

In this document, reviewer's comments and suggestions are reproduced as gray-shaded text, while item-by-item response follows in black.

Anonymous Referee #1

The manuscript describes the synoptic-scale meteorology and its climatic anomaly during the ASCOS field experiment in summer 2008 and compares key meteorological variables, their vertical structure and energy fluxes at the surface with similar observations obtained during three similar experiments during AOE-1996, SHEBA-1998 and AOE-2011. This is a worthwhile study and I recommend to accept it for publication after my below comments have been addressed by the authors.

We are very grateful to this reviewer for a careful review of our paper. We have tried to use many of the comments, although not all, for reasons that we explain in the item by item responses below.

In brief summary, we have edited the text in response to many of the comments, but we have *not* added any in-depth analysis of the synoptic scale differences between ASCOS and the other three expeditions; we explain below the rationale for this. Eight new sub-plots have been added; six in one new plot showing the mean and anomaly MSLP for the "other three" expeditions, to somewhat alleviate the lack of information on the synoptic meteorology for these, and two new sub-panels in the surface radiation plot, showing the atmospheric transmissivity and the surface albedo. In both cases text has been added to discuss these results. As an elaboration on the differences in baroclinicity between expedition, in the discussion of the median deep profiles of temperature and wind speed, we also calculated – and discuss very briefly – the maximum Eady-growth coefficients.

Major comments:

1. The motivation deals mainly with the role of the Arctic in global climate system but the authors are presenting snapshots over a relatively short time period and a small region along the trajectory. How this fits together? I recommend to discuss the value of the presented observations on shorter meteorological time scales.

Point well taken; although the original introduction does discuss the need for these types of observations, for example for model development, the revised manuscript has a more focused introduction, discussing the utility of short but detailed observational campaigns.

While one would like to be able to develop a pan-Arctic climatology of processes that deal with boundary-layer turbulence and clouds, this is not possible because of the lack of relevant observations. The SHEBA experiment provides the only available data covering an annual cycle with a sufficient degree of detail to understand some of the processes in this perspective. Only the three Oden-based experiments that are used in this study actually have the observations with similar detail. Several other experiments have also been carried out over the last several decades; there is also data from the Russian ice-drift stations and from the Tara expedition. But in most of these cases, the data either do not focus on summer, do not have sufficient cloud observations or are not detailed enough to be comparable.

The revised introduction better outlines the utility of shorter experiments, namely to study relevant processes to aid model development and development of satellite retrievals. But to realize this potential, it is also relevant to ask the question how representative these experiments are - the second objective of this manuscript. This does not mean, however, that we aim to explain in detail the causes for differences and similarities with the earlier summers; more on this below.

2. In the introduction the authors claim the need to interpret process-level observations within the context of larger-scale atmospheric circulation. Therefore contour plots of means and anomalies should be provided with a characterization of 2m temperature, 10 m wind, MSLP and 850 and 500 hPa geopotential heights for all four experiments.

While we agree this is a somewhat valid point, we also feel that this suggestion falls outside of the scope of the current paper.

The paper has two objectives. The first is to document and analyze the meteorological conditions during ASCOS for the benefit of those that want to use the large dataset that was collected during ASCOS, such as boundary-layer meteorology, atmospheric chemistry and aerosols, cloud microphysics etc. The second objective is to address the issue of representativity; to provide some insight into how representative the meteorological conditions were during ASCOS. This goes directly back to the utility of the observations (see above). If the data is to be used to inform model development, it needs to be made certain that ASCOS was not conducted during a very anomalous year. The title of the paper has been modified to make this order of priority clearer, and the sub-title has been dropped.

Including a detailed analysis of the synoptic scale properties for the three other experiments along the suggested lines would add at least nine new figures with a total of 45 new sub-panels and probably at least three pages of text; this paper is long already with a quite an extensive analysis of ASCOS, with many figures and even more sub-plots.

While we think this is a good suggestion for another paper, we also feel this should be written carefully, focusing on the comparisons/contrasting properties of the large scale during the four different experiments; not as a short add-on squeezed into an already quite long and extensive paper. This would become prohibitively long and no one would care to read it. To balance, we have added one new six-panel plot with the mean and anomaly MSLP for the “three other” experiments, along with a few words in the text.

The 10-m reanalysis wind fields are not included in either the revised or the original manuscript as we feel the quality of this reanalysis product to be somewhat questionable.

3. An assessment of the synoptical (spatial and temporal) characteristics of all four experiments is needed. How similar were the atmospheric and sea ice conditions during the four experiments with respect to climatological conditions? Were the presented data collected at the experiments field site representative of a larger spatial area and if how big is the correlation radius?

