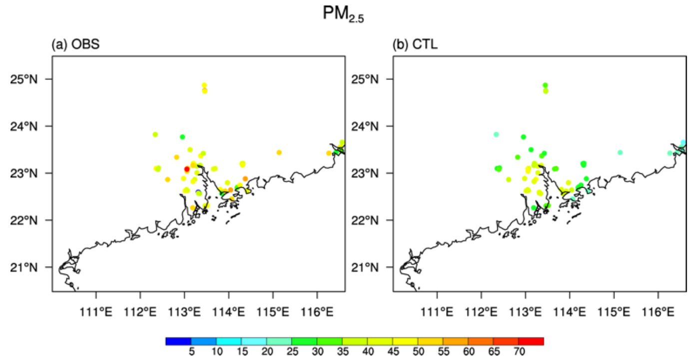

Figure R7. $PM_{2.5}$ concentration (unit: μg m^{-3}) average during December 14–16, 2013 for (a) observation and (b) control simulation. Colored circles denote in situ station locations.

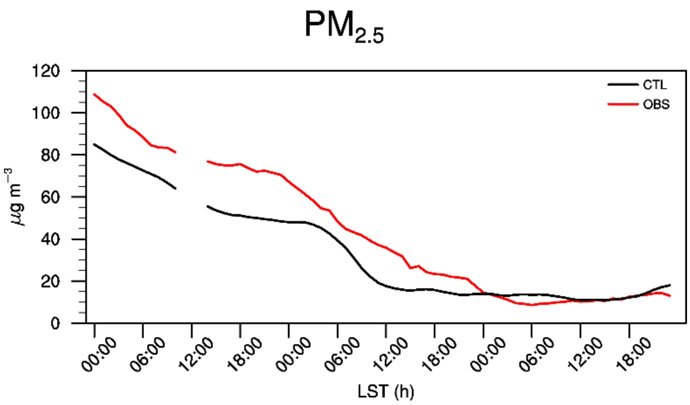

Figure R8. Time series of $PM_{2.5}$ averaged over all the stations during December 14–16, 2013 for CTL (black) and OBS (red).

7. P7, L20-22, this sentence is not justified. It could only because in the second day there were much larger in-cloud and below-cloud scavenging of aerosols due to much cloud and rain. The smaller aerosol effect in the first day can be a result of many factors particularly meteorological conditions, and the larger effect you see in the next day might not so related to the aerosols in the first day.

Response: Thanks for this comment. The aerosols influence the cloud droplet number concentration and cloud effective radius during all the three days. However, the rainfall changes induced by aerosols start from the second day when rainfall peak happens. This suggests that the aerosol impact on rainfall is modulated by the meteorological conditions. Related description is revised in the revised manuscript (P8 L4–6).

8. Figure 4, how is the cloud fraction calculated? Is the difference in percentage or absolute difference?

Response: The cloud fraction parameterization in the model follows Randall (Hong et al., 1998). The cloud fraction was calculated as the sum of cloud water, cloud ice and snow. The differences in Figure 4 are the absolute differences.

9. P7, L25-28, Better to use percentage differences or both in terms of quantifying the accumulated rain.

Response: Agreed. The rainfall differences in percentage are added in the revised manuscript (P8 L10–12).

10. P8, L6-11, the whole description here has a problem. The way it describes currently basically says that aerosols are the reasons responsible for the more and deeper clouds at later time and less and shallower clouds at the earlier time, which should not be true. The first order is the meteorological conditions that are responsible for the cloud amount and vertical distribution. On the top of it, aerosol may influence it, and then you can describe the influence in more quantitative way.

Response: Thanks for pointing this out. The description is revised in the main text (P8 L23–27).

11. Figure 5, the figure caption needs to be consistent with the figure label. If you label your panels in a, b, c,.., you need describe your figures in the same way so that readers can follow. This comment apply to many other figures. Also, the caption already has too many acronyms while another acronym CI for contour interval is used here, which only causes poor readability and confusion here. About the differences in CINC and ice effective radius, did you only consider cloud ice crystals, or all of the ice-phase particles were considered?

Response: Sorry for the inconsistency. The captions are revised correspondingly. Only cloud ice is considered here.

12. P 8, L15, when you say "dramatically", you need to give a quantitative value. Figure 5 only shows the absolute differences, which has the maximal value at the magnitude 80 cm-3 based on the legend. This change is not large unless you did a domain average and there are many cloud-free points in the analysis domain.

Response: We carefully checked the calculation and updated the results accordingly. As shown in the legend, the maximal value is 8×10^7 kg^{-1} (8×10^7 $[10^3\ g]^{-1}$=8×10^7 $[10^3\ cm^3]^{-1}$), which is equal to 8×10^4 cm^{-3}. The magnitude of this value is comparable to that in Zhong et al. (2015). In percentage, the cloud droplet number concentration has increased by 5.5 times. Descriptions are revised in the main text.

13. P 8, L20, what do you mean about "the interim processes"?

Response: The interim processes refer to that more cloud droplets are lifted to freeze into ice clouds. Our further analysis on source of latent heat is not attributed to freezing. The corresponding descriptions are deleted in the main text.

14. P8, L17-27, the entire description here about the ACI effect is not about the results from their study. The authors just followed what the literature describes. First, the description and the corresponding references do not reflect the symbolic literature studies on ACI on deep convective clouds. First, the idea of convective invigoration by enhanced latent heat from cold-phase processes (due to suppression of warm rain) starts from Andreas et al. Science, 2004 (obs), then Khain et al., QJR, 2005 (model), Rosenfeld et al. Science, 2008 (theoretical), Fan et al., JGR, 2009, etc, did detailed studies about it. The authors did not mention these studies at all (Rosenfeld et al., 2008 is not discussed in an appropriate way since it is the theoretical study for this theory). Second, the most recent development of ACI is the "warm-phase invigoration" in Fan et al. Science, (2018) where latent heat release from enhanced condensation is emphasized as a reason for the enhanced updraft speed. From Figure 5, the latent heat enhancement peaks below 8 km altitudes and there is a peak at 3-5 km, suggesting condensational heating might play a significant role here as well. The latent heat enhancement at low part of clouds from condensation plays a much more significant role than the correspondent at high levels as shown in Fan et al. 2018. The authors need to examine this in detail to understand what's the real reason behind it instead of just citing some literature studies since ACI is a key point of the study.

