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.

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.).

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.

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.

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”.

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?

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.

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?

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.

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.