Response to Reviewer #2:

General comments:

The study carried out by Xie et al. implemented a new relative dispersion treatment in the CAM5 cloud parameterization, accounted for its effect of on autoconversion process, and assessed its impact on the climate and aerosol indirect forcing. While this study is suitable for ACP, I have some concerns for the authors to consider when they revise the manuscript.

Response: Thank the Reviewer very much for the comments.

Specific comments:

1. The title: I am not sure if the new relative dispersion treatment constitutes a "New cloud parameterization". I am also not convinced that this study has done enough to be categorized as a "model evaluation" paper as shown in the title since only global means, seasonal means, and zonal means are compared with standardized observational data products. I think this study is a model sensitivity study and the title should reflect that.

Thanks for the suggestion. We have changed the title and the new title is "*Sensitivity study of cloud parameterizations with relative dispersion in CAM5.1: model evaluation and impacts on aerosol indirect effects.*" Furthermore, we compared key statistical measures based on global spatial distribution including spatial pattern correlation and the root mean squared error with observed data products in Table 2 (SWCF), Table 3 (LWCF), and Table 4 (precipitation rate), in addition to comparing the global means, seasonal means, and zonal means.

2. The results show that the AIF reduces by only 0.1-0.2W/m2 in CAM5, and this reduction is very small. This is much smaller than the previous study Rotstayn and Liu (2005), which implemented the same relative dispersion representation in the CSIRO Mark3 GCM. It will be interesting to discuss the difference between these two studies.

Thanks for pointing this out. The reduction of AIF in our model is much smaller than that from Rotstayn and Liu (2005). We think that a main reason is that the reference cases are different. In Rotstayn and Liu (2005), the reference case is performed with the autoconversion parameterization (Baker, 1993; Boucher et al., 1995) given below,

$$P = E_c \pi \kappa_1 \left(\frac{3}{4\pi\rho_l}\right)^{4/3} N^{-1/3} L^{7/3} H(R_3 - R_{3c}),$$

In our case, the reference autoconversion parameterization is

$$P_N = 1.1 \times 10^{10} \frac{\Gamma(\varepsilon^{-2}, x_{cq})\Gamma(\varepsilon^{-2}+6, x_{cq})}{\Gamma^2(\varepsilon^{-2}+3)} L_c{}^2,$$
$$P_L = 1.1 \times 10^{10} \frac{\Gamma(\varepsilon^{-2})\Gamma(\varepsilon^{-2}+3, x_{cq})\Gamma(\varepsilon^{-2}+6, x_{cq})}{\Gamma^3(\varepsilon^{-2}+3)} N_c{}^{-1} L_c{}^3,$$

with fixed dispersion of 0.4. According to the Reviewer's suggestions, we have added some discussions in revision: *"It is worth noting that the reduction of AIF induced by dispersion effect in this study is much smaller than that (approximately −0.5 W m^{-2} for global means) reported by Rotstayn and Liu (2005). This difference lies likely in the reference autocnversion parameterations. In this study, Eq. (3) with fixed dispersion of 0.4 is used whereas Rotstayn and Liu (2005) used a different one give in* $P_L = E_c \pi \kappa_1 (\frac{3}{4\pi\rho_l}) N^{-1/3} L^{7/3} H(R_3 - R_{3c})$. *"*

3. The treatment of dispersion effect on cloud droplet effective radius in the default MG microphysics scheme in CAM5 is based on Morrison and Grabowski (2007) and the new treatment used in this study is based on an earlier study Rotstayn and Liu (2003). I think it might be interesting to discuss why these two formulae are different (e.g., are they based on observations of different cloud regimes?) and provide a justification of your choice of the scheme.

Thanks for pointing this out. We have added some discussions in revision: *"The Morrison-Grabowski relationship is based on small number of measurements (ε=0.33 for maritime air masses and ε=0.43 for continental air masses) reported in Martin et al., 1994, while the Rotstayn and Liu relationship is derived from more measurements described by Liu and Daum (2002). Also, the Rotstayn-Liu relationship assumes the dispersion levels off at approximately 800 cm^{-3} while the linear Morrison-Grabowski* *relationship has no such limit.”*

4. Regarding the reference, I think the authors should try to cite other relevant studies on this subject in addition to their own previous studies, especially when the authors use strong wordings such as “it is well established: : :”.

Thank you for your good suggestions about adding other relevant studies. In the paragraph, we have added some important references. Hence, the sentence has been modified as *“It is well established that effective radius (Martin et al; 1994; Liu and Daum, 2002) and autoconversion rate (Liu and Daum, 2004; Liu et al., 2007; Xie and Liu, 2009; Li et al., 2008; Chuang et al. 2012; Wang et al., 2013; Michibata and Takemura, 2015) are both related to the relative dispersion of cloud droplet size distribution ε (which is defined as the ratio of the standard deviation to the mean value of droplet size distribution) in addition to droplet number concentration and cloud liquid water content.”*

References

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