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

Anonymous Referee #2

The manuscript will make an important contribution to the ASCOS Special Issue. Evaluation of the ASCOS observations with respect to synoptic-scale meteorology and inter-annual differences will put the whole data set in a broader context. Reading this manuscript will be very important for all those involved with more focused studied based on ASCOS data. In general, the manuscript is well written and progresses logically. The figures are of a high quality with an intelligent design to present a lot of information in a single plot. It could, however, be improved in some respects, which I explain below.

We are grateful to this reviewer for the positive and constructive review and for a careful reading 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. Responding to comments on baroclinicity, 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. It is important to evaluate the synoptic-scale conditions during ASCOS. I particularly liked the summary presented on page 24. What I am missing is a brief summary on how the synoptic-scale conditions differed between ASCOS, AOE-2001, AOE-96, and SHEBA.

The revised manuscript contains one new figure showing the mean and anomaly MSLP for the three other experiments along with a short discussion. Other than this, an in-depth discussion on the synoptic scale meteorology is beyond the scope of this 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.

We feel it would be better to save this discussion for a separate paper, where it could be developed much better. Including a more detailed analysis of the synoptic scale properties for all experiments would add at least nine new figures, with a total of 45 new sub-panels, and probably at least three pages of new text. This paper is already quite long with many figures and even more sub-plots. We feel that this should not be squeezed in shortly into an already

quite long and extensive paper, that would become prohibitively long and thus no one would care to read it.

2. The manuscript includes a plenty of statistical material comparing the four expeditions. The presentation of so much statistics would be better justified, if sufficient attention is paid also on the physical interpretation of the differences. This can probably be partly achieved by the summary I suggest in item 1 above, but also other aspects contributing to the differences should be considered. I recommend to somewhat shorten the text (Sections 4 and 5) that more or less directly explains what is seen in the figures, and adding more physical interpretation.

The original text in Section 4 has been shorted in the revised manuscript and some more text on the physics has been added as suggested.

However, as stated above, the purpose of this paper was never to discuss the differences and similarities between these four expeditions in detail. It was to describe conditions during ASCOS and to explore if there are similarities and differences to previous similar expeditions.

3. The comparison of ASCOS results against AOE-2001, AOE-96, and SHEBA is good. The comparisons are extensive and detailed; there is no reason to add much in the same level of detail. However, there have been many more meteorological field expeditions in the Arctic Ocean in summer, which are not mentioned in the manuscript, although they have included analogous, although less extensive, mutual comparisons. For example, our knowledge on climatology of temperature inversions over the Arctic Ocean has so far relied much on the studies by Serreze et al. (1992) and Kahl et al. (1996), based on Russian drifting station data. The ASCOS results should be evaluated also with respect to these classical studies. In addition, Lüpkes et al. (2010, GRL) compared meteorological observations from three RV Polarstern cruises in the Arctic Ocean in August 1996, 2001, and 2007; two of the summers were same as in the Oden cruises. Vihma et al. (2008, GRL) compared the meteorological observations made at Tara in summer 2007 against the SHEBA and Russian drifting station data. The main outcome of these and other previous studies should be summarized, either in the Summary and Conclusions section or in a separate section before it.

References to the Russian drift stations and to Tara were already given in the original text; this is now expanded to include the Polarstern & Tara results, and the work by Lupkes and Vihmo is added, as well as the older publications by Serreze and Kahl.

However, when it comes to detailed observations of atmospheric boundary-layer and cloud physics, none of these expeditions come even close to what is available from the four expeditions used in this paper; of the latter even AOE-96 is somewhat of a “border line” case in this respect since no cloud radar was deployed.

While the Russian ice drift stations have a long record of soundings, there are almost no boundary-layer or cloud observations (some eddy-correlation surface-flux instrumentation and a ceilometer has been included in the latest NP deployments). And while Tara has some surface flux observations and tethered soundings over the summer, there were no cloud observations, the total number of soundings was limited and they usually ended well below 2 km.

Therefore, a direct comparison using Tara and NP data would be different than that presented here. Both the Vihma and the Lubkes papers also contain comparisons with ERA reanalysis as a main item; the Vihma paper additionally comments on potential changes over time, using SHEBA, NP and Tara data; none of this is addressed in the present paper.

4. Comparisons between the results of the Oden expeditions and SHEBA suffer from the fact that SHEBA was drifting at lower latitudes. Therefore some conclusions remain somewhat

open. This is the case especially for solar radiation. Adding a comparison of the atmospheric transmissivity for solar radiation would therefore be useful.

Correct, and this fact is discussed in the original text; this is a pretty obvious difference. It is also discussed in the Vihma et al. paper mentioned above. The revised manuscript includes an analysis on the atmospheric transmissivity from all four expeditions and the surface albedo from those three with instruments on the ice.

5. Cloud-top radiative cooling is mentioned several times in the manuscript. As it is not any new finding, some comparisons against previous studies in the Arctic should be presented.

Cloud top cooling is rarely observed directly, but the turbulence is causes can be sometimes be observed or at least indicated if detailed boundary-layer observations are available. As far as we are aware, ASCOS and AOE-2001 provide the only observations over the central Arctic Ocean with sufficiently detailed PBL-profiling along with cloud observations to make such an analysis; SHEBA, although having superb cloud observations, lack the PBL-profiling instruments.

In fact, while a search for papers on "Cloud-top cooling" on the *Web of Science* generated 232 hits, adding "Arctic" to the search reduced that number to 25. Most of the latter discuss results from terrestrial or coastal Arctic, many are from airborne "case-study" experiments, and more than half are modeling papers while none discuss results from the central Arctic Ocean.

A short new discussion is added where the ASCOS conditions are compared to those during the AOE-2001. This is interesting because of the similarities and differences and how they appear when analyzing the temperature profiles from the scanning radiometer. In both cases, cloud-top cooling and buoyancy-generated turbulence dominates the surface based shear-driven turbulence, but in AOE-2001 the clouds where lower and the cloud layer appears to have been more connected to the surface-driven PBL; in ASCOS this was not the case, at least not as clearly. Instead the profiles appear more similar to that of the transition in the trade wind region between sub-tropical stratocumulus and shallow convection.

Minor comments:

P8, line 2: data are

OK, changed.

P8, line10: drop comma from the end of line

OK, changed.

P15, line 21: heights, strengths and occurrences of low-level jets may have ...

OK, changed.

P15, line 27: Add period after "between"

OK, done.

P19: the title of Section 5 is misleading: nothing is written about ice drift, i.e. kinematics and dynamics of sea ice motion.

Sorry, this was a misunderstanding. We are referring to the "ice drift" as the portion (or time period) of ASCOS when the icebreaker was moored to and drifted with the ice, not as discussing the drift of the ice itself.

In the revised text, this section title is changed from “of the … ice drift” to “during the … ice drift”. Hope that makes things clearer.