We are grateful to all reviewers for the very thorough reviews and for posing important questions. We have tried to respond to each question/statement and have also edited the revised manuscript because of these.

Below, we have copied the reviewers' comments in *red italic*; our response follows below each reviewer's comment. We hope that our comments and the changes in the revised manuscript are sufficient for all reviewers.

**ANSWER TO REFEREE 1:**

*There is one issue that I would like the authors to discuss a little more. It is about the observation that the stable clouds are water clouds and optically thin. The stable stratification is consistent with optically thin clouds, because the clouds are too thin to experience destabilization from cloud top cooling. But how are these water clouds maintained; why do they not glaciate? In the thick radiatively destabilized clouds, there is often a liquid layer at the top that is maintained by the continuous supply of liquid water from condensation in the cloudy updraughts. Such a mechanism does not exist for optically thin stable clouds. So how are these water clouds maintained. I would expect them to glaciate rather quickly without a turbulent regeneration mechanism. The authors could perhaps discuss this in the paper.*

This is a very good question, and we are grateful to the reviewer for posing it. We have attempted to discuss this further in the revised manuscript, although briefly because of length restrictions.

However, since this study is a statistical analysis on the characteristics of the three cloud states, it inherently does not include information regarding how a cloud state develops over time. Hence we can only speculate. One key may be that we also found that these clouds usually are accompanied by low concentrations of cloud condensation nuclei (CCN); they occur in air that presumably has a low concentration of aerosols in general. Previous studies (e.g. Prenni et al. 2007) have shown that the presence of sufficient ice nuclei (IN) is critical to whether clouds glaciate or not. We speculate that in the low aerosol concentration air there are not enough IN present to initiate ice particle formation. The fact that these clouds are most often very low also means that the temperature is usually only slightly below freezing in summer.

One such case was studied in detail in Mauritsen et al. (2011); the frequent presence of a so-called "fog bow", a halo-like optical phenomenon, strengthens the assumption there are no ice crystals present and the cloud consists of few but large spherical droplets. That they are large is due to the low CCN concentration; the cloud dissipates as the droplets become large enough to sediment out of the cloud, thereby feeding back on the low CCN concentration.

Two additional examples shown below, which illustrate some potential paths for stable clouds' evolution. The first case (Figure 1 below) focuses on an optically thin stably-stratified cloud, with LWP ~15g/m$^2$ and IWP ~0.11 g/m$^2$ that occurs on DoY234 (~5.00am). With time this cloud gets lower and thinner, while surface turbulence becomes weaker until it can no longer reach the cloud layer; eventually this cloud layer has dissipated by DoY 235.

The second case (Figure 2 below) refers to an initially optically thick stable cloud, with LWP ~65g/m$^2$, on DoY 219, 9.30am. At the beginning, this cloud appears stably-stratified and capped by the inversion. With time the cloud thickens and starts extending towards the inversion, while the stable stratification becomes weaker. After a couple of hours, when eventually the cloud extends in and above the inversion, a transition to a coupled state occurs; the weakening of the stable stratification and the transition to the coupled state is also accompanied by a gradual increase in LWP. For this case, it is hypothesized that the lack of incloud mixing is due to fact that the liquid may be homogenously distributed across the cloud layer, not allowing the destabilization of the cloud. As the cloud extends above the inversion and gains more liquid condensate, this distribution changes allowing differential cooling.

