Atmos. Chem. Phys. Discuss.,
https://doi.org/10.5194/acp-2018-977-RC1, 2018

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Contribution of local and remote anthropogenic aerosols to intensification of a record-breaking torrential rainfall event in Guangdong province" *by* Zhen Liu et al.

Anonymous Referee #1

Received and published: 26 November 2018

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 remote 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: The study has a good scientific value. However, the current paper is

not appropriate for a publication yet. The main problems this reviewer found are, 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. 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. 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.

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).

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 antoconversion form cloud droplets?

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

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?

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

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?

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.

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

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

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.

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?

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.

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

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.

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.

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.

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.

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?

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.

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.

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.

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.

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.

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.

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