

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

**Anonymous Referee #3**

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Review of ‘The thermodynamic structure of summer Arctic stratocumulus and the dynamic coupling to the surface’ by G. Sotiropoulou, J. Sedlar, M. Tjernström, M. D. Shupe, I. M. Brooks, and P. O. G. Persson

Understanding mixed-phase clouds in the high latitude/Arctic regions is an important (and highly uncertain) piece of the puzzle with regard to its role in the high latitude water and energy budget and their coupling to/impacts on melting Arctic sea ice. Any observational investigation of in situ data collected on these poorly understood clouds is very useful for the atmospheric science community (both for observationalists and numerical modelers). This paper is a thorough analysis of Arctic Summer Cloud Ocean

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Study (ASCOS) field campaign data that was taken with a wide array of cloud and atmospheric instrumentation. The authors characterize the observed mixed-phase cloud cases into a series of categories including those occurring in neutrally stratified atmospheres (with two sub-categories of coupled and decoupled from the surface) and stable stratified atmospheres. The neutrally stratified clouds tend to precipitate with much of the turbulence driven by radiative cooling from the cloud top and a minor (at best) role from the surface. In the case of stably stratified clouds, these tend to not precipitate, and are in fact solely composed of liquid droplets (no ice) much of the time. The authors go into significant levels of detail on the thermodynamic, radiation, dynamic, and hydrometeor environments of these different mixed-phase cloud types and come to some interesting conclusions about the processes at work.

As a non-specialist in this discipline (coming from a satellite cloud remote sensing background), I found this paper very interesting and educational, well written, thorough in its attention to detail, and convincing with regard to the conclusions arrived at. 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. 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 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? What is the new aspect of potential temperature that is so compelling? 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)? Are the authors underselling some of the work in the present study, i.e., are

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there other important differences with Shupe et al. (2013)?

Since the previous point is central to the importance of the manuscript, I would consider this manuscript requiring a major revision. However, it may not in fact require a major overhaul, perhaps some additional clarification would suffice. But I do think quantifying the overlap between the Shupe et al. (2013) and present study would add value to the present manuscript.

More specific comments:

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.

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.

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.

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

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?

p. 3835, lines 14-15: is additional support

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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?

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Interactive comment on Atmos. Chem. Phys. Discuss., 14, 3815, 2014.

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