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

Kevin J. Noone
Stockholm University

General Comments

The authors present very interesting measurements made at Mt. Tai in northeast China. They have an interesting set of measurements from an under-represented region. As such, it would be valuable for these data to become available. However, I feel that two factors prevent the manuscript from being published in its current form: 1) Substantial revision of the analysis is needed, particularly with regard to the criteria for sub-dividing the cloud events; 2) pertinent references prior to ca. 2000 are lacking, and would perhaps help augment the analysis.

The grammar and language in the manuscript is understandable, but could be improved.

The data are both interesting and useful. However, I feel the manuscript needs substantial revision before it can be published.

Specific Comments

P2, L10-15 The processes discussed in this paragraph have been investigated for decades, and there is a very rich literature on all the issues raised. The references here are all fairly recent – which is fine – but I feel that the addition of some citations to earlier studies would help shine a light on how rich the literature on these subjects actually is.

P2, L23 While the "first indirect effect" has become fairly accepted jargon in the cloud physics field, still it should be defined here.

P3, L5 Change "size distributions of clouds and aerosols" to "size distribution of cloud droplets and aerosol particles".

P4, entire I feel more detail on data processing is needed. Were inversion routines used to calculate cloud droplet and aerosol particle size distributions and CCN spectra, or were these derived directly from the various instruments?

P5, L6 "claculated" should be "calculated"

P5, L9 I can't find a definition of N_P, which I assume is total aerosol particle number in the size range the SMPS can measure (13.6-763.5nm). Is this correct?

P5, L20-25 The comparisons to cloud conditions at in city fogs, convective and orographic clouds are interesting, but I think comparing to cloud and aerosol measurements at other mountain-top sites would be even better. There are several such sites at which various field campaigns have taken place, with fairly complete aerosol and cloud measurements. These include e.g., Mt. Kleiner Feldberg in Germany, Jungfraujoch in Switzerland, Mt. Åreskutan in Sweden, Puy-de-Dôme in France, Great Dun Fell in the U.K., Mt. Soledad in the US. Some places to start for references to data at these sites are:

Kleiner Feldberg: Journal of Atmospheric Chemistry 19 (1&2), 1994. Special issue on the Kleiner Feldberg Cloud Experiment 1990

Jungfraujoch: Weingartner, E., et al. (2006), *Aerosol-Cloud Interactions in the Lower Free Troposphere as Measured at the High Alpine Research Station Jungfraujoch in Switzerland*.

Åreskutan: Heintzenberg, J., J. A. Ogren, K. J. Noone, and L. Gärdneus (1989), The size distribution of submicrometer particles within and about stratocumulus cloud droplets on Mt. Åreskutan, Sweden, *Atmos. Res.*, *24*, 89-101.

Noone, K. J., J. A. Ogren, and J. Heintzenberg (1990), An examination of clouds at a mountain-top site in Central Sweden: the distribution of solute within cloud droplets, *Atm. Res.*, *25*, 3-15.

Targino, A. C., K. J. Noone, F. Drewnick, J. Schneider, R. Krejci, G. Olivares, S. S. Hings, and S. Borrmann (2007), Microphysical and chemical characteristics of cloud droplet residuals and interstitial particles in continental stratocumulus clouds, *Atmos. Res.*, *86*, 225-240, doi:doi:10.1016/j.atmosres.2007.05.001.

Drewnick, F., J. Schneider, S. S. Hings, N. Hock, K. J. Noone, A. Targino, S. Weimer, and S. Borrmann (2007), Measurement of Ambient, Interstitial and Residual Aerosol Particles on a Mountaintop Site in Central Sweden using an Aerosol Mass Spectrometer and a CVI, *J. Atmos. Chem.*, *56*, 1-20.

Puy-de-Dôme:

Asmi, E., E. Freney, H. Maxime, D. Picard, C. Rose, A. Colomb, and K. Sellegri (2012), Aerosol cloud activation in summer and winter at puy-de-Dôme high altitude site in France, *Atmospheric Chemistry and Physics Discussions*, *12*, doi:10.5194/acpd-12-23039-2012.

Laj, P., R. Dupuy, K. Sellegri, J. Pichon, J. Fournol, L. Cortes, S. Preunkert, and M. Legrand (2001), Experimental studies of aerosol- cloud droplet interactions at the puy de Dome observatory (France), *AGU Spring Meeting Abstracts*.