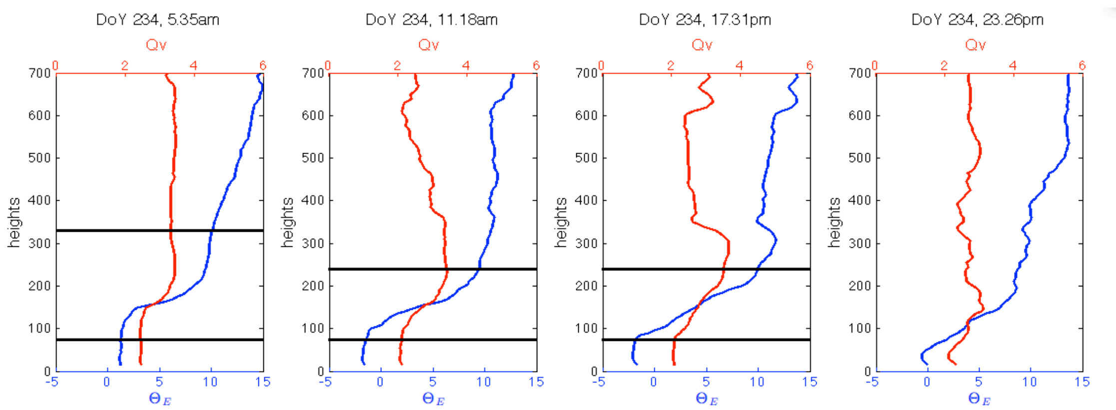


**Fig 1:** Radiosonde profiles of equivalent potential temperature ($\Theta_E$) [$^o$C] (blue) and specific humidity ($Q_v$) [g/kg] (red) for case study 1: DoY 234 (Aug 21$^{th}$, 5.00am) – DoY 235(Aug 22$^{nd}$, 00.00am). Black solid lines represent the cloud boundaries.

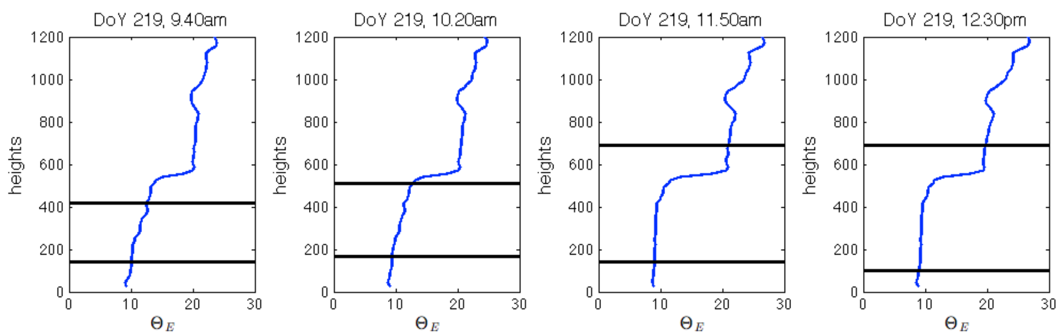


**Fig 2:** Scanning radiometer profiles of equivalent potential temperature ($\Theta_E$) [$^o$C] for case study 2: DoY 219 (9.30am - 12.30pm). Black solid lines represent the cloud boundaries.

These case studies indicate two potential development paths for the stable clouds; (1) they become even more tenuous with time until they dissipate (they probably can be maintained up to a day with this type of stratification). (2) They gain more liquid condensate (e.g. through vertical or horizontal advection) that leads to the redistribution of the liquid across the cloud layer or to the thickening of the cloud, so that eventually they become thick enough to drive turbulent mixing.

A description of the above two cases of stable cloud state and their possible evolution are included in Section 4 of the revised manuscript, although without showing the plots above.

**ANSWER TO REFEREE 2:**

*Whereas the manuscript is technically well-written, it is challenging for the reader to figure out what is actually new, and what is the main finding and/or hypothesis… Further, the authors admirably make no attempt to hide the fact that much of this has already been done by Shupe et al. (2013), albeit with different instruments and covering shorter periods, but this amplifies the need to shorten and focus the presentation. All in all, I find that the manuscript mainly repeats a previous study, the proposed hypothesis is not strongly supported nor does it explain the key finding, and moreover the presentation is longer than it needs to be. In principle, there is nothing wrong in repeating previous studies, this is more the rule than the exception, but we need to focus on the things that are new.*

Obviously we disagree with the reviewer on this point. While it is true that some of the hypotheses emanate from Shupe et al. (2013) – and other papers even earlier – these papers cover analyses of short cases focusing on processes; we would never even think of hiding that fact. Hence some similarities are apparent but also differences. To the extent that the two studies come to the same or similar conclusions this lends credibility to both; there are also differences.

The idea that initiated this research was – based on previous work - to study how often coupling between the Arctic low-level clouds and the surface occurs and what drives this coupling; e.g. is it the surface fluxes or the cloud itself? While Shupe et al. (2013) also attempt to answer similar questions, they mainly examine different aspects of the cloud-surface coupling. First, they analyze three case studies, each 9h to 12h 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 provide 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.

The present study, on the other hand, provides a complete statistical analysis on cloud-surface coupling; note that a statistical view of some important parameters (e.g. moisture, winds, surface fluxes, etc) for each coupling 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). Moreover, the present study employs a different technique and hence is able to utilize more data from ASCOS.

