The study titled “**Preconditioning of overcast-to-broken cloud transitions by riming in marine cold air outbreaks**” by Tornow et al. investigates the impact of frozen hydrometeors on an overcast-to-broken cloud transition during a marine cold air outbreak (CAO). The study makes use of a series of LES, which were set up following the ACTIVATE campaign in the NW Atlantic. The authors find that the formation of precipitation is necessary in order to simulate a cloud deck breakup, which is further accelerated if the ice nucleating particle concentration is increased. In this case, riming-related processes can trigger this accelerated breakup and precondition the cloud deck to transition from an overcast-to-broken cloud field.

The manuscript is well-written and contains very interesting findings which are presented in comprehensive figures and are described very clearly. The study adds to our current understanding of mixed-phase cloud (MPC) dynamics in CAOs and highlights the importance of microphysical processes, such as riming, for cloud field transitions and hence cloud albedo. Due to remaining lacks in our current MPC understanding and especially the importance of cloud field transitions for regional climate, I encourage publication in ACP. However, I have a few points that should be addressed prior to final publication.

General comment

1. In my opinion, the study would benefit from an analysis of the overall riming rates in the different simulations. The preconditioning by riming is an essential new finding of the manuscript, which could be stronger highlighted and supported. In Figure 6, the contribution of riming to aerosol concentrations is shown, but apart from that there is no graphical evidence of riming in the simulations. Thus, if available, riming rates and/or precipitation divided up into ice, snow and graupel (which would in turn allow an assessment of riming strength if the snow content is high) would help to make the main statement of the manuscript clearer and support several sections of the manuscript which mention e.g. “precipitation of riming-grown snow particles” (line 360).

Specific comments

Abstract

Line 11: I assume you mean “Greater boundary layer aerosol concentrations available for CCN activation” or “CCN concentrations” (i.e. in contrast to INP concentrations)?

Same line: “low-aerosol concentration” similar comment as above, I think it would be clearer to clearly distinguish between aerosols available for CCN activation and INPs.

Line 12 ff: The statement regarding impacts and the associated negative cloud-climate feedbacks seems slightly out of place here to me, as any affect of the cloud transition on climate parameters is not mentioned before. If you mention the climate effect of the cloud deck transition here, the effect of the increased ice and an abbreviated overcast state on albedo should be mentioned beforehand, e.g. in line 7. Otherwise I would suggest to move this section to the discussion.

Introduction

Line 18-24: Please provide some references to this passage.

Line 69: This research question itself sounds very similar to what was already answered by Eirund et al. (2019). I assume the difference is that you simulate a CAO in a Lagrangian perspective, while simulations by Eirund et al. (2019) were idealized and stationary? It would be worth pointing this out here.

Line 72-73: This statement sounds as if your analyses were performed for a variety of CAO throughout the campaign. However, in section 2.1 you describe that you simulate one specific CAO during the shoulder season. In order to allow for an evaluation of the generality of the results found in this study as you mention here, it could be useful to expand your findings to a variety of CAOs, potentially in the discussion. A potential discussion point could be if your results would remain valid if the CAO index was different, e.g. the median of the collected indices?

Simulations of a Cold Air Outbreak

Fig 1: From the coastlines, it looks Figure 1a and b do not exactly cover the same area. It would be helpful to adapt either Figure 1a or Figure 1b, such that the cloud field and the MERRA-2 trajectories match up.

Lines 83-85: This sentence is a bit hard to read, maybe spilt into two sentences.

Line 89: Similar to what I mentioned above, I assume here you mean aerosols available for CCN activation?

Line 95: Please remove “01 May” after the Morrison and Grabowski reference.

Line 109: Is it justified to follow Abel et al. (2017) here, even though their case was in a different location and a different season?

Results

Line 137: Why is this threshold arbitrary (line 190) and not e.g. based on a percentile of the simulated cloud cover? A pdf as shown in Figure 3 of cloud cover could maybe show if the 75% threshold is reasonable considering the cloud field evolution.

Line 147: How are the ensemble simulations set up and why were they performed only for the ice0 case?

Line 159: Does the prognostic CCN implementation allow for recycling of CCN? If yes – doesn’t the evaporation of rain below cloud release CCN, which could be re-entrained into the cloud layer?

Line 170: Abel et al. (2017) as well as Eirund et al. (2019) also show that precipitation formation is necessary for a cloud deck breakup, which might be worth noting as the latter studies also investigated MPCs.

Figure 2: You performed a "no aerosol loss" simulation, but did you also test the development of the cloud field under a scenario where autoconversion is not allowed as a baseline simulation (similar to Eirund et al. 2019 and Abel et al., 2017)? In their studies, the cloud deck remained completely overcast in the absence of precipitation (see my previous comment) - a similar experiment could strengthen your conclusion that precipitation formation is essential for cloud deck breakup also in this case.

Figure 5: It looks like the x-axes do not cover the full range of the vertical cloud water mixing ratio as well as the rain drop concentration shown in the small plots to the right of the contour plots.

Line 210: “substantial deepening of the PBL associated with longwave cooling” – do you have evidence of high LW cooling?

Line 214: “diurnal” instead of “duirnal”

Line 218: Is that really so unclear? It has previously been shown that cloud ice generally increases precipitation (Knight et al., 2002, Field & Heymsfield, 2015), which can then initiate regime transitions and cloud dissipation (Abel et al., 2017, Eirund et al., 2019).

Line 233: “ice vapor growth” – do you mean growth by deposition?

Same line: related to my comment 1, it would be very helpful to include the riming rates or the snow content/snow particle concentration e.g. in Figure 2 in order to follow this thought. Otherwise, the LWP reduction through riming sounds more like a suspicion rather than a fact.

Figure 6c: I assume, in the legend, the u-phys term should be dashed?

Line 240: similar to my above comment, where is the evidence for precipitation in the form of riming-grown ice crystals? Figure 2f only shows precipitation.

Line 285: Did you also investigate differences in longwave radiation between the simulations? As the difference between SST and cloud top temperature is quite large (Figure 1b, Figure 2h), is would be interesting to see the effect of changes in longwave radiation versus the simulated change in albedo/shortwave radiation.

Discussion

Line 349: How (and why) do you assume N_{inp} to change in a warming climate? Would the change in cloud ice alone not be sufficient for a negative cloud-climate feedback in the future?

I also think in the context of climate impact, it would be worth to again highlight the strong difference in albedo (as shown in Figure 2i) between the different simulations in the Discussion.

References

Knight, C. A., Knight, N. C., Dye, J. E., & Toutenhoofd, V. (2002). The mechanism of precipitation formation in Northeastern Colorado cumulus: I. Observations of the precipitation itself. Journal Atmospheric Research, 31(8), 2142–2147. https://doi.org/10.1175/ 1520-0469(1974)031h2142:tmopfi2.0.co;2

Field, P. R., & Heymsfield, A. J. (2015). Importance of snow to global precipitation. Geophysical Research Letters, 42, 9512–9520. https:// doi.org/10.1002/2015GL065497