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Interactive comment on "Modelling the relationship between liquid water content and cloud droplet number concentration observed in low clouds in the summer Arctic and its radiative effects" *by* Joelle Dionne et al.

Anonymous Referee #3

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In this paper, the authors investigate whether the linear relationship between LWC and CDNC observed in clouds during the NETCARE field campaign can be partially explained as due to autoconversion. They find that autconversion is an important contributor to this relationship, although some of the variability is captured by their model even in the absence of autconversion, and therefore must be due to other sources. The paper is generally clearly written, and I feel that it warrants publication, provided that the following comments are addressed.

General comments:

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Discussion paper

1. Is cloud droplet sedimentation (i.e. gravitational settling) included in SCM-ABLC? This should be explicitly stated, especially for the interpretation of the “no rain” case results. If not, then it is possible that this process would allow the model to better simulate the cases with the lowest CDNC values, due to the larger modelled cloud droplet sizes, regardless of which autoconversion scheme was used. For the case with a CDNC of 5 cm-3 in particular, the large modelled size of the cloud droplets could allow cloud droplet sedimentation to be significant, even in the absence of collision-coalescence processes to grow the cloud droplets to drizzle drop sizes. This should be discussed.

2. Does the cloud vertical extent vary between simulations with different autoconversion schemes? Does the relationship between cloud vertical extent and CDNC differ between autoconversion schemes? The sensitivity test described on page 22 suggests that this could be important for shortwave radiative fluxes. This should therefore also be included in the discussion of aerosol indirect radiative effects on page 22.

3. I do not see any justification shown for the authors’ inconsistent use of either $p=0.01$ and $p=0.05$ as thresholds for significance. The use of different thresholds is most jarring in the abstract, on P21, lines 535-539, and on P21-22, lines 541-546. In all three locations, a value of $p>0.01$ is used to imply that no significant difference exists, and $p<0.05$ is used to imply that a significant difference does exist.

I would suggest that if the authors have a justification for using a particular p value as the threshold for significance for this set of data, that it be included. If multiple different p value thresholds are used, this should be justified.

Alternatively, the discussion of p-values could be rephrased such that statistical significance is not a binary value: p-values and significance would thus be treated similarly to the way that Rˆ2 values and correlation are currently discussed in this and many other manuscripts.

Specific comments:

P2, lines 48-52: Changes in CDNC have also been linked to changes in cloud-top radiative cooling, which subsequently affects LWP through changes in cloud vertical thickness (e.g. Possner et al., 2017).

P4, lines 98-101: At least for the context of the observations, please offer a numerical value for the Mauritsen limit here. Also, the definition of the Mauritsen limit as "it is a proposed threshold for aerosol concentration, below which cloud droplets that form grow to sizes large enough to precipitate" is imprecise. Some cloud droplets will grow to sizes large enough to precipitate in many clouds with larger aerosol concentrations. And if it was true that below the Mauritsen limit all cloud droplets immediately grew to precipitation sizes, then no droplets would be observed in the cloud droplet size range. Please provide more precise definitions of the Mauritsen limit and the tenuous cloud regime. It might be helpful to define the tenuous cloud regime first, and to define the Mauritsen limit in that context.

P5, lines 146-147: For what reason is it expected that the number of larger droplets was negligible during these flights?

Section 2.1: It would be helpful to give a description of any available observations of precipitation (or the absence thereof).

P9, lines 241-243: This was not completely clear to me. Was the modification of the boundary-layer height an iterative process, requiring multiple simulations? Was the location of the LWC maximum in each timestep compared to the location of observed LWC maximum, and the boundary-layer height adjusted online during a single simulation?

P11, lines 310-315: If aerosols were omitted in the radiative transfer calculations, then why were their optical properties computed? Also, please directly reference the parameterizations used for the optical properties.

P13, line 370: This function cannot be correctly described as a linearization. It would be better to call it a piecewise function.

P22, line 551: Considering the discussion of statistical significance that precedes it, a different word than “significantly” should be used here.

Figures 2, 3: Would it be possible to have a single label for the corresponding lines and points in the legends? For example, could “Observations” be listed only once, with both the green line and green triangle? This would make the legends much clearer.

Technical corrections:

P1, line 21: “clouds,.” -> “clouds.”

P3, line 70: “consider the compare” please rephrase.

P9, line 246: please replace the dash after “SCM-ABLC” with a colon.

P9, line 257: please add a space after “ocean”.

P9, line 257: please add “implemented” after “As”.

P20, line 522: Perhaps it would be clearer to say “radiative transfer calculations” instead of “model runs”. The current phrasing leaves some ambiguity between the SCM-ABLC simulations (which are based on observations) and the radiative transfer calculations based on in-flight observations only.

References:

Possner, A., Ekman, A. M. L., and Lohmann, U.: Cloud response and feedback processes in stratiform mixed-phase clouds perturbed by ship exhaust, Geophys. Res. Lett., 44, 1964–1972, https://doi.org/10.1002/2016GL071358, 2017.

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