Concerning the assessment of the synoptic conditions during the three “other” experiments, see discussion above.

The property of the sea ice in the different experiments is described in Figure 3; the revised manuscript has some more text on this particular issue from what was documented during the different expeditions, also including an analysis of the surface albedo in the “surface-energy section” of the manuscript.

The issue of the spatial representativity of single column observations is something we believe has to be assessed with modeling experiments. Without additional substantial data at other locations around the field site, like was done in SHEBA, it is not possible to make this analysis from the observations. Even then, one could comment that surprisingly little has been published on the spatial homogeneity from SHEBA using the so called PAM-stations.

This problem is also exacerbated by the fact that we have utilized observations during both the ice-drift component of the experiments and while navigating in the ice. The rationale for this is better outlined in the new data discussion but is basically weighting the pros and cons

of expanding the amounts of data or using a stricter data policy at the cost of less statistical significance. With several year's worth of data at a single site, this would have been easy; but there are only four expeditions, of which three are less than two months, at different locations during different time periods also interspersed with several years in between.

4. The apparent differences in figures 10-15 between the four experiments should be discussed with respect to the baroclinic situations during these experiments. I suggest to discuss the baroclinic wind shear and the Brunt-Väisälä frequency.

The revised text has been improved in this respect. We also calculated the "bulk" maximum Eady-instability growth rate from the soundings and discuss this in the new text to address the baroclinic aspects.

5. The presented figures deliver important informations, but the origin of the differences in Figures 10-19 with respect to sea ice/ocean state and synoptic-scale forcing/ baroclinicity needs to be quantified.

As has been commented above, the purpose of this paper is *not* to pinpoint the reasons for differences or similarities; it is to investigate if there are any, rather than why. We feel that adding the suggested analysis to an already long paper would be a sub-optimal strategy. It would be far better to make such an analysis in a separate paper that can be devoted to this aspect.

Still, we added a few more comments on this, referring back to the new figure on MSLP (see above). As a side-comment we do note that the results between the four different expeditions in many respects are surprisingly similar, rather than the opposite.

6. What this study can contribute to understand and quantify feedbacks in the vertical under the influence of varying synoptical forcing conditions and different lower boundary effects connected to sea ice or open ocean?

We are not quite sure what the question here is, but we feel that the more process-level field-experiment data there is, the better chance we have of understanding some of the processes that are poorly described in current models, for climate as well as for weather forecasting, and thus to improve Arctic modeling. Also, the appearance - or not - by specific phenomena in this under-sampled regions can be better quantified by more experiments, even if they are short.

If by "this study" the reviewer means ASCOS, we have contributed a better understanding of mixed-phase clouds, optically thin summer clouds, summer aerosol depletion and nucleation events, cloud-induced mixing, cloud/aerosol/surface interaction and coupling and details of the processes that lead to the initial autumn freeze. Many of the observations have revealed previously unknown, or at least not properly observed, features and several papers have already been published on such aspects.

If the reviewer by "this study" refers to this paper (i.e. a comparison of late summer surface-cloud-boundary-layer observations from 4 campaigns), the objectives of this paper are discussed in the response to reviewer comments #2 & 3.

Minor comments:

1) Page 9, LN 22, wind analyses at 10 m?

Yes; the new text has been changed to reflect this.

2) Page 10, LN 11, trajectories based on ECMWF operational analyses with which time, vertical and horizontal resolution?

These trajectories were calculated using HYSPLIT and the NCEP GDAS meteorology data; this is now clearly mentioned in the new text and referenced. We suggest that HYSPLIT is a well established tool used by very many scientists. The output time resolution of the trajectories are 6-hourly, but exactly how the GDAS data is used to generate this output is hidden in the background somewhere on the HYSPLIT web page, as is common with public domain software and we now refer the reader to this resource.

3) Page 10, LN 27, explain subjective analysis of passing frontal disturbances.

We don't quite understand this request. The original text states "using the *time-rate-of-change* and the *slope with height* of Θ_e , aided by the MMCR cloud reflectivity"; there isn't really much more to be said.

Rapid temporal changes in Θ_e outline frontal structures; warm or cold fronts are interpreted by the slope of these "interfaces". The much higher time resolved MMCR data was used to shift details in these fronts, assuming the frontal clouds correlate with the front itself. MMCR data was also used to scan for frontal structures. While we feel that this was already well encapsulated in the current text, however, we did edit the text on this slightly and hope that this is sufficient. Other suggestions are welcome!