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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.

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

Review by Andrew Gettelman, NCAR

In general this is a very well written cutting edge analysis of a comparison between observations of upper tropospheric ice from aircraft and an advanced large scale global climate model. However, I have some questions on how the analysis was conducted and the sampling of the models and observations. The work is very cutting edge in the detail of the comparisons, but probably needs a few more pieces of description, and more information perhaps to back up the analysis. I think the manuscript needs some substantial revisions before it will be acceptable for publication in Atmospheric Chemistry and Physics.

1. In general, I am concerned that the comparison between the model and observations is not sampling them the same way. It does not seem as if the observations are averaged over a model grid box length before actually reporting values. This should be clarified.

2. But fundamentally, when this is done, you get a distribution of individual observations which make up a size distribution. In CAM6, it reports a single mass and number for clouds in a grid box. But this represents a size distribution itself. These size distributions can be compared: it may be that the mass and number is distributed differently than the observations. Gettelman et al 2020, in Press, JGR illustrates this method. You can plot the size distributions from the model by reconstructing the distributions from the model equations.

Gettelman, A, Charles G. Bardeen, C. S. McCluskey, and E. Jarvinen. "Simulating Observations of Southern Ocean Clouds and Implications for Climate." J. Geophys. Res., 2020. https://doi.org/10.1029/2020JD032619.

3. This leads to my next point. The lack of observations below 62 microns diameter means there may be a significant amount of missing ice in the observations. Have you used the distribution functions to truncate the model size, number and mass to reflect this? If not, it's going to make the comparison even worse. If so, there may be an issue just with the size distributions themselves (see earlier point).

4. I find it hard to imagine that a climate model can produce 30x less ice than observed and still produce reasonable radiative fluxes, particularly for the Outgoing Longwave Radiation. Maybe I am wrong. It's even worse if the model has small ice that is not seen in the observations, but it's explainable if you have removed all the small crystals

already. But since you are actually deriving IWC from a distribution of particles, it seems possible that the assumptions you are making may be very wrong (i.e. the number/size v. IWC relationship). What is the uncertainty here? See specific comment below.

5. Also, there is very little commentary on whether the nudged or free running reproduces observations better. At least not in the conclusions. I think there were some vague comments about the simulations being 'similar'. Is there no appreciable difference?

Specific Comments:

Page 2, L43: there are IWC observations and compilations that go back decades. Much of it by Heymsfield. Please cite some earlier work.

Page 2, L51: Fu and collaborators and Mitchell and collaborators have done some work with CALIPSO that might be relevant here, particularly for occurrence and particle size. IWC from CALIPSO is harder.

Page 4, L107: CAM6-nudg? Typo? Seems like you are using this throughout, but I don't see it defined.

Page 4, L110: Are you going to test the impact of this assumption? Does CAM6 have a similar or close relationship between IWC and number/size?

Page 4, L113: since CAM6 has 2 moments and a distribution, wouldn't it be wise to show the size distribution from observations (not just the mean diameter) and from CAM to see if there are biases in the shape or in parts of the size distribution?

Page 4, L119: 1hz is only maybe 200m horizontal. How can that be compared with a global model at 100km resolution?

Page 4, L121: does CAM6 also use Murphy and Koop? Could that be an issue (probably not).

Page 4, L123: whereas âĂŤ> where

Page 4, L128: you might note that some latitudes have some very different regimes.

Page 5, L138: I'm not sure I found where this size cutoff is noted. Is it wise to proceed with only half the size distribution? Seems like that would strongly affect how forceful you can make the conclusions. It also means the model needs to be sampled carefully.

Page 5, L140: so I assume this is then a sampling bias to your observational data set?

Page 5, L150: it would be also worth noting the most ice relevant adjustments in CAM6: the use of hoose et al mixed phase ice nucleation, and shi ET al modifications for preexisiting ice.

Page 6, L169: this is a pretty substantial limitation and should be noted much earlier in the text and even have caveats in the abstract. Showing model size distributions I think is critical here. Are you calculating simulated IWC without the small particles? Would that skew the results?

