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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Modelling atmospheric structure, cloud and their response to CCN in the Central Arctic: ASCOS case studies" *by* C. E. Birch et al.

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Reviewer general comment:

This is an interesting paper that addresses important observations of clouds in the Arctic. Polar clouds are badly represented with NWP and Climate models and this is a good attempt to redress these failures. While I have no hesitation in recommending the paper for publication there are a few minor points that could be clarified.

Response:

We thank Reviewer #2 for the review and the very positive evaluation of our manuscript.

Responses to the individual comments are given below.

Reviewer comment 1:

The authors assume that the vertical distribution of LWC within the cloud is adiabatic (20 2565) and I wonder if that is true of the tenuous clouds observed in the 5th regime? I would like a bit explanation of the physics of the tenuous cloud.

Response:

The degree to which clouds in the 5th regime are adiabatic is unknown and cannot be evaluated using the available observations. As stated in the text, there is some certainty regarding the boundaries of the cloud liquid water and the total LWP can be determined (with the stated uncertainty). Otherwise, there is little information on the vertical distribution of the cloud liquid water. Furthermore, the specific physics involved in these clouds is unknown. However, Mauritsen et al. (2011) have suggested that these clouds are tenuous due to a lack of CCN, such that the available moisture is consumed by the few droplets that are able to form and these droplets grow large enough to fall out of the cloud layer. In a CCN-limited regime it is quite possible that the liquid water is not adiabatically distributed, but further work is needed to provide more insight.

Reviewer comment 2:

I also wonder about the effect of open water leads on the albedo. The observed albedo seems to be much higher than the model albedo at most times even when a crude estimate is made of the lead fraction. This could have quite a big impact on the model - could a better estimate of the area averaged albedo be made from satellite measurements?

Response:

The model albedo is too low due to the temperature-dependant albedo scheme (discussed in detail by Birch et al. 2009) and this does indeed have a big impact on

the model surface energy budget and thus on surface temperature (absorbs too much downwelling shortwave radiation, surface temperatures are too high, which reduces the albedo to the minimum of 0.5). In the current study this error has a significant effect on the NWP model diagnostics used (Figures 2, 3, 4, 6 and 7, but particularly the temperature diagnostics in Figures 2 and 4). In the SCM runs the albedo was initialised at 0.75 (more similar to that observed) to reduce the impact of this error. Therefore, in the SCM diagnostics (Figure 5 and 9 to 11) this error is much reduced. The fact that the near-surface is still too well-mixed in the SCM (Figure 5) shows that the surface albedo is not the only issue with the model and thus the focus of this paper is on clouds and CCN. The crude estimate of 'albedo with a lead fraction' is shown to give an indication of the lowest possible observed surface albedo that could have occurred during the ice camp. It is at least 0.1 greater than that in the model, which shows the model is definitely underestimating it. This is discussed in section 4 of the current paper and in detail in Birch et al. 2009. The authors feel there is no need to discuss this in any more detail here. Here the focus is clouds, atmospheric stability and CCN. A few sentences have been added to the end of paragraph 4 in section 4 to clarify this.

Satellite measurements can't be used because there is almost always cloud cover, which masks the surface. The aerial photographs are the only way to estimate the open water fraction and these proved more than adequate for the estimation of a maximum open lead fraction (20%).

Reviewer comment 3:

Figure 2 has the temperature in K not degrees C - is think this may be a mistake.

Response:

This is a mistake and will be corrected in the revised manuscript.

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 2559, 2012.