This manuscript performs global model simulations with a simplified dust-specific ice nucleation parameterization, which relates the activation temperature for immersion freezing to dust number concentrations, to investigate the low bias in outgoing shortwave radiation fluxes over the Southern Ocean (SO). After implementing the parameterization into the Met Office's Unified Model, more LWP and less IWP are simulated in the Southern Ocean (SO), along with an increase in cloud albedo. However, the outgoing shortwave radiation fluxes in SO are found to decrease, likely due to a reduction in cloud fraction, which makes the bias over the SO even worse. The authors conduct sensitivity experiments to investigate the cloud fraction decrease.

The question that the authors investigate is important and very interesting. However, unfortunately, the authors seem to have conceptual misunderstanding on the impact of aerosols on ice nucleation process, and thereby the dust-specific ice nucleation parameterization proposed and used in this manuscript is not valid. Besides, I have some concerns related to the interpretation of the results and experiments performed in the discussion. I therefore recommend rejection of this work.

General comments:

1. The statement that higher (lower) dust number density results in higher (lower) nucleation temperature is incorrect. It has been well established that the activation temperature for immersion freezing is related to aerosol species, instead of aerosol concentrations. As found by many observational studies, organic and biogenic aerosols tend to nucleate at warmer temperatures, while dust particles have lower activation temperatures. Therefore, the parameterization proposed in this paper that relates the activation temperature to dust concentrations is not valid. Even if the parameterization is valid, the authors should explain why they choose this formula and evaluate it against observations. This is the major reason for my rejection of this work. It is also not clear to me why the authors link the dust concentrations and the activation temperature of heterogeneous ice nucleation to the detrainment temperature in convection scheme. In other words, how is the detrainment process related to primary ice formation in the convection system?

Actually, there are many dust-specific ice nucleation parameterizations that are ready to use (e.g., Atkinson et al., 2013; DeMott et al., 2015; Hoose et al., 2010; Knopf & Alpert, 2013; Niemand et al., 2012; Ullrich et al., 2017; Wang et al., 2014). These parameterizations are derived based on either observational or theoretical evidences. They have also been implemented into regional and global models. The authors may want to use these parameterizations in their future work.

2. The authors should evaluate the modeling results against observations, before concluding if the new parameterization leads to any improvements in the model. For example, the authors can use MODIS LWP, CloudSat IWP, and MODIS cloud fraction. It would also be interesting to compare the simulated shortwave and longwave cloud forcing (SWCF and LWCF) with CERES-EBAF dataset. If possible, the authors may also evaluate the simulated dust and INPs in SO. For dust, the

authors can use CALIPSO dust extinction vertical profiles. For INPs, a lot of field measurements are available in SO, e.g., CAPRICORN campaign (McCluskey et al., 2018).

3. To investigate the cloud fraction decrease in exp_{dust} over SO, the authors include the comparisons between exp_{cap} and *control* in their discussion. However, exp_{dust} and exp_{cap} are two experiments with different modifications in the microphysical processes. What happened in $\mathrm{exp_{cap}}$ should not be expected in exp_{dust}. Therefore, such comparisons do not help to understand the cloud fraction decrease in exp_{dust}. The authors should instead look into the changes in RH, precipitation, and probably lower-tropospheric stability (LTS) in exp_{dust}.

4. The sensitivity experiment, exp_{eff}, is not carefully designed. Why do you assume the liquid clouds are equally spread as the ice cloud in the convection scheme? Does this assumption make the model more physically correct? Are there any previous literatures that can support your assumption? Also, it is not fair to compare the DJF results in $\mathrm{exp_{eff}}$ with the annual mean results in $\mathrm{exp_{dust}}$.

Other comments:

Line 38: "… can proceed quicker …". It should be "proceed at warmer temperatures".

Section 2: It would be better to include how dust is parameterized in this section.