While some conclusions are similar, this study also contains original research. The structure of precipitation for each state is examined (no similar study done before). The correlation between the thermodynamic structure and the structure of precipitation for 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 also made.

Finally, Shupe et al. include only cases where turbulence is generated in the clouds, while we also identify the stably-stratified cloud state, with no in-cloud mixing. 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 identification and study of the stably-stratified cloud state (their properties, characteristics and structure), that was not mentioned in previous studies.
(c) The decomposition of the decoupled clouds into 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 (Fig. 15).

Since the study takes its cue partly from Shupe et al, there are obviously parts where the two studies overlap, where a comparison is a vital component; since after all rests on repeating the same studies using different data or different techniques:

(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; as discussed in the text (P3834 L4-18), the use of longer time series compared to Shupe et al. can affect the statistics on cloud properties (LWP) and lead to different conclusions.
(b) However, we also realize that Fig. 6 and Fig. 7 add no new information over the previous studies and they are thus removed from the revised version of our manuscript.

The differences between the two studies are discussed more clearly in the introduction of the revised manuscript.

*The bulk of the relatively long text is walking the reader through the many presented figures in an apparently random order, and it is not until towards the end that the many threads are tied together.*

Again we have to at least partly disagree with the reviewer. The order of the presented figures is of course not random; it starts with a statistical overview of some main characteristics, moves on to the characteristics of cloud properties and surface turbulence. The purpose is to investigate if it is the cloud or the surface that drives the interactions between them. Later, in the last section, how the boundary layer responds to (or the lack of) these interactions is examined.

In the revised version, the purpose and the questions that initiated this study are stated more clearly already in the introduction, so that the reader can have an idea from the very beginning about what information will follow. The text is also shortened by removing details from the descriptions of the figures.

*The idea that evaporating precipitation can cause decoupling of the sub-cloud layer from the stratocumulus layer is nothing new (e.g. Savic-Jovcic and Stevens 2008, and references therein). I am not an expert on this, but I have a feeling this idea dates back very far in the literature. This should be acknowledged. More at the fundamental level it seems that the authors conclude that this is an important process solely based on precipitation being present below cloud base (3838,18-19). The argument would be stronger if the authors could causally support their claim, for example with some calculation of how much evaporation would be needed to reasonably support decoupling and relate that with observed precipitation rates. Finally, the main finding that decoupling depends on cloud-base height cannot be predicted from this hypothesis; a clear short-coming. In short, I remain to be convinced that evaporation is the dominant process for decoupling.*

Thank you for pointing out this misunderstanding; we do not conclude that evaporating precipitation is a primary factor that drives the decoupling nor do we claim to have invented this idea. That part of the precipitation in general evaporates on its way to the surface is obvious; what is more difficult to estimate is if this in any way feeds back on the dynamics of the whole system.

We find that only cases with relatively deep Subcloud Mixed Layers (SMLs; the strongly decoupled cases) are impacted by evaporation/sublimation, whereas for cases with shallower SMLs there is no such evidence. Thus evaporation/sublimation processes only amplify or maintain the decoupling. Given the typically very moist (high RH) boundary (sub-cloud) layer in Arctic it is not obvious that evaporation/sublimation would have the effect it is known to have in e.g. sub-tropical marine stratocumulus.

Apparently, the way that our conclusions were discussed in the paper was confusing and lead to misinterpretations. In the revised version, we have attempted to clarify this aspect and have also included a more analytical discussion on the enhancement of decoupling by evaporating precipitation in Section 4. Moreover, relevant references have been added.

Unfortunately precipitation rates are not easily available from the observations. Yet, we did a theoretical calculation to estimate that the amount of precipitation required for a certain layer to cool sufficiently by evaporation/sublimation to cause decoupling is not unreasonable.

For this purpose, a case study from ASCOS was used: a strongly-decoupling cloud on DoY 241, at 17:40pm, with a lower inversion of ~1.5°C (top-base) separating the surface layer from cloud-driven mixed layer. We consider a portion of the SML ~ 100m deep, where all amount of precipitation that falls into it, evaporates. Then, the amount of precipitation that must evaporate to produce cooling of this magnitude (1.5 °C) at a certain timescale is: $dQ/dt = Cp/Lv * dT/dt \rightarrow dQ \sim 0.0005$kg/kg.

