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Interactive comment on “The thermodynamic structure of summer Arctic stratocumulus and the dynamic coupling to the surface” by G. Sotiropoulou et al.

Anonymous Referee #2

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Review of ‘The thermodynamic structure of summer Arctic stratocumulus and the dynamic coupling to the surface’ by G. Sotiropoulou and co-authors for consideration in Atmospheric Chemistry and Physics.

In this manuscript the authors present data concerning the vertical structure of the cloud-topped boundary layer during an expedition to the central Arctic Ocean. It is found that the surface is decoupled from the cloud more often than not in terms of turbulent mixing processes. It is hypothesized that cooling associated with evaporating precipitation is responsible for the decoupling.

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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. 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. I felt that the three conclusions presented (3846-3847) were a reasonable condensate of the results, however, the analysis is much more comprehensive than it needs to be to support them. 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.

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.

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. Therefore the manuscript needs major revision. Below are some comments to aid in this process.

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Specific comments:

The abstract is a bit technical. Focus on the main finding.

3816,6 consider a different word than 'creates'

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

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

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).

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.

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.

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.

3818,18 Swap 'are particularly'.

3819,23 Consider deleting 'epsilon' as it is only used here.

3821,14 'adopt'

3823,16 Consider deleting 'vertically-pointing'

3826,2 'to the surface'

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3826,27 SML is already defined

3827,1 Maybe 'profiles with an inversion'?

3827,26-27 There is some word missing.

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.

3829,2 Consider replacing 'ones' with 'clouds'.

3829,29 'latter'

3831,9-18 This is where the hypothesis of the paper is presented. But it is well-hidden. Section 3.2, I did not understand the point of this section.

3833,16 I would avoid using the word 'confirms' here. Maybe 'supports'?

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

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

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

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 re-organize 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.

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

3838,20 What is the point of including another case? It mostly confuses the reader, and

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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.

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

3843,13 Maybe 'synoptic scale'?

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

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

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?

Figure 7 a+b) use same scale.

Figure 15, add a vertical zero-line in all panels.

References

Savic-Jovicic, V. and B. Stevens, 2008: The Structure and Mesoscale Organization of Precipitating Stratocumulus. *J. Atmos. Sci.*, 65, 1587–1605. doi: <http://dx.doi.org/10.1175/2007JAS2456.1>

Winton, M. Amplified Arctic climate change: What does surface albedo feedback have to do with it? *Geophys. Res. Lett.*, 33, L03701, doi:10.1029/2005GL025244, 2006.

Pithan, F. and T. Mauritsen. Arctic amplification dominated by temperature feedbacks in contemporary climate models. *Nature Geosci.* doi: 10.1038/NCEO2071 (2014).

Vial, J. et al. On the interpretation of inter-model spread in CMIP5 climate sensitivity estimates. *Clim. Dyn.* 41, 3339-3362 (2013).

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