Response: Thanks for your constructive and insight comments. Following Fan et al. (2018), the latent heat released from condensation, deposition, and freezing during cold and warm cloud processes are diagnosed by rerunning the model (Figure R10). The rimming processes are included into the freezing. It is nothing to do with the freezing which means the precipitation enhancement with aerosols cannot be simply attributed to cold cloud invigoration effect due to freezing.

Figure R9 shows the changes in the mass and number concentration of the different hydrometers. The aerosols are activated to form more cloud droplets on which water condenses and produces more cloud water (Figure R9a). This process releases additional latent heat at 3–5 km due to condensation (Figure R10a) and lower supersaturation, which is also discussed in Fan et. al (2018). The smaller radius of cloud droplet shown in Figure 5a is not favorable to fast droplet coalescence and suppress warm rain. The precipitation decreases from 15Z to 20Z on 14 December (Figure S4). With aerosols, the precipitation is increased between 03Z on December 15 to 10Z on December 16. However, the changes in the hydrometers, particular for rain water, and sources of latent heat release are quite different between before and after 15Z on December 15. These differences indicate that the processes and their related mechanisms may differ from each other. In the first stage, before 15Z on December 15, there are abundant ice crystals (i.e. snow and graupel) above the 0°C isotherm around 5 km (Figure S8). With the presence of ice crystals, water vapor deposition is prior to happen on ice surface as the saturation with respect to water is supersaturation with respect to ice. As this occurs, the environment becomes unsaturated to water, resulting in the evaporation of liquid water. This is known as the Bergeron-Findeisen-Wegener theory. Correspondingly, the ice crystals (i.e. cloud ice, snow, and graupel) increase at the expense of rain water. Note the magnitude of snow and graupel mass is ten times of that of rain water. The latent heat release due to deposition in cold cloud is stronger than that due to condensation in warm cloud even though the latter is also important. After 15Z on December 15, most of the ice crystals fall as precipitation. Compared with depositional heating, the condensational heating plays a dominant role in intensifying convective strength. The rain water increases through accretion of added cloud droplets, leading to precipitation increases.

The corresponding figures and discussion are revised in the main text.

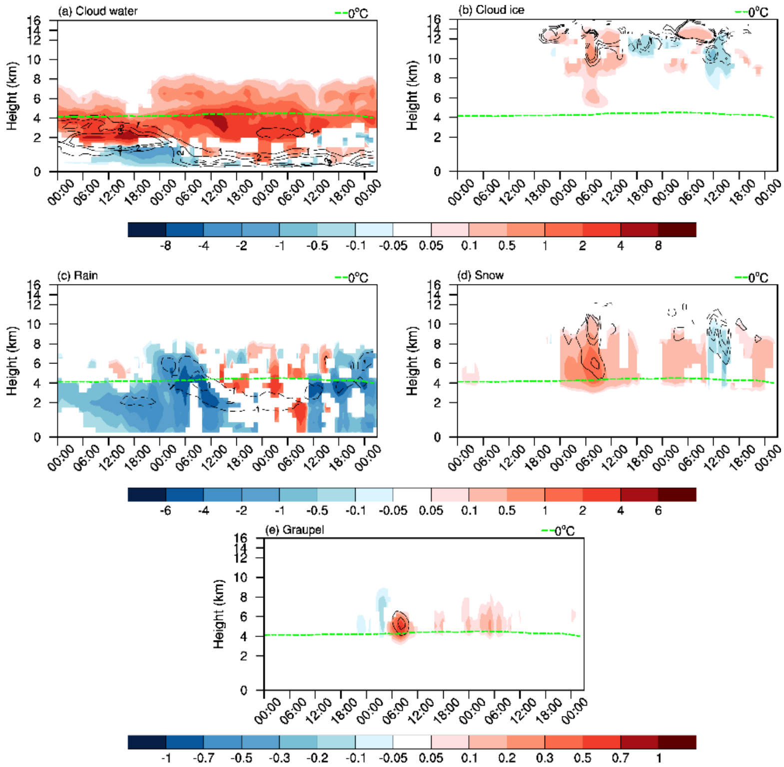

Figure R9. Differences with time (abscissa) and height (ordinate) in (a) cloud water (shading; unit: 10^{-5} kg kg^{-1}) and CDNC (contour; unit: 10^{7} kg^{-1}), (b) cloud ice (shading; unit: 10^{-5} kg kg^{-1}) and CINC (contour; unit: 10^{4} kg^{-1}), (c) rain (shading; unit: 10^{-5} kg kg^{-1}) and rain number concentration (contour; unit: 10^{5} kg^{-1}), (d) snow (shading; unit: 10^{-4} kg kg^{-1}) and snow number concentrations (contour; unit: 10^{3} kg^{-1}), and (e) graupel (shading; unit: 10^{-4} kg kg^{-1}) and graupel number concentration (contour; unit: 10^{3} kg^{-1}) between CTL and CLEAN (i.e. CTL minus CLEAN) averaged over the red box. Only anomalies that exceed 90% significance level are depicted with shading and contour.