Assuming that $m_{dry\ air} > m_{moist\ air}$ , $dQ \approx d\rho_v/\rho_d$, the increase in water vapor density for the whole layer (column) is: $d\rho_v*dz= dQ * \rho_d *dz = 0.0637$ kg/m$^2$. To achieve such an increase in vapor supplies within one hour, the precipitation rate that falls into that layer must be: dq/dt= 0.0637 kg/m$^2$/hour = 1.5288 kg/m$^2$/day ≈ 1.5 mm/day. For the same to happen to happen within 3 hours: dq/dt = 0.0212 kg/m$^2$/hour= 0.5088kg/m$^2$/day ≈ 0.5 mm/day.

Of course the assumption that all the precipitation that falls in layer evaporates is unrealistic. On the other hand, here the calculations refer to the case where decoupling is driven entirely by evaporating precipitation which is also unrealistic; our argument is that evaporation can mainly enhance the decoupling, thus smaller rates of evaporation would be sufficient for that and the values provided above are by no means unrealistic. While this is of course no proof that this is actually happening, it shows that our assumption is reasonable.

*3816,6 consider a different word than 'creates':*

OK, 'generates'

*3817,5 I don't understand what is meant by an increasing Arctic amplification*

The referenced papers suggest that Arctic amplification (that climate change is larger in the Arctic) observed today is expected to become stronger in coming decades.

*3817,8 In models this is not the case (Winton 2006, Pithan and Mauritsen 2014)*

Models are models, and not necessarily a good base to judge processes by. However, this sentence is rephrased and relevant references are added in the revised version.

*3817,27 These references are concerned with trade-wind cumulus. In climate models spread in Arctic cloud feedback is surprisingly small (Vial et al. 2013, Pithan and Mauritsen 2014).*

First, this sentence is not about Arctic clouds but about clouds in general; hence it is moved to the start of the paragraph as an introduction.

Second, we would argue that the reviewer's statement is not entirely correct, or at least that it is unknown what the Arctic climate sensitivity is, depending on what model that is analyzed. What is true is that the cloud climatology in the Arctic in different models is very, very different and that the differences in modelled climate change is larger in the Arctic than elsewhere.

Note that we are not claiming a global climate feedback from Arctic clouds; that would be bold. We are claiming a potentially large regional feedback from the clouds in the Arctic.

*3818,2-3 I looked in Shupe et al. (2011) figure 2 which is referenced, and couldn't understand how the authors interpreted this as 80-90 percent. Same lines; the word 'occur' occurs twice.*

In Shupe et al. (2011) figure 2, the annual cycle of monthly mean cloud occurrence fraction is given; it varies from 58% to 83%, with the maximum corresponding to summer (August). Curry and Ebert (992) present a similar figure (Fig. 1) where the cloud fraction is larger than 80% for all summer months. Same figure (Fig. 2) in Wang and Key (2005) shows that according to surface-based observations cloud fraction during summer months is around 85% during the whole summer.

Moreover, average cloud fractions from three expeditions on the Oden (1991, 2001 & 2008) from direct ceilometer observations reported in Tjernström et al. (2012) are equally high. Thus our statement that cloud occurrence during summer is about 80-90% is quite reasonable, and conforms to several previous studies and available observations.

*3818,6 What is meant by 'together in the same volume'? Is really a radar range gate meant, or do the authors actually mean that crystals and droplets can co-exist? I am not an expert, but my text-book understanding is that droplets will evaporate if crystals are in the immediate vicinity.*

This sentence referred to crystals and droplets existing within the radar range gate. However it is removed from the revised paper, to avoid confusing the reader.

*3818,15 It is advisable to avoid using the word 'significant' unless statistical significance is meant. Some use the word more freely, but to others it has a special meaning, i.e. that a significance test has been performed.*

Agree! The word 'significantly' is removed from this sentence.

*3827, 26-27 There is some word missing.*

***Up to*** 3000m; taken care of in the revised manuscript.

*3827,8 - 3828,20 This is where the selection procedure is described. However, I think it could and should be written more clearly, and it should include a short discussion of how the criteria possibly affect the end results. As a reader this is the kind of information that interests me.*

This is a very valid concern, and we would argue that some of this information is in fact presented; for example 40% of the profiles failed to match the cloud-top restriction because clouds were deep frontal clouds. For these, a change in cloud top criterion makes no difference; these are not the clouds we are after. Only 18% fail to be included because their top was higher than the limit. Moreover, some of the comparisons to the sounding data in the beginning of Section 3.1, also help in this context; these gives a slightly higher fraction of decoupled clouds, consistent with the conclusion that higher clouds are more often decoupled.

