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Interactive comment on "Impact of aerosols on precipitation over the Maritime Continent simulated by a convection-permitting model" *by* M. E. E. Hassim et al.

Anonymous Referee #2

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General comment:

This study examines aerosol effects on tropical convection and the associated rainfall over the region centered at New Guinea using numerical simulations with the WRF model. The results show a small impact of differing cloud droplet number concentration on convection, and the impact is found to be of opposite sign to what is referred to as "convective invigoration". This study is of interest to the community in which the notion of convective invigoration is controversial. I would recommend this paper to be published in Atmos. Chem. Phys. after the authors address my concerns described below.

Major comments:

Printer-friendly version

Discussion paper

My major concern is that the authors' microphysical analysis is not enough to identify the mechanism responsible for the suppression of rainfall from convective clouds in polluted conditions. The authors invoke the classical notion of the aerosol indirect effect on warm clouds, which accounts for reduced particle size and less efficient collision-coalescence process, but it is not clear how such a microphysical modification in warm clouds influences the subsequent ice processes (including riming) that lead to the graupel formation. The authors should add comparisons of particle size and the process rate of conversion among water species (e.g. auto-conversion and riming) between pristine and polluted conditions for stratiform and convective clouds. Such an analysis would clearly demonstrate that (i) the polluted condition suppresses the warm rain process through the classical second indirect effect mechanism, (ii) the ice crystals (cloud ice in this simulation) produced from the smaller-sized cloud droplets tend to have smaller particle sizes, and (iii) such smaller ice particles have less efficiency of riming that produces graupel. If the authors add these analyses, then their findings would be substantially strengthened.

Minor points:

1. Summary of model configuration is necessary (Section 2.1). The authors state that the physics packages were selected as in Hassim et al. (2016) (page 5, line 5-7). The authors should describe the model configuration in this paper as well in a concise manner. Would it be possible to include a table that summarizes the physics schemes/packages that are employed in this study?

2. More clarified description of microphysics scheme is necessary (Section 2.2). The authors mention that cloud ice and rain are double moment – how about other species? Are they all treated as single moment (i.e. predicting only mixing ratios)?

3. There are some editorial errors. Page 5, line 28: power law the links -> power law that links Page 13, line 29: than -> then

4. The figures should be better labeled. Figure 2: Please put "PRIS" and "POLL" to

the left of the figures. Figure 7: Please put “Convective” and “Stratiform” to the left of the figures and “Land” and “Ocean” above the figures. Figures 13 and 14: Please put “Convective” and “Stratiform” to the left of the figures and “Qcloud”, “Qrain”, “QNrain”, “Qice”, “Qsnow” and “Qgraup” above the figures.

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