Review of Papakonstantinou-Presvelou and Quaas: Sensitivity experiments with ICON-LAM to test probable explanations for higher ice crystal number over Arctic sea ice vs. ocean

Summary and Major Comments:

This study is a follow-up to a previous study by the same authors which found that ice crystal numbers, as estimated by DARDAR-Nice, are higher on average in Arctic low clouds occurring over sea ice than those occurring over open ocean. They test three hypotheses for why this might be: that there are more INPs over sea ice than over open ocean, that blowing snow contributes cloud ice particles to low clouds over sea ice, and that secondary ice production operates differently over the open ocean and over sea ice. They conclude that, because secondary ice production increases ice crystal numbers more over open ocean than over sea ice, blowing snow and more INPs over sea ice are the only tested processes that can explain the discrepancy in ice crystal numbers between clouds over open ocean and over sea ice.

Unfortunately, I am completely unconvinced of the authors' conclusions. They test the role of secondary ice production by including one secondary ice production process (Hallett Mossop rime-splintering) that operates within a narrower range of temperatures than the range that many observational studies have found secondary ice processes to operate on. They do not provide an explanation for why they expected that Hallett Mossop rime-splintering might behave differently in clouds over open ocean and over sea ice (and why it ultimately does but that the difference is in the wrong direction to explain the discrepancy).

They simulate a source of INPs over sea ice by multiplying the organic INP concentrations in ICON by constant (and large) factors over sea ice. They choose to do this with the organic INPs because they activate at high temperatures and the discrepancy in ice crystal numbers between low clouds over sea ice and over open ocean is largest at high temperatures (lines 235-237). Thus, they choose the INP properties that would best resolve the discrepancy, not ones that they expect based on observations or physical reasoning. In their literature review, they mention that there have been measurements of biogenic aerosol that can serve as INPs over both open ocean and melting ice, but they did not provide any evidence for there being larger concentrations of those particles over melting sea ice than over open ocean. Furthermore, they increased the INP concentrations uniformly over all sea ice, without taking into account where there was actually melting.

The approach for testing blowing snow is also simplified because the authors assume snow particles directly become cloud ice particles which has not been suggested by the studies on this topic (as the authors explain in lines 242-247). However, this test does have more solid physical reasoning behind it than the increase in organic INPs over sea ice.

For all three tests, the authors use several scaling factors for each parameterization. Taking into account the simplicity of the parameterizations used combined with the use of arbitrary scaling factors, it is very difficult to believe that the physical processes the authors intend to test are being well-represented.

I also don't think that the explanations the authors investigated are the only ones capable of explaining the discrepancy they find in DARDAR-Nice. They have not considered that there may be differences in the dynamical environments between the clouds over open ocean and those over sea ice. The authors acknowledge that clouds over sea ice and over open ocean have different morphologies (lines 49-50), wherein clouds over open ocean are more likely to be stratocumulus and clouds over sea ice are more likely to be thin stratus. Stratocumulus clouds have more turbulence in them which can alter liquid and ice formation processes, and the interactions between liquid and ice particles. However, that turbulence is not resolved in the kilometer-scale model that the authors use and thus their analysis is not sensitive to this alternative explanation.

Looking to the dynamics makes sense because even if INP sources are different at the surface over open ocean and over sea ice, the particles can be transported over long distances and the INPs in the clouds may not perfectly reflect those at the surface. Thus, clouds over open ocean can have sea ice INPs in them, and clouds over sea ice can have open ocean INPs in them. The dynamics are largely controlled by the fluxes at the sea surface which change instantaneously as you go between sea ice and open ocean.

Given the simplicity in the microphysics parameterizations used and the limitations of kilometer-scale models in representing the dynamics influencing microphysics in low clouds, I strongly recommend that the authors rely more strongly on the observations available to sort through their hypotheses for why ice crystal numbers are larger in the low clouds over sea ice.

The authors only use the aircraft observations to compute ice crystal numbers, and the observations fall out of their analysis after Figure 1 although they would also be relevant for Figures 3-5. It would be better if they looked at the observed ice crystal size distributions. Then they could see the differences both in ice crystal number and ice crystal size between clouds over sea ice and clouds over open ocean, and that could give some clues as to what the ice formation mechanisms are in each population of clouds. Furthermore, they can use the observed size distributions to evaluate the assumed size distributions in DARDAR-Nice, which may not be equally appropriate for the clouds over open ocean and over sea ice. Additionally, the authors should give an explanation for the discrepancy between ice crystal numbers from aircraft and from DARDAR-Nice over sea ice at temperatures between -20 and -30 C (Figure 1).

The authors can use satellite estimates of melt pond fraction in conjunction with observations from both aircraft and DARDAR-Nice to see if ice crystal numbers are larger over melt ponds, and to test that idea that melt ponds are a source of INPs over sea ice. If blowing snow is an important contributor to ice crystal numbers over sea ice, then the ice crystal numbers from both aircraft and DARDAR-Nice should decrease with height. Additionally, the authors can look for relationships between surface wind speeds from reanalysis and observed ice crystal numbers to further investigate blowing snow.

The observations that ice crystal numbers are larger in low Arctic clouds over sea ice than over open ocean, contrary to expectations, is interesting, and the authors can make a nice study in sorting out the possible explanations. However, I do not believe that they have considered enough hypotheses here, or done a thorough enough investigation of the processes that they have considered. I look forward to a future version of this study that uses the full suite of observations and simulations available to build a convincing and consistent physical picture of how ice particles form and/or evolve differently in the two populations of clouds studied (low Arctic clouds over sea ice and over open ocean).