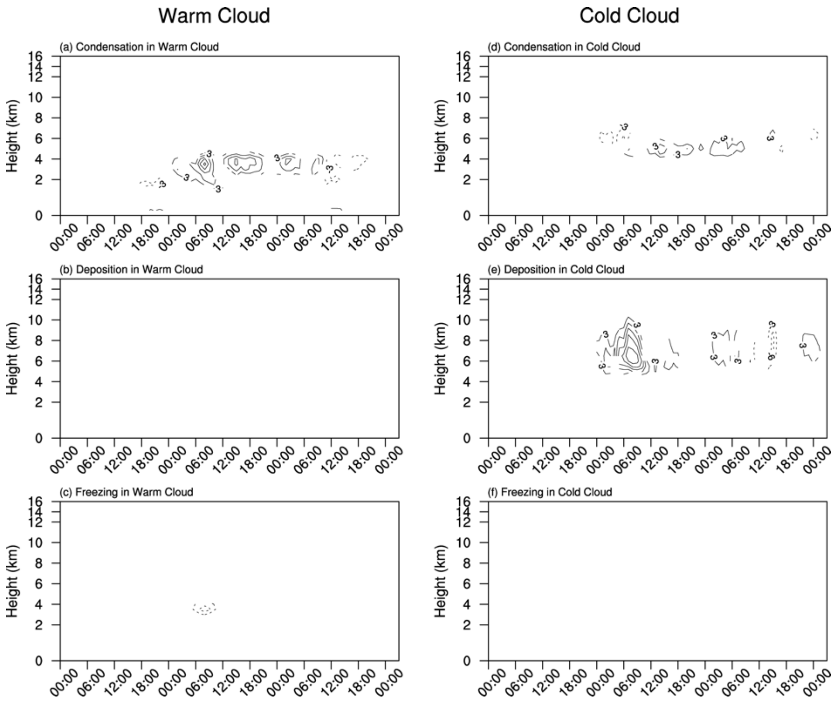

Figure R10. Differences with time (abscissa) and height (ordinate) in latent heat release (unit: K d^{-1}) from (a) condensation, (b) deposition, and (c) freezing processes between CTL and CLEAN (i.e. CTL minus CLEAN) averaged over the red box for the warm cloud. (d–f) Same as (a–c) but from cold cloud. Only anomalies that exceed 90% significance level are depicted with and contour. Zero-value contour lines are omitted, and negative values are dashed. The contour interval is 3 K d^{-1}. Note the blank represent the values are within 3 K d^{-1}.

15. P8, Eq (1), where is the horizontal advection terms for the moisture budget? In the model, this is an important term. If you considered it in the vertically integrated moisture flux (MFC) convergence in your calculation, then the MFC should be large at the convection permitting scale.

Response: Agreed. We integrated the moisture flux convergence (MFC) in vertical direction. As discussed in the manuscript, this term dominates the rainfall changes. The MFC term is further divided into two terms as

$$-\frac{1}{g}\int_0^{P_s} \nabla \cdot \left(q\overrightarrow{V_h}\right) dp = -\frac{1}{g}\int_0^{P_s} q\nabla \cdot \overrightarrow{V_h}\, dp - \frac{1}{g}\int_0^{P_s} \overrightarrow{V_h} \cdot \nabla q\, dp$$

where the first term on the right side is the horizontal moisture convergence (hereafter CON); the second term is the horizontal advection of water vapor (hereafter ADV).

As shown in Figure R11, the CON term dominates the contribution to total MFC. The resemblance of pattern between MFC and CON suggests that the increase in rainfall is mainly driven by CON changes. The descriptions associated with the figures are added in the main text.

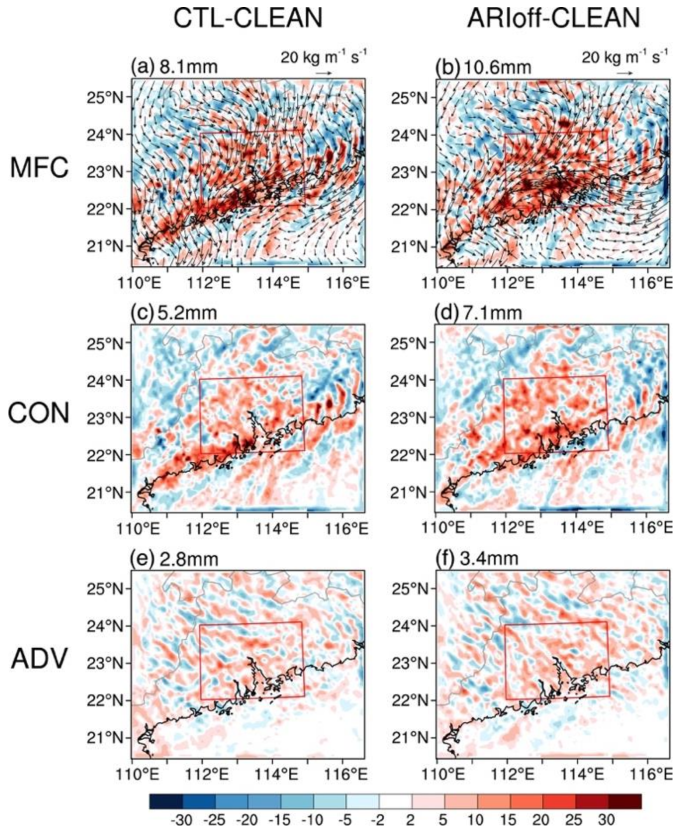

Figure R11. Differences in column-integrated flux convergence (MFC; shading; unit: mm) and moisture flux (vector; unit: kg m^{-1} s^{-1}), between (a) CTL and CLEAN (i.e. CTL minus CLEAN) and (b) ARIoff and CLEAN (i.e., ARIoff minus CLEAN) on December 15. (c, d) Same as (a, b) but for column-integrated moisture convergence (CON; unit: mm). (e, f) Same as (a, b) but for column-integrated advection of water vapor (ADV; unit: mm). The numbers at the top-left corner of each panel represent the values averaged over the red boxes. The red boxes (22°–24° N, 112°–115° E) denote the analysis region.

16. P9, L8, the figure number is wrong. Also, where is the moisture coming from the northerly wind since northerly wind generally brings in drier air? It would be good to show the spatial distribution of moisture field.