Section 3: The word "prognostic-dust parameterization" in the title of this section sounds like a dust transport parameterization. Please consider to replace it by something like "dust-specific ice nucleation parameterization".

Eq (1). How do you get the ice nucleation concentrations or the immersion freezing rate from $thetr_n$.

Line 162: "…, probably accounting for … than before". This sentence is not clear to me.

Line 164: Why do you show the IWP and LWP for stratocumulus boundary layer clouds only? Why not show those for the whole column?

Line 211-213: How do you know the liquid cloud fraction is smaller than the ice cloud fraction? What about the mixed-phase clouds? Also, the explanation in the second sentence does not make any sense.

Figure 6. It would be better to give a subtitle for each panel.

Reference

Atkinson, J. D., Murray, B. J., Woodhouse, M. T., Whale, T. F., Baustian, K. J., Carslaw, K. S., Dobbie, S., O'Sullivan, D., & Malkin, T. L. (2013). The importance of feldspar for ice nucleation by mineral dust in mixed-phase clouds. *Nature*, *498*(7454), 355–358. https://doi.org/10.1038/nature12278

DeMott, P. J., Prenni, A. J., McMeeking, G. R., Sullivan, R. C., Petters, M. D., Tobo, Y., Niemand, M., Möhler, O., Snider, J. R., Wang, Z., & Kreidenweis, S. M. (2015). Integrating laboratory and field data to quantify the immersion freezing ice nucleation activity of mineral dust particles. *Atmospheric Chemistry and Physics*, *15*(1), 393–409. https://doi.org/10.5194/acp-15-393-2015

Hoose, C., Kristjánsson, J. E., Chen, J.-P., & Hazra, A. (2010). A Classical-Theory-Based Parameterization of Heterogeneous Ice Nucleation by Mineral Dust, Soot, and Biological Particles in a Global Climate Model. *Journal of the Atmospheric Sciences*, *67*(8), 2483–2503. https://doi.org/10.1175/2010JAS3425.1

Knopf, D. A., & Alpert, P. A. (2013). A water activity based model of heterogeneous ice nucleation kinetics for freezing of water and aqueous solution droplets. *Faraday Discussions*, *165*, 513. https://doi.org/10.1039/c3fd00035d

McCluskey, Hill, T. C. J., Humphries, R. S., Rauker, A. M., Moreau, S., Strutton, P. G., Chambers, S. D., Williams, A. G., McRobert, I., Ward, J., Keywood, M. D., Harnwell, J., Ponsonby, W., Loh, Z. M., Krummel, P. B., Protat, A., Kreidenweis, S. M., & DeMott, P. J. (2018). Observations of Ice Nucleating Particles Over Southern Ocean Waters. *Geophysical Research Letters*, *45*(21), 11,989-11,997. https://doi.org/10.1029/2018GL079981

Niemand, M., Möhler, O., Vogel, B., Vogel, H., Hoose, C., Connolly, P., Klein, H., Bingemer, H., DeMott, P., Skrotzki, J., & Leisner, T. (2012). A Particle-Surface-Area-Based Parameterization of Immersion Freezing on Desert Dust Particles. *Journal of the Atmospheric Sciences*, *69*(10), 3077–3092. https://doi.org/10.1175/JAS-D-11-0249.1

Ullrich, R., Hoose, C., Möhler, O., Niemand, M., Wagner, R., Höhler, K., Hiranuma, N., Saathoff, H., & Leisner, T. (2017). A New Ice Nucleation Active Site Parameterization for Desert Dust and Soot. *Journal of the Atmospheric Sciences*, *74*(3), 699–717. https://doi.org/10.1175/JAS-D-16-0074.1

Wang, Y., Liu, X., Hoose, C., & Wang, B. (2014). Different contact angle distributions for heterogeneous ice nucleation in the Community Atmospheric Model version 5. *Atmospheric Chemistry and Physics*, *14*(19), 10411–10430. https://doi.org/10.5194/acp-14-10411-2014