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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Cloud and boundary layer interactions over the Arctic sea-ice in late summer" *by* M. D. Shupe et al.

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1) The suggested text change has been made in the revised manuscript.

2) There is very little discussion in the literature of stochastic, homogeneous freezing of liquid water droplets to form ice crystals at relatively warm supercooled temperatures (base on a Google-Scholar search on "stochastic ice nucleation mixed-phase clouds"). Regardless of whether stochastic homogeneous freezing can occur, it is generally well accepted that heterogeneous ice freezing processes dominate at the relatively warm temperatures observed during ASCOS. Furthermore, the processes of ice formation are not a central subject of this paper. These processes are quite interesting, particularly over the central Arctic, and should be explored in other papers/studies.

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3) We agree that any given threshold of dissipation rate may be imperfect for determining the base of the cloud-driven mixed layer. In our analysis, we examined various fixed thresholds and also a variable threshold based on the magnitude of the dissipation rate in the cloud. While the mixed-layer base did change somewhat depending on the exact definition used, none of the results was substantially impacted. Furthermore, the equivalent potential temperature is also not a perfect means for identifying the mixed-layer base, as a semi-constant equiv. potential temperature simply means that mixing has occurred but not that it is currently occurring. Lastly, the chosen threshold also appears to nicely capture the top boundary of the mixed layer, corresponding generally to the base of the temperature inversion that exists near cloud top. For the case sited by the reviewer, the mixed-layer base derived from dissipation rates is highly variable around the time of the radiosounding and the radiosonde naturally drifts away from the point of launch. With spatial/temporal variability in mind, it is certainly conceivable that the mixed-layer base along the path of the sounding was actually closer to 0.35 km, as might be suggested by the radiosonde. However, we do not know this for certain. Our choice to use a fixed dissipation rate threshold to define the mixed-layer base is intended to identify the base of active mixing suggested by the Doppler radar, which it appears to do quite successfully. Furthermore, we clearly provide the definition that we are using in this study.

4) The reviewer brings up the point that the mixed-layer base might be hard to determine if there is no ice precipitating below the cloud. This is true. We have indicated that turbulent dissipation rates can only be determined where hydrometeors (such as precipitating ice crystals) are present. So in non-precipitating cases, the mixed-layer base cannot be determined using the dissipation rate method. Such time periods were not included in the statistical characterization. This was implied in the initial manuscript, but the text in the first paragraph of Section 4 has been modified to make this point clearer. The following statement has been added: “Moreover, cases are only considered where hydrometeors are present from cloud level down to the surface, or to below the identified mixed-layer based, to ensure that the mixed-layer base can be determined using

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the methods presented here."

5) This comment is basically the same as the one addressed in #3 above. The short answer, in agreement with the reviewer, is that spatial and temporal variability could lead to some of these small differences. The secondary answer is that quasi-constant equivalent potential temperature as a function of height does not necessarily show where active mixing is currently occurring, but rather where it has occurred but not yet been modified by some other process.

6) The layer of enhanced turbulence near the surface in this case could be influenced by both advective and local processes and the relative balance of these processes is not entirely clear. We have simply noted that the surface fluxes are very weak in this case. In the text we have been careful to state that the layer grows from the surface upward but not to speculate on the source. To make this even clearer we now state that the layer grows "in depth" from the surface upward.

7) This comment is identical to comment #2 addressed above. The reviewer asks for a specific statement to be "toned down." The statement in question merely states that little information is known on the local ice nuclei (IN) that are needed for ice particle nucleation. The reviewer apparently thinks that IN are not needed for ice particle nucleation, but at the observed temperatures ($\sim$-8C) it is generally accepted by the community that heterogeneous nucleation is dominant. Thus, we have not removed this statement, but have instead added "at these temperatures" to the statement to make it clear that we are not talking about ice nucleation at colder temperatures (where IN may not be needed).

8) References have been thoroughly checked and updated to ensure that all are included in the references section.

9) Table 1 has been updated to be 35 GHz.

10) As requested, the caption for Figure 4 has been modified to note that the 0Z mixed-

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layer base is at the surface.

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