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Atmospheric Chemistry and Physics Discussions

Interactive comment on "The evolution of cloud microphysics upon aerosol interaction at the summit of Mt. Tai, China" *by* Jiarong Li et al.

Anonymous Referee #1

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General comments: This study investigates aerosol-cloud-interactions (ACI) using measurements from the high mountain site of Mt. Tai in China. As limited studies of ACI exist from high altitude measurement stations in this region, the study can potentially provide some useful data about these complex processes to the scientific community. However, the methodologies employed within this manuscript to investigate ACI are questionable, and lacking the necessary in-depth analysis currently associated with probing ACI - one of the most challenging topics currently facing the climate community. A number of conclusions presented are unsupported by the data, and rather arbitrary in nature. Numerous statements throughout the manuscript are not persuasive or lack evidence. Furthermore, the manuscript is not organised very well and the language and grammar throughout is far from the quality required for a scientific

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Discussion paper

publication. Given the large concerns associated with some of the methodologies applied, subsequent conclusions drawn, and the general quality of the text I recommend a major revision of the entire manuscript before consideration for publication.

Major comments

Methodology:

When investigating ACI it is of crucial importance to separate the contributions of changes in both aerosol and meteorology on any observed or simulated cloud response; as performed in other studies of ACI in the scientific literature, e.g. Malavelle et al., 2017. There does not appear to have been any attempt to account for variations in the meteorology in this study. If any of the analysis related to ACI is to remain in the manuscript, the study should include, but not limited to, the following additional analysis:

A. A detailed description of the measurement station with regard to location of instruments and prevailing meteorology. A picture and/or schematic is required to put the results in context of the environment in which they were measured, in particular, statistics on the height of measurements in relation to cloud base/top.

B. Isolating the role of below cloud variations in meteorology, (updraft velocity) on in-cloud variations in supersaturation and cloud microphysical properties, e.g. cloud liquid water content; cloud droplet effective radius, cloud droplet number concentration. There is a vast amount of literature addressing this, e.g. Lance et al., 2004. If observations of cloud base updraft are not available, then alternative approaches should be sought, e.g. using a cloud model in conjunction with the in-cloud measurements to probe sensitivity to variations in meteorology in a robust manner.

C. Accounting for the role of measurement height relative to cloud base in analysis. The measured cloud microphysical properties will be strongly dependant at the height they are measured in relation to cloud-base. This needs to be accounted for in the data analysis prior to drawing conclusions regarding the role of aerosols on measured cloud properties.

D. Accounting for the role of topography on cloud droplet formation (Romakkaniemi et al., 2017).

E. A more robust isolation of anthropogenic pollution using either air-mass back trajectory based approaches such as Tunved et al., 2013, or chemical composition analysis if available. Furthermore, PM2.5 is not the appropriate measurement to separate aerosol conditions to investigate ACI. Please use an appropriate measure of the aerosol physical properties.

F. A discussion of the role of wind direction on the reliability of measurements of cloud properties from the Fog monitor, see discussion on cloud droplet measurements in Leskinen et al., 2009 as well as a detailed description of any corrections performed to the measured parameters and uncertainties associated with sampling methods.

G. Justification of choice of metrics, e.g. CCN at 0,2% supersaturation and others not commonly employed in ACI process studies, e.g.: Nccn(0.2)/Np. Why did you not focus on the droplet activated fraction (Nc/Np)?

H. In light of the new analysis associated with (A-G) a newly revised, clear explanation of cloud processes using the observations. Conclusions should not be presented as fact unless they are fully supported by the observations in the manuscript.

I. A detailed explanation of how cloud top albedo was calculated including any assumptions made and a discussion as to their validity. An assumption is made related to the calculation of cloud liquid water path, e.g. Stephens, 1978 that is not discussed. Is the assumption of 100m cloud depth valid? Furthermore, it appears that this calculation might be inconsistent as cloud top measurements of cloud microphysical properties are not provided.

Given the strong reservations on large elements of the methodology employed for the scientific analysis in the study I recommend that these are addressed in full in the first instance prior to consideration for publication.

Regarding the terminology, language and grammar: This needs improving throughout the manuscript. Sentences found throughout such as: Line 24:25: "the perturbation of particles rapidly scrambled the water of the formed cloud droplets" Do not meet the quality required for scientific publication. In addition, throughout the manuscript some ambiguous and unclear terminology is employed. I strongly recommend that if the major revisions regarding methodology can be addressed that the manuscript text be then carefully revised, probably by a native English speaker.

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