Response: Thanks for pointing this out. Yes, the air from the northerlies is drier which is shown in the spatial distribution of water vapor (Figure 20c and 20d). It may thus not correct to claim the moisture from the northerly wind. As shown in Figure R11, the MFC term is dominated by CON term which depends on the convergence field rather than the moisture. The convergence is attributed to the microphysics-dynamics feedback discussed in the manuscript. The statement is revised in the main text (P10 L25–26).

17. P 9, L11-12, since there is compensation effect here, a figure for ARI effect should be shown to quantify how much is the compensation effect.

Response: The ARI effect is included in the revised figure (Figure 8).

18. P9, L16-20, again, key processes are not shown and the summary description might not be accurate. First, it is not correct to say "water clouds ascend to freeze into ice clouds" since it is just that more cloud droplets are lifted to the higher levels and form more ice

particles. Second, as I pointed out above, the source of latent heat enhancement is not examined and the authors just assumed it is mainly resulted from more droplet freezing. Third, the much enhanced horizontal convergence could be gradually induced by other feedback such as precipitation or radiation since the simulation duration are a few days, not just a few hours. Another question is that how the changes in domain 1 impact the results over domain 2?

Response: Thanks for the comments. Based on Figure R10, the source of latent heat is mainly induced by deposition before 15Z on December and condensation after 15Z when the precipitation is increased with aerosols. Although the simulation duration is a couple of days, the precipitation increases with aerosols only occur between 06Z on December 15 to 10Z on December 16. Moreover, the persistent convective system makes the impact last for longer time. Strong latent heat is released during this period, and ARI has a little impact on the increased precipitation. These results drive to conclude that latent heat release is the main reason for enhanced horizontal convergence.

With aerosol emissions in domain 1, the aerosols are emitted or formed. The aerosol concentration is transported to domain 2 through lateral boundary conditions.

19. Section 3.3, the remote and local aerosol effects can strongly depend on how strong the coupling between the two domains. With the two domains running together, the coupling is very strong and the Dom 1 keeps updating Dom 2, which could lead to very strong effect from any variable in Dom 1(not just aerosol). If you run domain 2 separately with the IC and BC updated in every 3-hours or 6 hours from Dom 1, and do the same studies, the results could be changed.

Response: Thanks for the comments. It is correct that running domain 2 separately would change the results, but it does not reflect the real situation. In reality, atmosphere does not have any domain, and should be highly connected. To reflect the real situation, the domain 1 and domain 2 should be online coupled by running them together. In addition, following the commonly used approach, the results of outermost ten grid points of each boundary of domain 2 are excluded to minimize the influence from the lateral boundary conditions.

20. P10, L28-29, this may indicate secondary droplet nucleation, meaning activating enormous smaller aerosols at higher-levels due to higher supersaturation. Without looking at it carefully, you can not just assume it is mainly because of ascent of cloud droplets.

Response: Thanks for your constructive comments. We agree your opinion that the increase of cloud droplets at 1.5–4 km cannot be attributed to ascent motion as the vertical velocity is reduced in the 10× simulation. With ten-time changes in aerosol emissions, more aerosols are activated to form cloud droplets at higher level due to higher supersaturation. The consumption of moisture and energy limits the formation of low cloud. The content is revised accordingly. Thank you.

21. P11, L1-6, again, you can not just guess by citing a literature work assuming it apply to your study. Key results need to be shown. The reduction of low-level cloud could just because more deep cloud form consuming moisture and energy which would limit the formation of other type of clouds. Evaporation and sublimation have to come from clouds. In addition, the lower-level cloud and the high-level clouds shown here might not be

vertically connected over the domain. For example, shallow clouds could mainly occur in the northern part of the red box associated with cold front and deep clouds could mainly occur in the southern part associated with the convergence zone. Increasing aerosols suppresses shallow convection, which would be different from the story you describe here now. It would not be nothing to do with sublimation if that is the case.

Response: Thanks for your comments. We agree that the low cloud reduction is because of the consumption of moisture and energy due to formation of high-level cloud. The cloud regimes are quite consistent in the northern and southern parts of the red box as shown in Figure R2–R6. Figure R13 shows the latent heat release due to condensation, freezing, and deposition for both warm and cold cloud. Deposition is the most important factor while freezing play a negligible role in this case. The strong latent heat released from deposition is consistent with the snow increase from 00Z to 12Z on 15 December. The underline mechanism is related to the Bergeron-Findeisen-Wegener theory as discussed in the responses to comment 14 but with a much stronger magnitude. However, after 15Z on December 15, the changes in rain water mass and latent heat in 10× are quite different from that in control simulation. We agree that our previous description may not fully reflect the mechanism. The reason is thus discussed in the revised main text. Thank you for your comment.

