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Comment on acp-2021-82

Anonymous Referee #2

Referee comment on "Preconditioning of overcast-to-broken cloud transitions by riming in marine cold air outbreaks" by Florian Tornow et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2021-82-RC2, 2021

I lost my comments before submitting the preview. When I hit the preview, they were not there anymore. I did not copy / paste this before hitting the preview, so it is lost. This is a quick, shorter retype, in a different state of mind of course, so apologies for the brevity. If I get to review this again, I will work in a separate app, cut/paste into this form, and avoid this data loss, so that review will be better. I am asking the Editor to warn other reviewers about this pitfall.

Tornow et al. investigate the impacts of riming on the transition from overcast to broken/open cloud fields during a Cold Air Outbreak using Lagrangian LES simulations. With simulations that have no ice, they demonstrate the importance of precipitation and loss of activated aerosol for the transition from overcast to broken clouds. Using simulations that include ice nuclei, they show that riming can lead to an acceleration of this transition through three different processes: (1) Reduction of cloud liquid water, (2) early consumption of cloud condensation nuclei, and (3) early and light precipitation cooling and moistening below cloud. The authors refer to this as *preconditioning by riming*.

The findings of this study are interesting and further the understanding of the cloud transition in cold air outbreaks. The writing of the manuscript is very concise and clear and the findings are displayed in well-chosen figures that are easy to understand. The authors account for uncertainties by varying various parameters. I’m really interested to which extend the described phenomena can be observed in the future because the accelerated transition has important implications for cloud-climate feedbacks. Overall, I have some general comments, however, I would not consider these major comments and would suggest this submission for publication after minor revisions.

General Comments:

- Lines 144 – 145: As mentioned later the selection of the 75 % cloud cover threshold for a broken cloud field is somewhat arbitrary. It might make sense to show a MODIS image and indicate what 75 % cloud cover looks like in that image. This might help to justify the selection of this threshold or might also lead to the selection of a different threshold. I would think that 75 % cloud cover might be a little too high for a broken cloud field in Cold Air Outbreaks.
- This study argues that the accelerated transition and ice-mediated reduction in albedo may have important implications for cloud-climate feedbacks, i.e. a negative feedback. This remains speculative and needs to be borne out by other modelling and observational studies. In fact, Fig. 1a shows evidence to the contrary. The young (short-fetch) cloud albedo is higher to the north, and much lower south of Cape Hatteras. Helical roll circulations probably are omnipresent along the coast in the convective BL, amassing small convective cells, but further north the ice crystals near cloud top bridge the streets, whereas south of Hatteras, in the absence of ice, the cloud edge is defined by water droplets, which remains closer to the parent updrafts, less likley to bridge the cloud street subsidence regions, hence lower albedo.
- In the discussion section I would like to see some more comparison with observations. Is Abel et al. (2017) (mature marine post-frontal clouds) really the best choice here if it looks at a different location for CAOs? This study looks at a step-change environment, rapid air mass transformation. Very different in terms of aerosol supply and surface flux history, compared to Abel et al. The authors should probably at least add some more quantitative values from Abel et al. that can be compared and contrasted. Moreover, is it not problematic that the satellite imagery suggests that the "overcast state was sustained hours longer" than in the simulations when the maximum difference between all the simulations is only ~1.5 hours (Fig. 8h)? See comment 1 as well.

Minor Comments:

- Line 23: "capped by strong subsidence"
- Section 2.1: I think it would be good to add some more description of how this specific CAO event was chosen. What observations (if any) are available for this CAO event?
- Line 147: The ensemble members are not mentioned until this point. The authors might want to add some description of the 3 ensemble members, and how and why they were chosen.
- Section 2.2: In my opinion, the authors could add a table summarizing the setup of their simulation (which schemes are used/horizontal and vertical grid, etc.), to make it easier for the reader to see the whole setup at one glance.
- Line 114: Do the authors potentially mean 230K? 130K seems excessively low.
- Does Figure 3 show the statistics of the whole domain or only where clouds are present?
- Please be consistent with the naming of "u-phys term" in Figure 6 and "-phys loss" in Figure 8. Also add a legend for the dot dashed lines in Fig. 6a.
- Overall, I like the content of all the figures and how it is displayed. However, I would improve some minor things in the figures. Here are my suggestions: in Fig. 3 and 6 I would put the legends outside the plot and make it larger like it is in Fig. 3. In the figures which have a colorbar (Fig. 4,5,7) I would improve the display of the colorbar, maybe put a black box around them and color the ticks in black instead of white. In Fig. 5 some of the plots have data going outside the range which should be corrected.