A review of ‘Examination of Aerosol Indirect Effects during Cirrus Cloud evolution’, by

Flor Vanessa Maciel, Minghui Diao, Ryan Patnaude

submitted to Atmospheric Chemistry and Physics

This article reveals cirrus cloud microphysical properties and aerosol indirect effect in five cirrus evolution phases, i.e. clear-sky ice supersaturation region, nucleation, early growth of ice crystals, later growth of ice crystals and sedimentation/sublimation. The five phases were determined based on the method from Diao et al. 2013. Using 12 field campaign measurements, the authors investigated the five cirrus cloud evolution phases characteristics, and aerosol indirect effects in both northern and southern hemispheres. Some interesting results are found, including cirrus water content and ice crystal number concentration are positively correlated with aerosol number concentration – aerosol indirect effects are different for different size distributions of aerosol particles at different cirrus evolution phases, which is stronger for larger aerosol particles at nucleation phase, while it is stronger for later growth phase for smaller aerosols. The authors also indicate that aerosol indirect effect is stronger in southern hemisphere than in northern hemisphere. By comparing ice cloud microphysical properties and aerosol indirect effect with the simulations from NCAR CAM6, the authors found lower IWC and number concentrations compared with observations, and they hypothesized that lower frequencies of ice supersaturation and lower vertical velocity standard deviation in the early/later growth phases contribute to the underestimation of IWC and number concentration.

Overall, this paper is comprehensive. It provides some new thoughts of aerosol-cloud interaction from the Lagrangian view and possible solutions to improve model simulations. This paper deserves publication after some minor revisions.

My main comments are as follow:

1. In section 2, it is a little bit messy in describing the in-situ observations. I would suggest using a table to summarize the variables (e.g. IWC, Di, Ni, w, RHi, etc.), the instruments that the variables are from, references, and the variable uncertainties.
2. Also in section 2, the methods for data analysis are sometimes confusing. It is adopted from Diao et al. (2013), but it seems different in defining the ‘consecutive sample’. Sometimes, the analysis is for pixel level. Making the definition clear is important to understand the results.
3. For aerosol indirect effect analysis, although the authors mentioned that the correlation or trend is significant, I do not see any significant tests for the regression. I would strongly suggest significant tests for the regression. Particularly, if the regression is insignificant, how to explain the results.
4. In Figures 8-11, Di varying with Na were shown, but there are not explanations for the results. The difference between Di vs Na100 and Di vs Na500 is obvious. Please describe these results in the text.

Specific comments:

Page 3, line 85-86: ‘We applied the method of Diao et al. (2013) to derive various evolution phases of cirrus clouds, which enables a detailed examination of aerosol indirect effect…’. I suggest describing

Diao et al. (2013) in more detail about their method of separating the five cirrus cloud evolution phases. Then in section 2, focused on your methods as a follow-up of Diao et al. (2013).

Page 3, Line 90: please specify 'Na'.

Page 4, Line 117 & 119: 'this merged observational dataset','For the merged datasets', what datasets?

Page 4, Line 119: 'For the merged datasets, ice supersaturated regions (ISSR) and ice crystal regions (ICR) were identified using values of RHi and Ni, respectively.' How the ISSR and ICR defined, in pixel-level, cloud-element level or in the same way as Diao et al. (2013)?

Page 6, Lines 170- 177: It is confusing about how to calculate the probability. In line 171, it is mentioned that ' the result shows that a cloud segment has the highest probability ….', which indicates the results are for cloud segments. But in Line 173, it mentions that the method is counting points of observations, which is for pixel levels. Please clarify your methods in the method section.

Page 6, Line 186: 'NM and NT have lower frequencies of clear-sky ISSRs compared with their counterparts in the SH', NM and SM show large difference in the frequency, but NT and ST show very close frequency of phase 1. It is better to explain midlatitude and tropical regions separately.

Page 7, Line 189: frequency of Phase 1 is about 0.2 not 0.35.

Page 7, Line 196: 'reduces' → reduce

Page 7, Line 200: 'interestingly, the highest RHi values are mostly seen in phase2, …', this is a good point. The result here also agrees with Diao et al, 2013.

Figure 5: fig. k is confusing. Is it fig. l/ fig. f? Please describe it clearly.

Page 8, Line 227: 'compared with in-situ observations', in-situ formed cirrus clouds.

Page 8, Line 230-234: 'This is consistent with the fact that ATTREX mostly sampled the western Pacific Ocean region', This is not a good explanation for the particle number concentration between NASA DC3 and ATTREX.

Page 9, Lines 274-278: it is confusing to me how temperature affects the variation among the phases.

Page 9, Line 282: 'by subtracting the average values of the corresponding 1-degree temperature bin from the individual bin.', it is not clear how to deal with the temperature here.

Figures 14&15: please add colorbars.

Page 11, Line 343: please clarify what w_{sub} represents.

Page 11, Line 348: how the σ_w impact ice formation?

Line 355: 'RHi distributions with respect to temperature', do you show that in the paper?