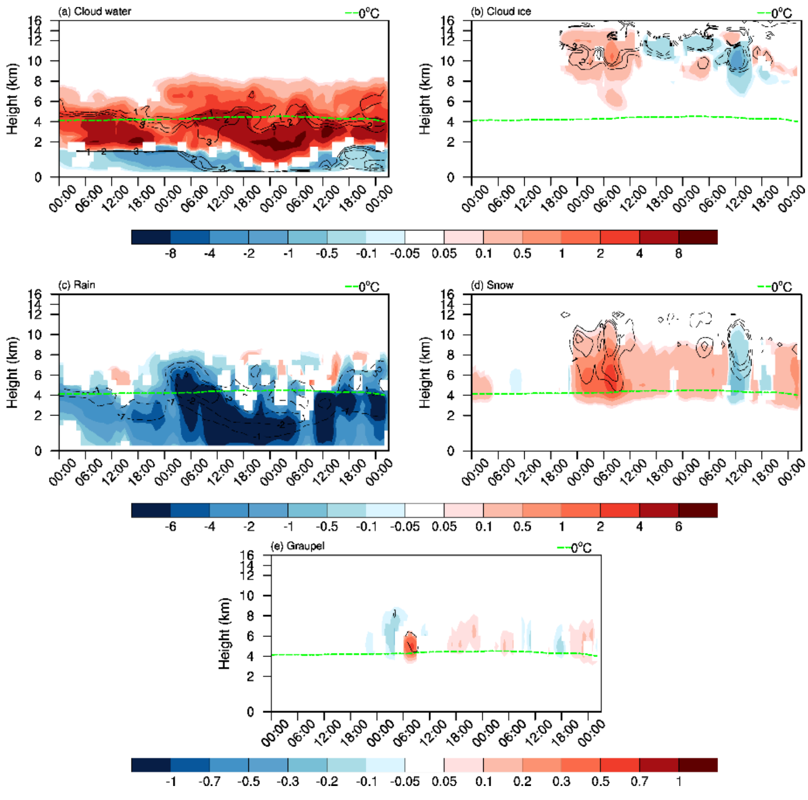

Figure R12. Differences with time (abscissa) and height (ordinate) in (a) cloud water (shading; unit: 10^{-5} kg kg^{-1}) and CDNC (contour; unit: 10^{7} kg^{-1}), (b) cloud ice (shading; unit: 10^{-5} kg kg^{-1}) and CINC (contour; unit: 10^{4} kg^{-1}), (c) rain (shading; unit: 10^{-5} kg kg^{-1}) and rain number concentration (contour; unit: 10^{5} kg^{-1}), (d) snow (shading; unit: 10^{-4} kg kg^{-1}) and snow number concentrations (contour; unit: 10^{3} kg^{-1}), and (e) graupel (shading; unit: 10^{-4} kg kg^{-1}) and graupel number concentration (contour; unit: 10^{3} kg^{-1}) between 10× and CLEAN (i.e. 10× minus CLEAN) averaged over the red box. Only anomalies that exceed 90% significance level are depicted with shading and contour.

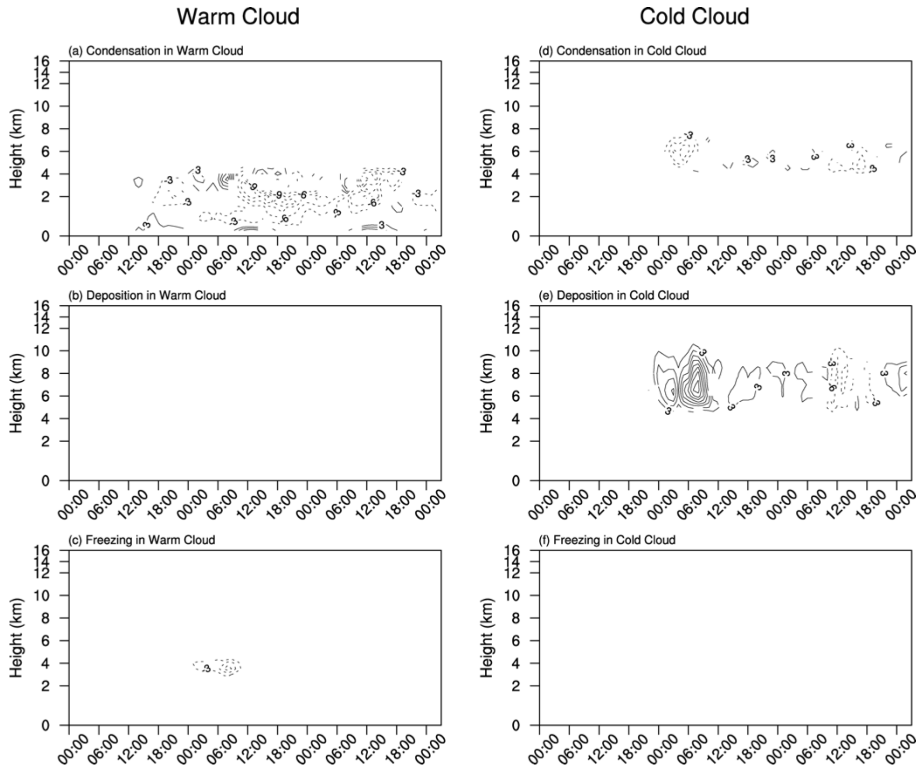

Figure R13. Differences with time (abscissa) and height (ordinate) in latent heat release (unit: K d^{-1}) from (a) condensation, (b) deposition, and (c) freezing processes between 10× and CLEAN (i.e. 10× minus CLEAN) averaged over the red box for the warm cloud. (d–f) Same as (a–c) but from cold cloud. Only anomalies that exceed 90% significance level are depicted with and contour. Zero-value contour lines are omitted, and negative values are dashed. The contour interval is 3 K d^{-1}. Note the blank represent the values are within 3 K d^{-1}.

22. P11, L8-17, do not agree with some of the discussion. Compared with Figure 3, I only see the corresponding increase and decrease in the Dom 2 become more significant in Figure 12. The authors did not discuss why there are two significantly different precipitation response regimes to the change of emissions. It seems that they are located in different dynamic regimes so have different cloud types. More detailed description about what types of clouds were formed in the cold side is needed in the description of the case at the beginning of the result section. It would provide basis for the related discussion after that.

Response: Thanks for pointing this out. We choose a box located over the area (24–25°N, 110–112°E) where precipitation is decreased. The corresponding analysis are put in the paragraph six of discussion. The cloud types in domain 2 are also discussed in the beginning part of the result section.

23. Section 4: a. First paragraph, Summary should include description of what have been done as well.

b. Second paragraph, see my comment above about how to look at different aerosol impacts on different cloud regimes/types. The current discussion might not relevant because the cloud types should be very different between the code and warm sides of the frontal system.

c. The third and fourth paragraphs may need to be changed accordingly after my relevant comments above are addressed.

Response: Yes, these paragraphs are thus revised accordingly in the revised main text.

