

ANSWER TO REFEREE 2:

We are grateful to this reviewer for a very thorough review and for posing many 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 reviewer's comments in *red italic*; our response follows below each reviewer comment. We hope that our comments and the changes in the revised manuscript are sufficient for this reviewer.

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-Jovicic 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 $\sim 1.5^{\circ}\text{C}$ (top-base) separating the surface layer from cloud-driven mixed layer. We consider a portion of the SML $\sim 100\text{m}$ 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: $dQ/dt = C_p/L_v * dT/dt \rightarrow dQ \sim 0.0005 \text{ kg/kg}$.

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

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 (1992) 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 where 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.