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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Influence of convection and aerosol pollution on ice cloud particle effective radius" *by* J. H. Jiang et al.

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This study examines and parameterizes the influence of atmospheric convection and aerosol optical thickness (AOT) on the effective radius of ice cloud crystals (Re) using 4 years of satellite data. The satellite retrieved products used in the study include AOT and Re from MODIS, and a convection (CONV) index derived from ice water content (IWC) at 215hPa from the MLS. The main body of the study is to parameterize the dependence of Re on AOT and CONV over different parts of the world in attempt to explain the differences of the parameters that denote the strength of the influence of AOT and CONV on Re under various meteorological and aerosol conditions.

There have been ample studies concerning the effects of aerosol and convection on cloud particle size. The uniqueness of this study lies in 1) the analyses of wealth of satellite data over a very large spatial domain and long period; 2) the effort of using a

simple scheme to describe a very complex problem; 3) understanding the dependence in terms of aerosol and meteorology. Such attempts are useful towards "establish a framework for parameterization of aerosol effect on Re in climate models". However, the parametrization as proposed may not be suitable for application in any climate models due to the following major concerns:

1. The concept of the convection index (CONV) derived from IWC at a particular level is confusing. The only rational given is the correlation between the CONV and model derived OLR. First of all, why not use the CERES measured OLR instead of modeled one? Second, the instantaneous relationship as shown Fig 1 (left) is not good to support the argument. The good correlation found in the mean quantities may not be construed as a proof, as the relation between the mean quantities can be driven by large-scale factors (e.g. seasonal changes, spatial variation, etc.), while the influence of aerosol on cloud is instantaneous. Third, why was the IWC at a particular pressure level chosen? One could use IWC at a different level, or cloud top height, or vertical velocity, etc. Forth, it is necessary to explain how the quantity given in Eq. (1) is used as a proxy of convection? For the mean IWC in the denominator, it is not specified the spatial domain over which the mean quantity is computed, let alone understanding its physical meaning.

2. No physical consideration is given to support the choice of the specific format of the equations. Given enough number of freedom, good fitting can be achieved by different functions. As such, the good agreement between observed and fitted values are not surprising. The assumption that the effects of AOT and CONV on Re are independent ("decoupled" as the authors put it) are contradictory to the later discussion. Yet, it has been widely recognized that the effect of AOT on cloud particle size depends highly on the strength of convection (e.g. Tao et al. 2007, JGR; Lebsock et al. 2008, JGR).

3. It is somewhat arbitrary to simply divide the world into a few geo-locations. While it is true that different regions are subject to the impact of different types of aerosols, within many of the big domains exist many different types of aerosols, including mixed ones.

In other word, the model parameters would have wide ranges of values had they been derived over much smaller sub-domains dictated by any a particular type of aerosols. As the objective of the study is related to climate model applications, such a rough geo-differentiation would not be adopted in any GCMs. It'd make a lot more sense to discriminate according to aerosol types and meteorological regimes. Even though it is unfeasible to classify the entire world this way, it'd be more valuable to choose smaller domains with more uniform aerosols and meteorology in order to better understand the variability of their effects.

Specific comments: 1. Several papers from the same group are cited with a similar research theme. It is thus necessary to explain the distinction of this study from previous ones. 2. There are many means of denoting convection strengths used in observation and modeling communities. Give a justification for the selection of the IWC-based on as defined in this study. 3. Reference Macfarqure should be MacFarqure 4. The MODIS Re retrieval is sensitive to the very top of cloud, not about 0.1-0.2 optical depth. Supposing cloud optical depth is 100, the current statement would mean the peak at 10-20, which is totally incorrect. 5. Elaborate the GEOS-5, in particular how OLR is obtained. 6. Increase in the Re with AOT is also found from satellite data (Yuan et al. 2007, JGR) 7. On page 9, confusing statement "For CONV>1, it approaches the maximum of 1". 8. The study of Menon et al. cannot be used to support the hypothesis of exceptionally strong absorbing aerosols in East Asia, as the study is nothing but sensitivity tests which assumed a very low single scattering albedo (0.85). Several later observation-based studies (e.g. Lee et al. 2007, JGR) found that the mean value is around 0.9 in the region, which implies strong absorbing but not exceptionally stronger than ther populated areas. 9. Fig. 3, what are the dashed lines? 10. Typo in the figure caption of Fig.2 (not 3). 11. Missing the article title for the last reference Zhang et al.

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