Atmos. Chem. Phys. Discuss.,
https://doi.org/10.5194/acp-2017-1227-RC1, 2018

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Satellite-based estimate of the variability of warm cloud properties associated with aerosol and meteorological conditions" *by* Yuqin Liu et al.

Anonymous Referee #1

Received and published: 2 May 2018

In this work the authors use the diurnal cycle of cloud properties to examine how aerosols might impact cloud development. Concentrating on the region around China, they look at the relationship between aerosol optical depth and MODIS cloud properties over a four year period. Initially, they investigate the instantaneous relationship between aerosol and cloud properties, finding strong relationships between aerosol and cloud properties, similar to previous work. The second half of the paper follows a method from Gryspeerdt et al (2014), which looks at development of cloud properties as a way of accounting for meteorological covariability. They find a strong relationship between aerosol optical depth and the development of cloud properties, attributing this to an aerosol impact.

This paper is within the scope of Atmospheric Chemistry and Physics, but there are a number of points which should be clarified before this paper could be published. In particular, it is not clear how the authors are accounting for or considering the impact of local meteorology. Additionally while this paper shows some interesting results they are not discussed context of previous work, meaning that they may not achieve the impact they otherwise would. As such, I would recommend major revisions.

Major points

The authors make use of a technique previously used to investigate possible links between aerosol and cloud fraction, extending it to look at the development of other cloud properties. A key part of this method involves making sure that the starting state similar as possible for high and low aerosol environments and then investigating the difference between them. If this method works as intended, the mean change in cloud properties over the timestep should be a function only of local meteorology and there should be no difference in the cloud properties between the high and low aerosol populations at the start time. I am therefore unclear what is being shown in section 4.1, where a difference apparently exists. Are the authors following the method of Gryspeerdt et al (2014), or have they created a new method? If the authors are just looking at the relationship between AOD and cloud properties, how have they accounted for the impact of local meteorology (e.g. Quaas et al, ACP, 2010)?

Similarly, it is not clear what section 4.2 is showing. While the title states that it is discussing the 'mean change', it is apparently also investigating the difference between high and low AOD. If this is the case, could it not be merged with section 4.3, which is explicitly about the difference in relation to the aerosol environment? I would expect that the difference in the development between the regions would be a function of local meterology. If 4.2 is intended to be about the mean cloud development, perhaps it could be used to better describe the meteorology of the different regions, helping the interpretation of the regional variation of the results in section 4.3.

While this work has the potential for producing interesting results if the method is properly clarified, the results that are currently within the paper are not set in the context of existing work, which makes them difficult to interpret. The results in section 4.3 and not compared to section 4.1 or previous work, meaning that potentially interesting results are missed. As some examples, P13L14 suggests that there is little change in the CDR development as a function of aerosol - this inability to detect the Twomey effect might mean that this method is not suitable for investigating aerosol cloud interactions, or it could mean that changes in CDR proceed via different pathways and timescales than the CF changes observed in Gryspeerdt et al. (2014). Although the difference in results over land and ocean was one of the key results of Gryspeerdt et al (2014), other result are different - this work finds exactly the opposite dCF response to relative humidity (section 4.4). This would again be an interesting result for discussion that is missed as it is not set in context.

I am not clear of the purpose of choosing the different regions in this work. They are explained in section 2, but very little reference is made to these meteorological differences later in the paper. Other than noting that the aerosol-cloud relationships are different in these regions, there is little discussion of why there is a difference. As variations have previously been noted in the strength of aerosol-cloud relationships, it would be good to include some discussion as to why they are different. This would help this paper build on the previous literature in this area.

Specific comments

P1L39: Twomey 1974/77?

P2L3: a smaller droplet radius does not always result in precipitation suppression, especiallyif the warm rain frequency is already low (e.g. Muelmenstaedt et al., GRL, 2015)

P4L1: Why not use collection 6 data? There is also almost four times as much MODIS daily data available as it being used here. Why has this specific time period been

chosen? A larger data record would improve the statistical significance of this work.

P4L24: Why is aerosol optical depth used? Many previous studies have that it had severe limitations proxy for CCN (e.g. Penner et al, PNAS, 2011)

P5L2: 'representative of typical thermodynamic conditions'- it is not clear what this means

P6L1: Are all parameters considered at the same time? Gryspeerdt et al, also used meteorological parameters normalisation.

P6L2: Normalisation by cloud fraction makes the biggest difference in what?

P6L2: Does this mean this normalisation method is applied throughout this work?

P6L24: As mentioned earlier should the difference between the cloud properties at the start time not be zero?

P7L7: Perhaps also Yuan et al, ACP, 2008 (Increase of cloud droplet size with aerosol optical depth: An observation and modeling study, 10.1029/2007JD008632)

P7L22: Many previous studies have shown links between aerosol and cloud properties over China but it might be good to know why these relationships are different.

Figure 3: What is this sample time series?

P10L4: If the variation of cloud properties depends little on the initial AOD, does that not mean that section 4.3 should show no results? This would be in contrast to previous studies

P13: As there have been several previous studies looking at aerosol and cloud relationships, it would be good to set these results in context of previous work.

P15L7: presumably LTS

P15L12: I read exactly the opposite, it looks like there is a high impact of aerosol with descending air parcels

P15L18: Is this change a very large relative humidity statistically significant or just noise?

P15L23: LTS is almost always positive

P15L25: 27K is a very high value for LTS and does not distinguish much between high and low values

P16L4: Why is the initial cloud fraction included if it's impact is not clear? Can we learn anything from it?

P17L28: This seems like something that could receive more discussion

P17L13: This relationship between initial cloud fraction and the changing cloud fraction is mentioned again with very little explanation as to why.

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