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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Quantifying the aerosol effect on droplet size distribution at cloud-top" *by* Lianet Hernández Pardo et al.

Anonymous Referee #2

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Review of acp-2018-1087: "Quantifying the aerosols' effects on droplets size distributions at cloud-top", by LH Pardo et al.

Summary: This is a theoretical study of sensitivities of cloud droplet size distributions to initial aerosol loading. There are two unique aspects in this study: first, the authors limit their discussions on cloud top properties only; second, the sensitivity tests are thoroughly spaced over aerosol characteristics, including total number, median size, standard deviation of a log-normal distribution, and the hygroscopicity. This is a clearly structured manuscript with adequate figures and literature overview. The conclusions agree with various previous studies using different modeling tools and/or with different parameter choices. The main limitation of the current study is the use of a highly simplified kinematic model, albeit with detailed microphysical representations. I understand

that there are tradeoffs to be made in order to carry out a large number of sensitivity tests. However, there should be a much more detailed discussions listing various limitations, and their associated errors, in both the kinematic framework and in handling aerosol activation processes. In addition, I think the scientific quality of the current manuscript can be improved with additional simulations and analyses. I will detail my suggestions in the follow section. There could be significant revisions if the authors decided to carry out some of the additional sensitivity studies.

Main concerns: 1. There are significant limitations in using a kinematic model. In additional, some key aerosol activations processes in the model that have been simplified. The authors skimped some of these limitations here and there in the manuscript. However, they have missed the most important aspect of the limitation discussions, that is, how these simplifications might affect their main conclusions. This is essential if the conclusions were to be useful for understanding aerosol-cloud interactions in the real world. I would suggest that the authors add a discussion section before the conclusion, to carry out some detailed, in-depth discussions. The following is the list of my suggested topics. Some of them are more obvious than others. Some of them are totally missing in the manuscript and need careful considerations. a). Will the conclusions change if a full dynamic model were used? b). If the initial sounding and/or vertical velocity profile changed, will it change the conclusions? c). A small cumulus with cloud top below 6km seem to be the closest real world resemblance of the kinematic model setup. A key piece that is missing is the entrainment of environment air, together with additional aerosols, into such a small cumulus. This is not discussed at all in the manuscript. The entrainment could come from the cloud bottom, side of the cloud, and most challenging, from the cloud top. Since the focus of this study is the cloud top properties, the variations in the cloud top entrainment along might change the existing conclusions. I think that the entrainment can be added fairly easily in the kinematic framework, with pre-determined entrainment rates and vertical variations. I suggest that the authors repeat their calculations with various entrainment rates, repeat the analysis, and see if the conclusions remain the same. I am particularly interested in how the cloud top properties change if entrainment from the top is added. I believe these additional simulations will improve the scientific quality of this study significantly. d). Prognostic aerosol activation is another significant limitation of the current study. On P4, L24, the authors stated that they use "a 0.25 factor that attempts to accommodate for the fact that not all CCN will grow to the size of the first droplet bin." Please discuss in details how the factor of 0.25 was chosen, how this factor could affect aerosol activation and cloud droplet spectra, and how it will affect the sensitivities. e). Since aerosols are represented prognostically, there is no sink term for them in the microphysical calculations. In reality, aerosols are removed in clouds through both activation and wash out. Please discuss how this simplification will affect the conclusions. f). Aerosol sizes also grow with increasing supersaturation, and consume certain amount of water vapor supply. This is not considered in the model. How important is this process?

2. There are significant vertical variations in simulated cloud properties, as shown in Fig. 2. It will be beneficial to conduct the same sensitivity calculations in Fig. 3 for vertically averaged cloud properties, and compare them with the cloud top properties. The results can also be compared with Cecchini et al (2017).

Minor points: 1. P2, L23: "Must of the previous studies" should be "Most…"; 2. P3, L28: "1 s" should be "1s", so is "1200 s"; 3. P8, L5: "Thus the width of the aerosol spectrum can be more important for droplet activation than…". I don't agree with this statement. Calculations in Fig. 6 have different units. One cannot compare numbers with different units. 4. Fig. 3: What is the meaning of individual point with the same color? Are they averages over certain time period, or across certain height levels, or something else? 5. It will be nice if the zero lines are labeled in Figs. 4-7, so the positives and negatives can be clearly separated.

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