Grammatical problems: P4, L19: grammar error. P7, L6: grammar error. P7., L32, past tense is not needed here. There are many places in results section that have the mixed past and current tenses. Better to be consistent in tense to improve readability and avoid confusion.

Response: Thanks for pointing this out. The grammar errors are corrected. The result section is revised with current tense to keep consistency.

Reviewer 2:

This study employed the WRF-Chem model and a series of sensitivity experiments to study the aerosol microphysical and radiative impacts on a historical heavy precipitation event in southern China. The effects of local and remote aerosols are compared by altering aerosol concentrations in different domains. The finding about the aerosol invigoration effect with a moderate aerosol increase generally agrees with the existing argument that aerosols tend to induce more extreme precipitation. The topic of study is important and fits with the scope of ACP very well. However, there are still lots of unclear writing and insufficient analyses in the manuscript. Major revisions are needed before it can be accepted by ACP.

Two major comments:

1) It is kind of surprising to me that the simulated aerosol properties and spatial distributions are not shown in the manuscript. They are actually about the strength of the WRF-Chem model in doing aerosol-cloud research. PM2.5 is plotted, but it is not quantitative index for either CCN effect or radiative effect. The spatial distributions are critical for us to understand the potential influence of remote aerosols. The aerosol chemical component determines the aerosol radiative properties, absorbing or scattering, as well as CCN ability.

2) Process-level analyses on ACI and ARI in this case should be strengthened. For ARI, I do not see any analysis on the radiative fluxes, temperature field, and associated dynamical adjustment. Is there any atmospheric heating due to black or brown carbon in this case? For ACI, the microphysical properties of all hydrometeor and co-varying water vapor field should be studied. See more specific comments below.

Response: We thank the reviewer for the through and thoughtful comments. We tried our best to address all concerns and have revised the manuscript accordingly. Hope you find our revisions useful. Thank you very much.

For your questions, we only considered black carbon for atmospheric heating in our simulations because there is lack of reliable parameterization for brown carbon in our study region. The analysis was focused on the ACI impact because of its dominant role in this case.

Specific comments:

1) I feel the literature review in the introduction part is not done thoroughly. Considering both ACI and ARI have been extensively investigated for the past 10-20 years, more credits should be given to the studies with the similar topic.

- **P2L4, inaccurate statement. Actually, there are lots of existing studies on the influence of aerosols on different types of extreme weather, such as tropical cyclone (Wang et al., 2014, Nat. Clim. Change; Zhao et al., 2018, GRL), hail storm (Ilotoviz, et al., 2016, JAS), etc.**
- **P3L1, the competition between ARI and ACI has been widely discussed on both cloud- resolving scale (Lin et al., 2017, JAS; Wang et al., 2018, AAS) as well as regional climate scale (Wang et al., 2016, JGR).**

- **P3L3-5, different aerosol types can be a critical factor as well to determine the invigoration or suppression effect of aerosols (Jiang et al., 2018, Nat. Commu.).**

Response: Thanks for pointing this out. The studies are cited and a more through literature review are added into the introduction part.

2) P3L15-20, it is not clear what are hypotheses for the different effects from local and remote aerosol emissions? Different concentrations, chemical compositions, or spatial distributions? What did observations tell us about their differences? Without stating those explicitly, readers fail to follow the logic flow of the paper.

Response: We thank the reviewer for pointing this out. The different effects from local and remote aerosol emissions refer to different aerosol concentrations. Figure R14 shows the spatial distribution of aerosol optical depth during December 13–16, 2013. The values are missing over the southern China because of the mask effect of cloud. The aerosol optical depth is higher than 1 over north-eastern China, indicating strong air pollution. Given the wind pattern in Figure S1b, the aerosol concentrations over local region could be from either local emission or transport by monsoonal flow. As shown in Figure 10, the aerosol concentrations from local aerosol emissions accumulate near the surface decrease dramatically once the peak rainfall initiated. By contrast, the aerosols from transport extend a higher altitude in the atmosphere and last for much longer time. These statements are added in the main text.

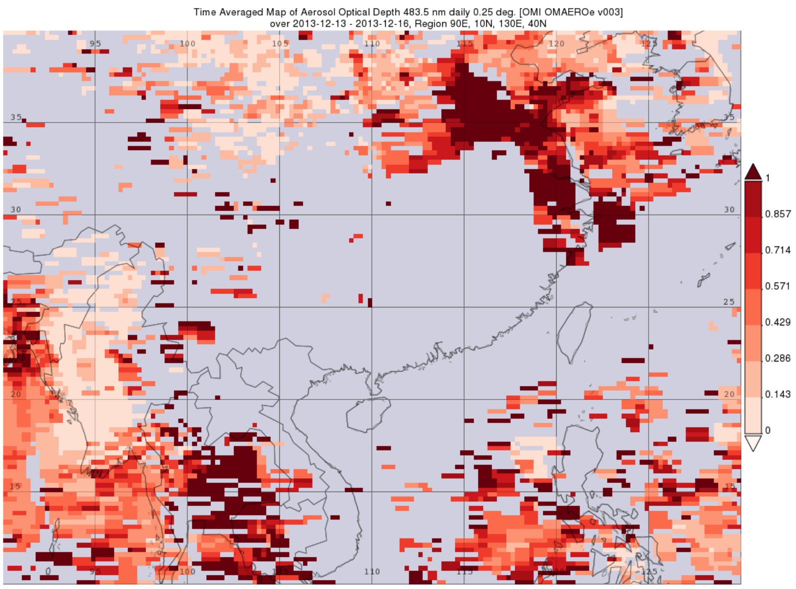

Figure R14. Spatial distribution of averaged aerosol optical depth at 483 nm from OMI during December 14–16, 2013 in 10°–40°N, 90°–130°E.

3) Fig. 2b and 2c, rather than only showing the dots over the stations, I suggest to plot the rainfall map over the whole domain for model and satellite, which is helpful to characterize the system. You can still keep open circles to compare the rainfall over each station.

