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Interactive comment on "Effects of Thermodynamics, Dynamics and Aerosols on Cirrus Clouds Based on In Situ Observations and NCAR CAM6 Model" *by* Ryan Patnaude et al.

Anonymous Referee #2

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Review of "Effects of Thermodynamics, Dynamic and Aerosols on Cirrus Clouds Based on In Situ Observations and NCAR CAM6"

Patnadue et al.

This paper represents a nice study of microphysical observations versus climate modeling. The paper is well written and clear. I shear some of the same concerns as the other reviewer "Andrew Gettelman" but I think this paper could be published after all comments are addressed. Below are my main comments and concerns.

Line 65: Is the reason for increased crystal size with increased aerosols due to the competition between homogeneous and heterogeneous ice nucleation and that you

actually have fewer ice crystal concentration in the polluted environment?

Line 73: How does the increase in aerosol number concentration help increase the size of ice crystals?

Line 86: I suggest to rephrase: "….and found a decrease in Ni with increasing aerosol concentration due to the …..."

Line 131-133: Are there differences between ocean and land as well?

Line 148: If you used the microphysical version including graupel and hail (MG3) you should cite Gettelman et al 2019, "The impact of rimed ice on Global and Regional Climate" in JAMES

Line 154. Do the nudged runs also use prescribed sea-surface temperature?

Line 166: If you disregard the smallest sizes in the observations to define in-cloud conditions, but account for all sizes in the model when defining in-cloud conditions how often do you miss observed in-cloud conditions compared to modeled in-cloud conditions?

Line 167: Why is the additional constraint on cloud fraction not used for the CAM-nudged

Line 181: When mentioning figure 4 I suggest adding a sentence stating that 3 top rows are observations and 3 lower rows are model. Perhaps you can add a label in the figure as well.

Line 187: Mention that the CAM6-nudg data is the 3 bottom rows

Line 207: Did you include the pre-existing ice option by Shi et al in the simulations? Perhaps you should mention that here.

Line 210: I suggest using same color scale between figure 6 and figure 7

Line 211: What is the cause of the systematic "wave" showing up in the tropical RHi in

Figure 7?

Line 212: It is difficult to see the difference between the solid line and the dashed line in figures 6 and 7.

Line 224: Figure 8 and 9 (and other figures with variance of w). Since this value is never negative, I suggest starting the scale at zero.

Line 224. The 200 seconds of data corresponding to 46 km, is that true for all flights?

Line 252: Figure 11: Are the number of samples normalized for the colorbar? I also suggest label the top row as observations, middle as CAM6-nudg and bottom as CAM6-free data.

Line 273: Figure 12. I do not see a large positive correlation between Di and ïAşw. I would suggest state: ". . ...which differs from the slight observed positive. . ..."

Figure 13. Label the two left columns as Na500 and the two right columns as Na100. In the caption, figures q-t are not described.

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Interactive comment on Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2020-497, 2020.