ANSWER TO REFEREE 3:

We are grateful to this reviewer for several pertinent comments and questions. Below, we have copied the reviewer's comments in *red italic*; our response follows below each reviewer's comment. We hope that our responses below, and changes in the manuscript, are sufficient for this reviewer.

In the paragraph that straddles pp. 3819-3820, the authors state that this paper provides a complementary view of Shupe et al. (2013). In that paper, turbulence dissipation rate is used to characterize the rate of coupling to the surface. In the present paper, potential temperature is used toward that end… What is the new aspect of potential temperature that is so compelling?

Shupe et al. (2013) use the radar-derived dissipation rate ε to identify the cloud-driven mixed layers and categorize cloud profiles as coupled or decoupled, depending on whether these mixed layers extend below 150m (first radar gate). Then they use conserved properties for consistency. While deriving profiles of ε requires more ideal conditions (e.g. that mixing is an ongoing process), the method we used here allows us to include a substantial larger portion of data in the study. In addition the applied method is based on the use of scanning radiometer and radiosonde profiles; these instruments' first observation heights are 30m and 16m respectively, which allows us to examine decoupling closer to the surface (decoupling can occur below 150m).

The above analytical description of the differences between the two methods is added in the revised version.

The one major issue I had with this paper is that it was not at all clear how much new information is provided in this paper over previously published work, especially with regard to the Shupe et al. (2013) reference… Are the authors underselling some of the work in the present study, i.e., are there other important differences with Shupe et al. (2013)?

The study by Shupe et al. (2013) was one motivating factor for this study. Some similarities are apparent but also differences. To the extent that they come to the same or similar conclusions this lends credibility to both; there are also differences.

Both Shupe et al. (2013) and the present study examine the cloud-surface interactions during ASCOS, but using different methods (dynamic vs. thermodynamic) and different lengths of the timeseries (1-week vs. whole 40 days of ASCOS). The two studies generally examine different aspects of the cloud-surface coupling issue; Shupe et al. includes only cases where turbulence is generated in the clouds, while the present study also identifies the stably-stratified clouds, with no incloud mixing.

Shupe et al. analyze three example case studies (9h to12h long) to provide a process-level view of what happens in these clouds; time evolution and the transitions between coupled and decoupled states are important aspects of this study. They also give a statistical description of some characteristics of the coupled/decoupled state, although for a limited time period and based only on single-cloud layer profiles. This study, on the other hand, provides a complete statistical analysis on cloud-surface coupling; note that a statistical view on some important parameters (e.g. moisture, winds, surface fluxes, etc) for each cloud state has not been offered before. The main purpose is to highlight properties in the thermodynamic structure that generally

characterize each state and identify the similarities and differences between the three categories (coupled, decoupled, stable).

In addition the structure of precipitation for each category is examined in this paper (no similar study done before). The investigation of the correlations between the thermodynamic structure and the structure of precipitation with reference to each coupling state is an important aspect of the present analysis; an attempt to illustrate how evaporation/sublimation of precipitation affects cloud-surface coupling is made. To summarize, the new information that this paper provides is:

(a) A statistical overview of the thermodynamic and microphysical structure of the different coupling states and their interactions with the surface fluxes.
(b) The study of stably-stratified clouds: their properties, characteristics and structure.
(c) The fact that decoupled clouds can be divided in two subcategories with different features; the first consists of lower decoupled clouds with shallower subcloud mixed layers (SML), which are disconnected from the surface with weak inversions and the second includes higher clouds with deeper SMLs, that are decoupled from the surface with stronger inversions. An important finding is that evaporation/sublimation of precipitation impacts mainly the latter case; this illustrates that such processes can amplify the decoupling, but they are probably not the primary factor that drives the decoupling.

Finally, there are obviously parts where the two studies overlap:

(a) Shupe et al. provide some statistics on cloud boundaries and cloud properties regarding the coupled/ decoupled state, similar to Fig. 5, Fig. 9 and Fig. 10. However, their statistics are based on a substantially smaller portion of data (~up to 5 days). Moreover, these figures are also of great interest because they include some important information on the newly-introduced cloud state: stable clouds. In addition, as discussed in the text (P3834 L4-18), the use of longer timeseries compared to Shupe et al. can affect the statistics on cloud properties (LWP) and lead to different conclusions.
(b) P3860 Fig. 6 and P3861 Fig. 7 add no new information on the previous study and thus they are removed in the revised version.

The above differences between the two studies are stated in the introduction of the revised manuscript.

What I would find really useful is a quantitative description of the degree of overlap of the data categories between the two manuscript's definitions of stable/neutral and well mixed/decoupled. Are there at least some similarities with the samples in each category between the two papers?... I see that the present approach allows the authors to use much more data, but are the categories similar? Does the relative sample size remain similar between the categories, or is one type of cloud more frequent than the others depending on the observation used (turbulence dissipation vs. conserved thermodynamic quantities)?

Shupe et al (2013) analyze only clouds that can drive mixing, thus only coupled and decoupled cases. They also study a week-long period of ASCOS, known as the fourth period of the ice drift (see P3822, L4-7) and show that decoupled state occurs 75% of the time, whereas coupling occurs only 25%. Sotiropoulou et al also take a third state

(stably-stratified) into account, when calculating occurrence statistics. For the same week-long period (P3858, Fig. 4) we find that 65% of the profiles are decoupled, 23% are coupled and 12% stable; thus taking only the neutrally-stratified cases into account, the coupled and decoupled occurrence statistics are very similar to the results in Shupe et al (P3830, L4-12). The different methods do not lead to different statistics, which lends credibility to both methods.