The problem is that these limits are imposed by hard constraints from the observations. We need to have the cloud base below the highest height of the scanning radiometer to be able to use that instrument, and we judged we need three range gates of cloud from the MMCR to define it as a “cloud layer”. It would have been great if we could have extended the top to include all stratocumulus clouds; that was only possible using the soundings, see above. These, on the other hand, are much fewer and so it becomes uncertain what any difference means. Along the same line of argument, we could not shrink the size of the MMCR range gates; instead including more levels would pose a real physical restriction on the analysis that would make it impossible to judge if any difference in the results is due to a sensitivity to the criteria or to the fact that we would view a different part of the dataset.

We have edited these paragraphs with the aim to enhance clarity.

*3831,9-18 This is where the hypothesis of the paper is presented. But it is well-hidden.*

This more a result of the analysis than a hypothesis and comes as a consequence of the results presented in Figure 5 and described in the paragraph above.

*Section 3.2, I did not understand the point of this* section.

This section is complementary to the previous in attempting to answer one of the main questions that initiated this study: what drives the cloud-surface coupling(?); the cloud dynamics or surface turbulence?

The section previous to this indicates that the cloud-induced turbulence rules the coupling. However, intuitively, one would expect that stronger surface fluxes would facilitate cloud-surface coupling. The statistics presented in this section suggest that there is no correlation between the surface fluxes and the coupling states, and this enforces the hypothesis that cloud-surface coupling is driven by the cloud and not by the surface.

The revised text has been added for clarity.

*3833,16 I would avoid using the word ‘confirms’ here. Maybe ‘supports’?*

OK, ‘confirms’ is replaced by ‘supports’

*3833,27-28 There is something wrong with the sentence.*

This sentence is replaced in the revised version of the manuscript. We imply here that if liquid is homogeneously distributed across the cloud layer, then instead of generating turbulence and mixing, the whole cloud will cool.

*3834,2-4 This is one place were a statistical significance test should be applied to see if the estimates are significantly different.*

Student’s t-test suggested that LWP estimates for stably- and neutrally- stratified clouds are significantly different; this information is included in the revised text.

*Section 3.4, I did not understand the point of this section.*

Cloud formation relies on CCN; moreover, the concentration of CCN has a strong impact on the optical properties of the cloud. Since one of the purposes of this study is to highlight the similarities and differences between coupled, decoupled and stable cases, a comparison for all available cloud properties (LWP, IWP as well as CCN) is of great relevance.

In this section the CCN concentrations for the three cloud mixing states are calculated and the results further support the findings of the previous section: (a) that coupled and decoupled clouds do not differ in cloud properties and (b) that stable cases are much thinner in optical depth compared to the coupled/decoupled cases.

*Section 3.5, Here a vertical normalization is applied, however for practical reasons the normalization is different for three categories of clouds. This makes them barely comparable, edging on directly confusing. I would suggest to reorganize this section and associated figures (12-20) such that first one goes through the coupled clouds, then decoupled clouds and finally stable clouds. Thereby, one could take the opportunity to see if some of the plots could be left out.*

The different normalization applied for different cases induces difficulties in comparing them, but unfortunately there is no other practical way we could use. Yet, taking into account that each layer (e.g. SML, cloud layer, etc) is independently normalized, it is possible to compare some general features (e.g. increasing/decreasing profile with height, range of magnitudes, etc.) within the same layer for different cases.

Thus, there are advantages in choosing this way of presenting the figures. While presenting all the different results for each class separately would have avoided the confusion related to the normalization, it would have made comparison of the different classes more difficult; hence a perfect choice is impossible and we argue that the way we presented these results is the optimal way.

Moreover, the figures of this section give a statistical overview of the boundary layer structure regarding each cloud state; no similar analysis has been done before. Thus, the presentation of the basic meteorological parameters (temperature, humidity, wind profiles) and the microphysical structure (radar moments) is important for the acquirement of the complete picture of the conditions that characterize each case of mixing state.

