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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Potential impact of aerosols on convective clouds revealed by Himawari-8 observations over different terrain types in eastern China" *by* Tianmeng Chen et al.

Anonymous Referee #1

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1 General comments

Chen et al. present in their study "Potential impact of aerosols on convective clouds revealed by Himawari-8 observations over different terrain types in eastern China" the impact of terrain height, meteorological parameters, and diurnal cycle on the aerosol-cloud interaction. The authors used two seasons from May-September 2016-2017 of observations from the geostationary satellite Himawari to retrieve the cloud mask, particulate matter from measurement station to retrieve the pollution concentration, MERRA-2 reanalysis to retrieve lower-tropospheric stability, vertical velocity, potential temperature, relative humidity, and specific humidity at different levels in accordance

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with the terrain height. The study describes an innovative convective cloud mask that the authors developed and they collocate spatially and temporally with pollution and meteorological information. They infer an exhaustive list of interaction of clouds with their environment with some important features on the aerosol-cloud interaction, convective clouds occur more likely under unstable environment with some caveat comparing the morning and the afternoon and they compared between clean and polluted environment for example. I specifically appreciated that the authors provide a physical explanation for the different observations they acknowledged that they are using observations and further work is needed to support the interpretations and conclusion.

The overall presentation is well structured and clear and the Figure are explanatory and provide argument for the text. The scientific method and assumptions seem valid and they are clearly outlined. I have some concerns regarding the conclusion of the article and the strong statement that the article study the impact of aerosol on convective clouds but I am not sure about that, as the data are not compared with clear sky situation, I developed what I mean in the next section. I have also some concern about the cloud mask validation, but I think it just a lake of details from the current version. Finally, working with a large amount of data, statistical tests are missing to quantify the effect observed. Otherwise, the topic and the results fit totally within the scope of ACP and I strongly recommend the publication in the ACP after some modifications.

2 Main comments

1. The study compares different regimes, handling a large dataset and the conclusion often belongs to observation from the Figures. Quantification through statistical tests are often missing to support the description and the conclusions. I put some examples here:

    - l. 394: The authors mention "non negligible contributor...", the changes

in ω do not seem statistically significant, did the authors try to perform a statistical test?

- The key of the study is explained in lines 598-602, with the explanation of decorellation between meteorological parameters and the PM2.5 concentration. I do not know if if we can say that q (for example) is uncorrelated with PM2.5. Did the authors try to quantify the potential correlation between the different parameters?
- l. 537: The authors mention "especially before 14:00 LT", I do not see a difference at all between polluted and clean after 14:00 from Figure 10, I think the term "especially" should be change for "only". For this entire section, I doubt that the results are statistically significant, did the authors try to perform a test?
- Line 618: CCF peaks decrease under stronger updraft, I am not sure to understand what it is meant here, I think quantification would help.
- I am not convinced by Figure 11, the differences between the graphs are not high. Did the authors try to quantify the difference with a statistical test? Moreover, the highest terrain is not the one with the minimum CC fraction for PM2.5 greater than 30mug/m3. I am curious on how does it fit on the author's explanation? Is it the different between plateaus and hill?

2. I need some clarification about the cloud mask. l.295-l.307: Some of the thresholds from which the cloud mask is based on correspond sometime to mature and small convective clouds, is it a problem? Did the authors try to change the different thresholds? If yes, how does it affect the results?

   The validation of the cloud mask has been performed on a specific day. The method works well on a day with a lot of convective clouds, but how does it perform if there are less convective clouds? When the authors compare with ISCCP regimes, does it refer to the cloud optical depth/cloud top pressure diagram? Can

the authors describe how the ISCCP define convective clouds? If I understand correctly, at 8:00 there is an accuracy of 20% of the author's cloud mask algorithm to detect convective clouds, is it correct? I consider the author's method more robust than the COT-CTP diagram. Can the authors comment on that? The results from this part are interesting but I am not convinced that it serves as a validation of their algorithm.

On Figure 3, there are some clouds that are not detected by the algorithm but they are by MODIS and they could be convective (on the eastern part of the figure), can the authors have an explanation for that and can they comment it? In the article, the authors mention the opposite and refer to other cloudy pixels. I think it mainly refer to cloud edges, can the authors comment on that?

3. I am confused about the aim and the conclusions of the study. On line 642 "However testing whether the results are due mainly to aerosol effects is only a first step", the authors answered many questions in the manuscript but I do not see how we can affirm that aerosols are the main contributor to convective cloud occurrence. Further analysis would be needed to study the aerosol-cloud interaction, comparing with non cloudy pixel occurrence constrained for meteorological parameters for example. I do not know if the meteorological conditions discussed in the text favored the convective cloud formation or "simply" the cloud formation. It is acknowledged in the text but it can be misleading in many parts of the manuscript.

   How are the meteorological parameter variations with clear sky occurrence (for example Figure 5). Is it really the conditions which favored the convective cloud occurrence or is it the meteorological conditions difference between the areas and an other parameter which favor or inhibit the cloud formation? Do the authors take that into account?

4. Cloud interactions with aerosols and cloud processes are different over land or

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over ocean. In the study the authors merged over open ocean and over land. Did the authors try to remove the ocean in their analysis?

3 Specific comments

1. Figure color bars: The rainbow color bar is not suited for colorblind people, I suggest to change it.
2. l. 37: I suggest to change to "convective cloud fraction increaseS then decreaseS"
3. l. 44: I suggest to change to "aerosolS decrease. . . "
4. l. 50: I suggest to change to "Convective cloudS are. . . "
5. l. 57: A space is missing between "climate" and (Zhao et. . .)
6. l. 60: I suggest to change to "light-absorbing aerosolS"
7. l. 103: "In recent years. . . ", then the authors refer to Lynn et al. (2007), is it still considered as "recent years"
8. l. 105: "WRF" is not spelled out I think
9. l. 113: "Only few studies. . . ", can the authors cite the few studies they are referring to.
10. section 2.1: It is not clear here if they consider data over ocean or not.
11. l. 160: Can the authors plot the region of interest in Figure 1, it is not clear which region is covered or not.

12. l. 188: Is PM_1 used in the study? The article mainly refer to $PM_{2.5}$ only.

13. l. 253: Is q really at the surface or at 2m above surface?

14. l. 271: What is the matrix size?

15. l. 300: "With mean contrast>3.5", where does the value of 3.5 comes from?

16. l.312: I suggest to change to "isolating convective cloudS"

17. Fig. S1: Is this graph only for July 30th 2016 or for the two seasons considered later?

18. Fig. 4: It is difficult to distinguish the gray line, the author should highlight it differently.

19. l. 398: "most common", do the authors mean "higher".

20. Fig. 4: Are the sub-figures snapshot of the specific time indicated or is it integrated over two hours?

21. l. 422-424: Considering the potentially high spatial variability of aerosol with the different terrain heights, how good is the "nearest-pixel" assumption?

22. Fig. 6: It is difficult to distinguish between the red and magenta lines, I suggest to find other colors.

23. l. 535: "We can see from Figure 11...", I think the authors want to refer to Figure 10.

24. l. 706: I suggest to change "convection by the enhancing" to "convection by enhancing"

25. l. 708: I suggest to change "development of convective cloud" to "development of convective cloudS"

26. Figure 7: There are red spots that the authors highlight with the presence of cities. What about the other red spots, is there any reason?

27. Figure 8: The caption mentions "red solid lines" but the lines seems dashed.

28. references: Many doi are missing

29. There are "Houze Jr, R. Q.: Cloud dynamics, Academic press" twice, for 1993 and 2014. Can the authors use only one edition ? I would suggest the most recent one.

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