The occurrence statistics are mainly affected by the total sample size of observations used for each study. The present study estimate that during the whole ASCOS (all ice drift periods plus transits), 46% of the available radiosonde profiles are decoupled, 28% are coupled and 32% stable (to compare more subjectively with Shupe et al results, we can exclude the stable case: 62% of the neutrally-stratified clouds are decoupled and 38% coupled). The higher decoupled cloud fraction found by Shupe et al is due to the fact that they focus on a short period with relatively steady conditions, when a persistent stratocumulus deck is observed. This cloud layer has most often its base above 500m; both studies show that such high clouds are more frequently decoupled from the surface than coupled to it (P3859, Fig 5). On the other hand, the present study includes all ASCOS periods which are characterized by variable weather conditions. Hence this study includes a substantial number of cloud profiles with lower bases (<500m) which are usually coupled to the surface or stably-stratified.

A short paragraph that compares the above results between the two studies is added in section 3.1 in the revised manuscript.

p. 3826, lines 17-18: why limit the inversion detection to only 100 m above the cloud top? Sometimes the thermal structure could be rather ragged above the cloud top and one could miss inversions with this approach.

Sedlar et al. (2011) did a detailed analysis of Arctic low-level clouds that are either capped by the inversion or extend above the inversion base. They showed that the cases where cloud tops reside in the inversion are 75% of the total ASCOS profiles, whereas for the 25% of the cases the clouds are capped by the inversion, the difference between inversion base and cloud top height is of the order if a few tenth of meters, certainly within 100 m. Thus this threshold would be sufficient for the specific dataset.

Along the same lines, I also found it confusing that in some places the authors discuss some of these ideas in equivalent potential temperature space, but some of the later discussion (e.g., Fig. 15) is done in potential temperature space.

The reason why Θ is plotted later in the analysis, instead of Θ_E, was explained on P3838, L23-29 in the original manuscript. Equivalent potential temperature is a conserved property that is not affected by evaporation/condensation processes; thus a constant Θ_E profile indicates mixing. On the contrary, potential temperature tends to increase in the cloud interior because of the release of latent heat due to condensation. For the above reasons it is easier to identify mixed layers using Θ_E profiles and classify clouds as coupled, decoupled or stable. The only defect of using Θ_E is that, as it is estimated as the sum of a temperature and a moisture term: $\Theta_E = \Theta + (L\,\Theta / C_p T)\, Q_v$, it could be hypothesized that a decrease in temperature might be balanced by an

increase in humidity, resulting in a constant Θ_E profile; despite the fact this case would be thermodynamically decoupled in T and Θ profiles, it would appear as coupled in Θ_E. To ensure that such a case does not occur in our dataset, we plot Θ instead of Θ_E in Fig 15; this shows that the profiles we initially classified as coupled using Θ_E do not include any case of thermodynamic decoupling that is masked by an increase of humidity at the decoupling height. Thus there is consistency between results based on Θ and Θ_E .

And in the same paragraph, the ice drift is brought up a few times but it was hard to see if there was any result on the relationship of the relative occurrence of the different types of clouds with ice drift. Did the authors conclusively show a relationship between the two? How can the cloud structures (decoupled/coupled and neutral/stable) and their connection to the ice be separated from meteorological variability? (And I would assume there is a connection between ice drift and weather variability.) There was some discussion of horizontal winds, and a figure towards the end of the paper, but the relevance with ice/meteorology could be made clearer.

The ice drift refers to a period when the icebreaker was moored to and drifting with the ice. The analyzed transit periods were however also entirely within the pack ice; the ice cover was similar during the entire period. Hence all the variability we see is due to atmospheric variability and not to changes or variability in the ice conditions; this is explicitly mentioned in the revised text.

The effects of weather variability can be excluded by focusing only on the 3rd to 5th period of the ice drift, which are characterized by more steady conditions (P3821 L18-29, P3822 L1-10). Figure 2 reveals that stable cloud fraction is higher for periods 3 and 5, when the surface temperature is very low (-6°C and -14°C respectively), whereas the 4th period, when the surface temperature is near the saline water melting point, is almost thoroughly characterized by neutrally stratified-clouds.

p. 3827, lines 26-27: including cloud returns below 300m?

Yes, there is no lower limit in the cloud returns that are included in the analysis apart from natural restrictions (e.g. the instruments' first range gate).

p. 3828, line 26 to p. 3829, line 4: could some of this be driven by coarser vertical resolution of the MW profiler compared to radiosondes?

When a cloud gets decoupled, the surface layer is substantially colder than the cloud-driven mixed-layer, thus both instruments would capture the decoupling; the only difference would be in defining the exact decoupling height; using the scanning radiometer data, the vertical position of the decoupling would be more uncertain than using the soundings profiles.

The only cases where the finer resolution could be responsible for the higher fraction of decoupled clouds detected by the radiosonde, is if decoupling occurs often below 45m (scanning radiometer's 1st measurement height). For this dataset, the minimum decoupling height detected by the radiosonde is ~ 60 m.

p. 3835, lines 25-27: for the decoupled normalization, I take it that the two layers from z=-2 to -1 and z=-1 to 0 are independently normalized since the ratio of the depths of the two layers can vary from cloud to cloud?

Correct; for all states, all different layers are independently normalized, except the free troposphere, above the inversion base/cloud top. This is mentioned in the revised text.