*3837,1-3 I didn't understand the rationale for this investigation?*

Plotting radar reflectivity for the three main states (Fig. 12) reveals a large scatter in values for the SML of decoupled cases. A possible explanation for this scatter is that there are large variations in SML depths among the decoupled profiles. To investigate this, the effects of SML depths on radar reflectivities at the decoupling height were examined (Fig. 13).

The results show relationships between radar reflectivities and SML depth, which motivated the separation of the decoupled cases in the two subcategories. Later, the fact that a separation based on the SML depth coincides with a separation based on the decoupling strength strengthens the motivation to continue with these subclasses, however, renaming “deep” and “shallow” to “strong” and “weak” decoupling.

*3838,20 What is the point of including another case? It mostly confuses the reader, and looking through the results the scientific outcome is very marginal. If the authors want to go with four classes, against my recommendation, they should do so throughout.*

The subdivision of deeper and more shallow decoupled clouds is not random; it comes from examining the data (Fig. 12 & 13). With an aim is to characterize the different coupling states, the large spread in the reflectivity in the SML for the decoupled clouds needs to be examined; from this examination it becomes clear that the deeper and more shallow SML's have different structures.

The rest of the discussion takes its cue from this. The structure of precipitation for the different cloud states and evaluating the hypothesis how evaporating/sublimating precipitation may promote the decoupling are important aspects of this paper.

The introduction of two subcategories reveals that they are indeed substantially different when it comes to the strength of the decoupling; this provides additional rationale for the sub-division. This realization could have come from examining the temperature profiles and noting a spread in decoupling strength, bypassing the discussion on SML depth. But that is not the way it happened. Noting that the more strongly decoupled states are also deeper is important to the discussion on how evaporation/sublimation can amplify the decoupling under certain circumstances (e.g. when precipitation falls in a relatively deep subcloud layer).

Thus, one of the main findings of this study is based on this separation. Moreover, this separation affects only the structure of the subcloud mixed layer; it doesn't impact the cloud interior and the surface turbulence, which is why it is only applied in section 3.5 where the vertical structure is examined. There is simply no point in introducing this separation in the previous sections, where the surface fluxes and cloud properties are investigated.

*3841,9-21 What is the point of presenting RH over ice?*

The main focus in this discussion is in the subcloud layer, where ice precipitation occurs. That is why RH over ice, instead over liquid, is chosen for the plot.

*3844,14-15 Consider rewriting 'by absorbing latent heat'.*

This has been rephrased in the revised manuscript.

*3845,17-20 I didn't understand the purpose of this sentence.*

This was an attempt to explain the lack of substantial ice in stable clouds. However, we agree that other processes may provide better explanations and thus, this sentence is removed from the revised text.

*3846,9 I didn't perceive the study as concerned with surface-cloud interactions. In fact, later it is concluded that the surface simply responds to cloud processes. Maybe the authors mean that they focus on cloud to sub-cloud layer coupling?*

The investigation of cloud-surface interactions is what motivated this study. The fact that coupling doesn't occur frequently, that the decoupled state is dominant and the fact that surface-driven turbulence does not play a large role is the outcome of the study. Thus the title is inspired by the purpose of this research; the result that the clouds are often decoupled from the surface has large implications for hypotheses on the life cycle of the clouds.

Finally, all comments regarding improvements in the scientific language and figures are considered in the revised manuscript.

**ANSWER TO REFEREE 3:**

*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:

(d) A statistical overview of the thermodynamic and microphysical structure of the different coupling states and their interactions with the surface fluxes.

(e) The study of stably-stratified clouds: their properties, characteristics and structure.

(f) 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:

(c) 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. 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.

(d) 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 $\Theta$ is plotted later in the analysis, instead of $\Theta_E$, 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 $\Theta_E$ 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 $\Theta_E$ profiles and classify clouds as coupled, decoupled or stable. The only defect of using $\Theta_E$ 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 $\Theta_E$ profile; despite the fact this case would be thermodynamically decoupled in T and $\Theta$ profiles, it would appear as coupled in $\Theta_E$. To ensure that such a case does not occur in our dataset, we plot $\Theta$ instead of $\Theta_E$ in Fig 15; this shows that the profiles we initially classified as coupled using $\Theta_E$ 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 $\Theta$ and $\Theta_E$.

*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 1$^{st}$ 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.