Response: Thanks for your suggestions. The rainfall map over the whole domain is aded in the revised manuscript (Figure 2).

4) Fig. 1b, the photo here deliver very litter information. Suggest to replace by the wind pattern analysis . Also, P6L20, a more accurate expression here is "monsoon system" or "monsoonal flow".

Response: The photo is replaced by the suggested wind pattern figure, while the descriptions are also corrected.

5) P6L29-31, why not using TRMM which is better at heavy precipitation? What is the point to show a satellite product even worse than the model?

Response: The CMORPH data was used in this work because of its higher spatial and temporal resolutions (i.e. 8 km and 30 mins) than those of TRMM. The resolution is comparable to that of model output (i.e. 4 km and 1 hour, respectively) and rain gauge data. The finer temporal resolution allows us to check the aerosols' effect at the peak time of the study event, which was previously discussed in the literature. In addition, the CMOPH data was also used in recent studies (e.g. Zhong et al., 2015) to evaluate the model performance on extreme rainfall cases.

To have a better understanding about the two data sets, we conducted a comparison between them. Figure R15d shows the TRMM data for this case. The TRMM data shows a better performance over south-western Guangdong Province and western Guangxi Province, in which CMORPH may underestimate precipitation. For precipitation along the coast and over the Pearl River Delta region, even though TRMM's performance is better than CMPRPH's, TRMM also shows an underestimation.

Overall, as explained above, CMORPH was used due to its higher spatiotemporal resolutions and would like to use the similar dataset used in previous studies to provide a fair comparison with literature. The discussion is added in the revised manuscript. Thank you.

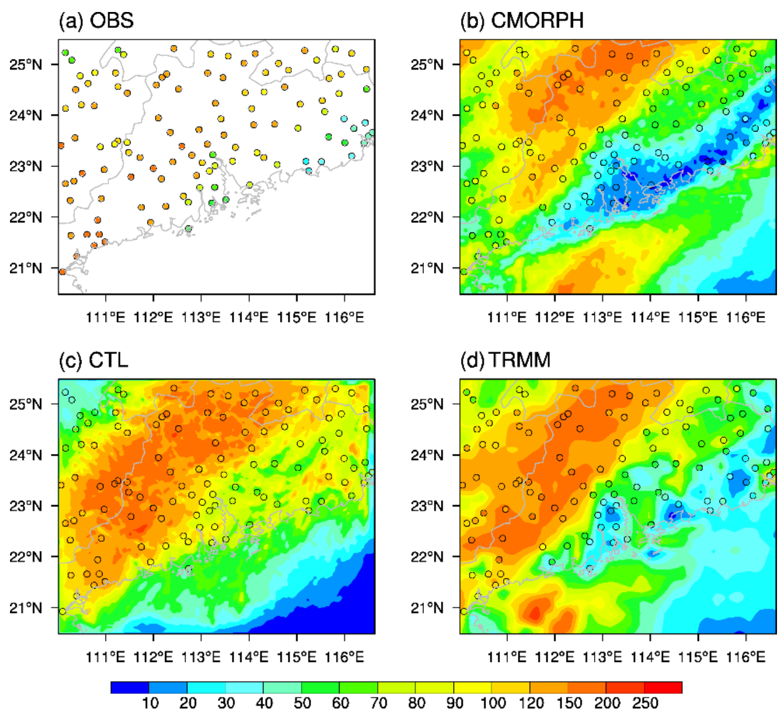

Figure R15. Spatial distribution of accumulated precipitation (unit: mm) from 00Z on December 14, 2013, to 00Z on December 17, 2013 from (a) station observations (OBS), (b)

CMORPH, (c) control simulation (CTL), and (d) TRMM. Circles denote locations of in situ observations.

6) The physical meaning of ARIoff - CLEAN is not obvious, as the authors use (CTRL - CLEAN) - (CTRL - ARIoff) to approximate ACI. I suggest the authors state this assumption explicitly and use ACI to replace ARIoff - CLEAN for all figure legends. Also, be careful about the usage difference between hyphen and minus sign.

Response: Thanks for pointing this out. As suggested, ARIoff – CLEAN is replaced by the term ACI in all figures' legends and captions. The hyphen and minus sign are distinguished in all figure legends.

7) It is unclear for me how the statistical analysis is conducted in those figures. As I understand, the authors only have one run for each model configuration. How to get the sufficient samples for the Student's t-test at each grid point?

Response: As mentioned in P5 L4–6, to isolate robust signals from model natural variations, five ensemble members with a perturbed initial time at 3-h intervals were conducted for each experiment. The significance level was calculated based on the five ensemble members.

8) P8L15-25, the authors only mentioned about the latent heat from droplet freezing. However, according to Fig. 5, clearly there is a significant portion of latent heat release below 4 km (warmer than 0 degree C). Can you plot the changes in liquid water content to confirm it? For the oceanic DCC, aerosol induced diabetic heating has two peaks, one in the warm portion and one in the mixed-phase portion (Fig. 3a of Wang et al., 2014, Nat. Commun.). Another interesting point here is that the Morrison microphysical scheme in WRF-Chem uses the simple water vapor saturation adjustment for condensation/evaporation. I speculate that this scheme cannot account for CCN effect in fostering condensation. Since this paper indirectly infers more liquid water forms, it is intriguing to see why.

Response: Thanks for pointing this out. We agree that the condensational heat below freezing level also plays an important role. The source of latent heat is found to be not related to droplet freezing. Figure R16 shows the differences in cloud water and cloud ice induced by aerosols. The liquid water content increases below 0°C during almost all the period. The cloud ice also increases when the rainfall peak happens. The latent heat from microphysical processes are further divided into three parts from condensation, deposition, and freezing for warm cloud and cold cloud (Figure R17). Note the rimming processes are included into the freezing. Aerosol induced diabatic heating also has two net heating peaks in this case. However, the peaks are much higher than that in Wang et al. (2014) for oceanic deep convection, and just slight cooling occurs due to melting in warm cloud (Figure R17c). The net heating peaks are attributed to condensation in warm cloud and deposition in cold cloud at the height of 3 km and 7 km, respectively. In CLEAN experiment, fast coalescence forms warm rain and reduces the integrated droplet surface area, leading to supersaturated clouds. With aerosols, additional number of cloud droplets are nucleated (Figure 5a) on which water vapor condenses. This is consistent with Fan et al. (2018). Contents in the main text are revised accordingly (P9 L4–20). Thank you very much for your comment.

