Atmospheric Chemistry and Physics Discussions

Atmos. Chem. Phys. Discuss., referee comment RC2
https://doi.org/10.5194/acp-2022-492-RC2, 2022

Comment on acp-2022-492

Anonymous Referee #2

Referee comment on "The impacts of dust aerosol and convective available potential energy on precipitation vertical structure in southeastern China as seen from multisource observations" by Hongxia Zhu et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2022-492-RC2, 2022

This study investigates the possible impacts of dust aerosol and convective available potential energy on precipitation vertical structure by using multiple source observations. Great efforts have been made to try to disentangle the aerosol and meteorological conditions. This manuscript is well designed, and the analysis methods are technically sound. However, the scientific and presentation quality need significant improvement. For example, 1) introduction section is a bit confusing for me. A lot of studies and findings were simply listed in a detailed but unclear way. In the end, it is still not clear what are the challenges in this topic and especially why this study is novel, which hampers the manuscript. I recommend the authors to revise this section into a good story; 2) section 3.2 is important and actually contains quite a lot interesting findings. But only the simple descriptions were presented without giving any discussion, implication or even comparison with previous studies. After reading this section, I don't really get scientifically useful information. It's more like a technical report. As such, I would recommend its publication pending the above-mentioned concerns and following specific comments satisfactorily addressed.

Specific comments

Line 16: How did author define the 'pristine days'? It is incorrectly used if the authors only meant days with low dust concentrations, because other aerosol can dominate especially over east China.

In lines 49-57, the author showed the findings of dust aerosol weakening convection precipitation but immediately in lines 59-65 the opposite was listed. I would expect at least an explanation / mention here.

Lines 133-136: this is a repetition of lines 130-132.

MODIS-retrieved aerosol size parameters have little quantitative skill over land (e.g., https://doi.org/10.5194/amt-4- 201-2011). Thus, derivation of CMAOD from FMF is not a good try. In addition, how did the author consider the aerosol humidification effect in the presence of precipitation?

Lines 167-169: But it's not always the case and even rarely happens that one precipitating grid can be surrounded by eight clear-sky grids.

Lines 169-171: Did the author take the study region as a whole when defining "dusty day"? For example, for a individual day, mean clear-sky CMAOD surrounding precipitating grids is larger than 0.5, in this case, how did the authors deal with other clear-sky CMAOD far away from precipitation? Also classify it as dusty days? This is not clear.

Line 173: It's better to clarify how the authors did the spatial and temporal co-locations between TRMM and ERA5?

Lines 227-229: It is true for convective clouds but not for stratiform clouds. Can the authors explain the reason?

Line 231: Please develop a bit how dust can suppress warm rain?

Lines 236-239: As I understand, the contoured frequency by altitude diagrams is 2D probability density distribution, which represents how the data concentrate. Thus, it can not be used to illustrate if dust increases or decreases LH for a specific altitude. To do so, one should normalize data so that probability sums to 1 for each altitude, so called 'joint-histgram' .

Figure 6: Three LH methods are quite different with each other. I was wondering if the LH

profiles are reliable? Why did author chosoe VPH in Figure 5? I don't see any validation studies were cited. It is expected that the results will change quite a lot and also the conclusion will not hold anymore if other two methods are used since the vertical profiles have large difference as shown in Fig. 6.

Line 254: Why the warm rain was sometimes included and but sometimes not? Any reason?

Figure 9: It's interesting that the dependence of Slope on PTT is getting stronger from C to A. Could the authors develop a bit on this? Also, Fig.9 was kind of repeating Fig.7 & 8. Although the plot types are different, all information as discussed in Fig 9 can be also seen in Fig 7&8. I recommend the author to condense a bit or put one into SI.

Line 313-315: What is the regression slope mentioned here? Can the authors explain more? How can the similar slopes indicate the growth rates of rain drops are similar under 'pristine environment'?

Line 331: Good idea!

Lines 322-323: Any references support such argument?