Great Dun Fell: Choularton, T. W., et al. (1997), The Great Dun Fell Cloud Experiment 1993: An Overview, *Atmos. Environ.*, *31*(16), 2393-2405.

and the other papers in the special issue of Atmospheric Environment

Mt. Soledad: Modini, R. L., et al. (2015), Primary marine aerosol-cloud interactions off the coast of California, *Journal of Geophysical Research: Atmospheres*, *120*(9), 4282-4303, doi:10.1002/2014JD022963.

Schroder, J. C., S. J. Hanna, R. L. Modini, A. L. Corrigan, S. M. Kreidenwies, A. M. Macdonald, K. J. Noone, L. M. Russell, W. R. Leaitch, and A. K. Bertram (2015), Size-resolved observations of refractory black carbon particles in cloud droplets at a marine boundary layer site, *Atmos. Chem. Phys.*, *15*(3), 1367-1383, doi:10.5194/acp-15-1367-2015.

Sanchez, K. J., et al. (2016), Meteorological and aerosol effects on marine cloud microphysical properties, *Journal of Geophysical Research: Atmospheres*, *121*(8), 4142-4161, doi:10.1002/2015JD024595.

There are more than these references, of course. These are the ones that come quickly to mind.

P6, L2-3 I find the discussion here a bit simplistic and even incorrect. LWC often tends to increase linearly with height in a cloud. If entrainment processes are active, the increase of LWC with height could still be linear, but less than the maximum adiabatic rate. Increasing N_c or r_{eff} does not necessarily lead to increases in LWC. You can get an increase in N_c with no increase in LWC by simply having a larger number of smaller droplets. Similarly, r_{eff} can increase at a constant LWC if the droplets became fewer in number but larger in size.

P6, L4 I believe "Hyderaba" should be "Hyderabad".

P6, L13 T_a should be defined before it is used for the first time.

P6, L20-30 The sub-periods into which the various cloud events are divided are not clearly defined. What do "clean 1", "perturbation 2", "dissipation" and the other descriptors mean?

P6, L23 The authors divide liquid water content by cloud droplet number concentration (LWC/N_c) and report a value of 1.9 mg water per droplet. This is clearly erroneous. For a water density of 1 g cm^{-3}, this would give a droplet radius of 0.8mm, which is clearly far too large. This comment holds true for Figure 3(b) as well.

P7, L11-13 The bounding lines referred to here (and shown in Figure 3(d)) appear arbitrary. Is there any physical explanation for these lines? Why does one have a zero intercept and the other an intercept of -200?

P8, L1-13 Once again, references seem to be limited to rather recent publications. There is a wealth of literature about cloud susceptibility starting in the 1990s. I suggest starting with [Platnick, S., P. A. Durkee, K. Nielsen, J. P. Taylor, S. C. Tsay, M. D. King, R. J. Ferek, P. V. Hobbs, and J. W. Rottman (2000), The role of background cloud microphysics in the radiative formation of ship tracks, *J Atmos Sci*, *57*(16), 2607-2624] and the papers that cite this one for more comparisons.

P8, L18 I have a difficult time understanding how soluble organic particles can by hydrophobic.

P8, L28+ The discussion of Figure 5 is confusing, mostly because the figure itself is not clearly labeled. There is a great deal of information in Fig. 5; at a minimum, a clear caption is necessary. I'm afraid I can't follow the arguments presented here, and feel this material needs significant work to be understandable.

P9, L19 Given the amount of temporal variability in LWC, do hourly averages of this quantity have any real meaning?

P9, L26-27 As per my previous comments, I feel that the stages into which the authors divide cloud period 2 are arbitrary. I haven't found any explanation of these stages in terms of quantitative parameters. The physical processes discussed on pages 9-10 are certainly valid ones, and pertain to clouds in general. However, I don't find the division of the cloud events into arbitrary stages to be convincing in terms interpreting the measurements at Mt. Tai in the context of these processes. Unfortunately, I feel that Figure 6 and the discussion around it is unconvincing. There may well be interesting information here, but a clearer rationale for stratification of the data will be necessary before it can be elucidated.

P10, L27 Is there any reason to assume that the cloud thickness is 100m? My own experience measuring clouds from mountaintop sites is that cloud thickness varies quite dramatically, and at most sites is highly sensitive to changes in wind speed and direction.

Figure 2 The rightmost label in panel (e) of Figure 2 $dN/dlogD_c$, not $dN/dlogD_p$

Figure 5 This figure is very colorful, but very difficult to understand. The figure caption needs significantly more detail.