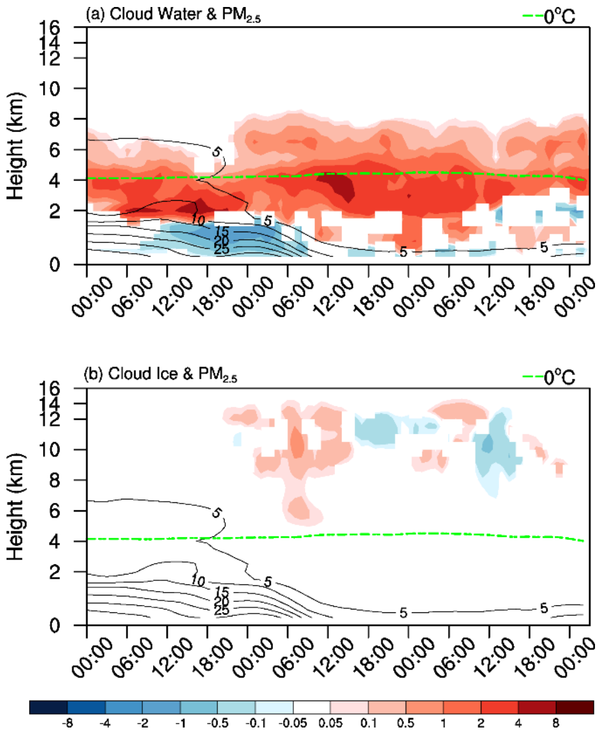

Figure R16. Differences with time (abscissa) and height (ordinate) in (a) cloud water (shading; unit: 10^{-5} kg kg^{-1}) and $PM_{2.5}$ concentrations (contour; unit: μg m^{-3}) (b) cloud ice (shading; unit: 10^{-5} kg kg^{-1}) and $PM_{2.5}$ concentrations (contour; unit: μg m^{-3}) between CTL and CLEAN (i.e. CTL minus CLEAN). Only cloud water, cloud ice, and $PM_{2.5}$ concentration anomalies that exceed 90% significance level are depicted with shading and contour. Green dashed lines denote 0°C isotherm calculated as the averaged zero-layer height over the red box in Figure 3.

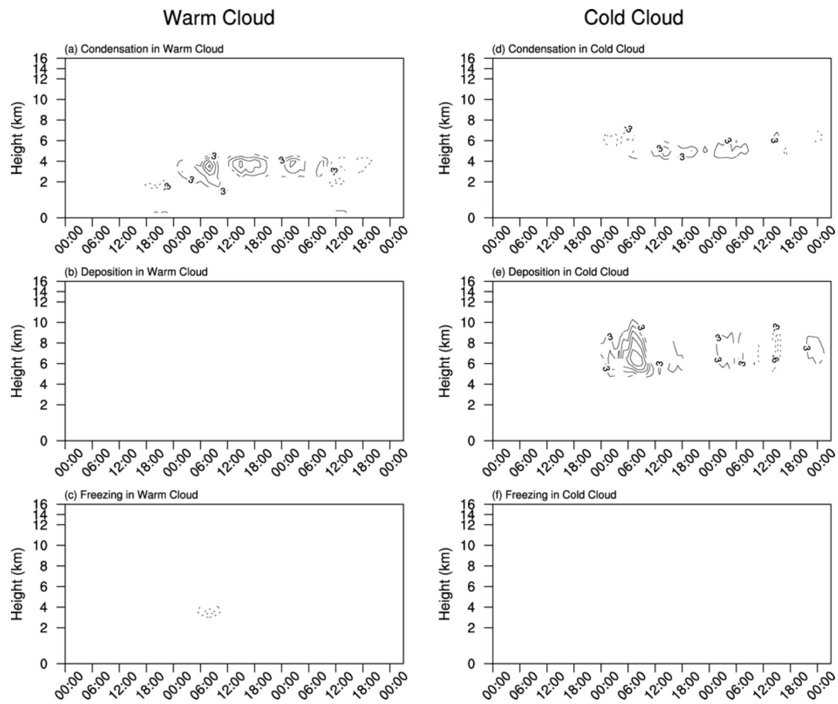

Figure R17. Differences with time (abscissa) and height (ordinate) in latent heat release (unit: K d^{-1}) from (a) condensation, (b) deposition, and (c) freezing processes between CTL and CLEAN (i.e. CTL minus CLEAN) for the warm cloud. (d–f) Same as (a–c) but from cold cloud.

Only anomalies that exceed 90% significance level are depicted with and contour. Zero-value contour lines are omitted, and negative values are dashed. The contour interval is 3 K d^{-1}. Note the blank represent the values are within 3 K d^{-1}.

9) Fig. 4,7, cloud fraction is about cloud macrophysics, which may not accurately reflect changes in cloud microphysics (water content, number concentration). The latter are more relevant with the aerosol invigoration effect. As mentioned above, I strongly suggest the authors plot and systematically analyze the changes in the mass and number concentration of the different hydrometeors.

Response: The cloud fraction calculation in our model follows Randall (Hong et al., 1998) with value range from zero to one. Their values were calculated as the sum of cloud water, cloud ice and snow, which actually was based on mass. We chose cloud fraction in Figure 4 and Figure 7 because this variable is an indicator of mass for both liquid and ice clouds. The changes in the mass and number concentration of different hydrometeors are also analysed in Figure 6 and Figure 7 in the revised manuscript.