Page 6, L172: as noted above I think the method here is critical. Please describe it. I would feel more comfortable if you show he size distribution by deriving it from the mu and lambda of the gamma distributions, to understand the truncation issue.

Page 6, L177: I suggest that this is a major limitation of correlating Na500 with INP at the temperatures you are working with: INP activation is a strong function of temperature.

Page 6, L187: do you want to comment why here? SH is oceanic, NH is contintential.

Page 6, L190: tropical regions with colder temps might have more small crystals. Would this affect the CAM v. OBS results? I'm concerned you have not filtered the cam results by truncating size distributions.

Page 6, L194: I find the fact that you can get the OLR right with 10x less ice a little bit strange. Does ice mass not mater as all? Or does the di change compensate? Or is the underestimation a product of the Observations really missing a lot of small ice? These don't seem to work the same way. So I am concerned that you are comparing 1hz IWC v. 100km IWC and this is producing anomalous results.

Page 7, L208: the more interesting comparison to me with Righi ET al 2020 would be how they did their comparisons between model and observations, and what sensitivity and size range did their data have? Is it the same or different than here?

Page 7, L215: is the RHI data averaged over similar ranges to the cam observations? It would seem this would be required for a reasonable comparison.

Page 7, L225: it's half the scale of the CAM simulations. Also, note that CAM has a wsub minimimum value of a few cm/s, and in upper trop clear sky it's probably at that limit.

One complication is that wsub comes from TKE as you note, which comes from the turbulence scheme. High wsub indicates convection and turbulence would be active, so the pathway for freezing may be very different, as active convection would create liquid that would either be homogenously frozen in the microphysics or frozen with specified size in the macro physics. It would not run through the activation code. I.e. high wsub might just indicate convective outflow. Can you check this?

Page 8, L255: decrease

Page 9, L262: is rhmini set to 80% in the simulations? How is ice formed? With a RH threshold? I am not sure CAM 6 has such a closure. Please state the value. The Gettelman et al 2010 reference is for CAM5.

Page 9, L266: have you shown where in parameter space (RHI, Temp, Di, Ni) the IWC is most biased in the observations? I think it would be great to summarize this in the text. Maybe this comes later?

Page 9, L269: this gets back to sampling. And remember the model represents a distribution, which you should plot I think. There is variability in a single value in the model. It’s not one value.

Page 9, L278: see above. The ice formation mechanism in CAM might not be what you think in the presence of convection at cirrus temps.

Page 9, L281: I do not think using cloud fraction here is wise. Cloud fraction is a function of scale as well as the detection threshold: if the OBS don’t see any Di < 65um, that will skew things itself relative to CAM. Maybe you should use something easier to make consistent between model and OBS. I’m not sure what that is, maybe Ni since it is an in cloud only quantity.

Also, can you sample aerosols in cloud in the Obs? If not, then are you filtering that out of the model?

Page 10, L300: it’s not clear to me there was much shown with the CAM6 free running simulations. I assume you will summarize Any differences later in this section?

Page 10, L304: since zonal locations in each latitude band are narrow, are you sure this is general and not just a land-sea contrast? Is it anthropogenic aerosols or just land v. ocean?

Page 11, L325: could the model just be producing smaller crystals that are not seen in the observations when Na100 is large?

Page 11, L330: this statement is a bit to grandiose: it’s not really comprehensive and there are a limited set of factors.

Page 11, L331: some more summary of what the results actually said here is warranted. What did you discover about geographical locations?

Also, I’m not sure zonal averages are that helpful if the mix regimes, as noted earlier.

Page 11, L332: I’m still not sure the comparisons and elimination from the model of small ice are done correctly.

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Discussion paper

Page 25, L592: I would recommend collapsing this to put the observations (maybe as a shaded region) on the same plot as the simulations. It makes comparisons easier and results in fewer figure panels.

Page 28, L610: Figures 6 and 7: maybe you could normalize these to get them on the same scale. What is going on in CAM with regular frequency peaks in temperature?

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