

## ***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 #1**

Received and published: 7 May 2019

I have reviewed “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 Dionne et al. My comments are mainly on presentation, but I do have two substantive concerns on the analysis that put the manuscript in the major revisions category.

C1

### **1 Comments on presentation**

I am not an expert on arctic clouds, but globally, the question of LWP adjustments to  $N_d$  changes is extremely important in the context of rapid adjustments (formerly the “cloud lifetime effect”) to the radiative forcing by aerosol–cloud interactions (formerly the “Twomey effect”). See, e.g., Gryspeerdt et al. (2019 ACP); Rosenfeld et al. (2019 Science); Mulmenstadt and Feingold (2018 Current Climate Change Reports). It might help to make the connection to this literature in the introduction. As currently written, the introduction left me confused whether this is an aerosol–cloud paper (as the focus on  $N_d$  would suggest); or a feedbacks paper (as the statement about rapid warming would suggest); or a paper on the interaction between the two, in which case, perhaps cite Nazarenko et al. (2017 JGR) or Lohmann (2017 JGR) or an arctic equivalent, if that exists. However, from the main text, I think it’s an ACI paper, so I would focus the introduction on LWP adjustments to the Twomey effect.

The other major presentation question I had after reading the paper was what justified this focus on pure liquid clouds. Perhaps this betrays my ignorance, but I thought the most radiatively important low cloud type was mixed-phase in the Arctic, even in summer (Shupe and Intrieri, 2004?; de Boer et al., 2009).

### **2 Comments on the analysis**

My main substantive concern is that I am not convinced the findings are statistically robust. If I understand the analysis correctly, the authors simulated 11 cloud profiles with different mean  $N_d$  and LWC. In the observations, cloud-mean LWC is proportional to cloud-mean  $N_d$ , based on these 11 data points. The authors then tried different micro-physical schemes to determine which one is best able to reproduce the observations. A setup without autoconversion gives the worst regression coefficient, and based on

C2

this, the authors conclude that autoconversion is responsible for the proportionality.

My concerns, in detail, are:

1. The no-autoconversion setup does pretty well, actually. It simulates higher LWC than the other setups, but that is to be expected, because a big sink process for LWC is turned off. The slope in Fig. 2 looks indistinguishable from the setups that are claimed to work better.
2. The constant  $N_d$  runs also have pretty large slopes, which would indicate to me that a big part of the LWC increase is *not* due to autoconversion, or at least not due to the parameterized  $N_d$  dependence in the autoconversion rate.
3. Continuing on that thought: in an attribution of an observed relationship to a candidate process, I would want to see some discussion on why other candidate processes are eliminated. Eliminating candidate processes is, of course, what models excel at, but confirming them, not so much (Oreskes et al. 1994, maybe?). First, I would want to know whether the clouds are adiabatic. Then, I would want to know what stage in their life cycle they are in. At that point, a clearer picture may start to emerge; for example, in an adiabatic cloud, the vertically averaged LWC increases with cloud geometric thickness (thermodynamic conditions being equal), and the geometric thickness increases with  $N_d$  (Pincus and Baker, 1994), purely from energy budget considerations.

Based on those concerns, I think a more convincing way to approach the problem would be first to build a conceptual model of the clouds and then to eliminate candidate processes (which probably requires numerical modeling), rather than to pick one process seemingly arbitrarily and trying to “confirm” it (because, as we know, science is the process of hypothesis refutation, not hypothesis confirmation).

My methodological comment is on letting the single-column model run to equilibrium. Actual clouds do not reach equilibrium, because precipitation acts as a condensate

C3

sink that (along with evaporation) causes the clouds to dissipate. In your method, the condensate loss by precipitation is balanced by moisture supply by advection, allowing the cloud to live forever. Clouds that live forever seem like a major limitation in a study on the cloud lifetime effect.

On the other hand, the model obviously needs to spin up.

This is a problem that the authors need to solve, but two suggestions they may find useful are:

1. Argue that clouds at any given point in time are in “quasi”-equilibrium. This is actually an assumption in many GCM parameterizations, i.e., the state the GCM tries to capture is representative of a cloud field averaged over a fairly long time step (30 minutes). However, I don’t know if I would buy the argument for an individual cloud.
2. Spin up the model with one  $N_d$ , then observe the transient behavior when you abruptly change to a different  $N_d$ .

There is a series of papers by Andrew Gettelman (2015) on SCM studies of different cloud microphysics that might provide insight.

---

Interactive comment on Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2019-290>, 2019.