Response to the comments by Reviewer 3

We thank the reviewer for her constructive comments. The comments are copied below and our responses are written in red.

General comments

This review paper compiled together an impressive amount of recent literature about small-scale processes related to the Arctic Ocean climate, including troposphere and its boundary layer, snow and sea ice, ocean, and their interfaces. The goal of the paper is to summarize recent advances in our understanding of small-scale processes mostly based on SHEBA and later field campaigns organized during and after the IPY 2007-2008.

It is a very timely paper highlighting many important recent advances concerning the Arctic Ocean physical system. Compilation of all this amount of knowledge together will surely help both our understanding of the Arctic climate and sea ice processes by considering the system as a whole with intrinsic interaction among its components. SHEBA campaign was probably the first showing the importance of studying Arctic system in its entirety including ocean, ice/snow and atmospheric processes simultaneously and understanding interconnections. This idea forms a fundament for the present paper and has a potential of demonstrating a large step in understanding the Arctic climate during the recent years. Although the article is very long, I don't see a problem for the readers to focus only on sections of interest and then on the interaction (concluding sections). Reading this paper will also help setting priorities for further research including better interaction among researchers from different disciplines.

However, the paper needs some major revisions before being considered for publication. While some sections have a very focused and consistent text, others contain a lot of scattered information, sometimes contradictory, jumping from topic to topic (particularly section 2). There are sometimes contradictory and returning statements on the same subject, and abrupt conclusions without proper mechanism explanation. Probably necessity to cover many topics does not leave space for deeper discussions of physical processes. Still I find that in some sections the authors managed to keep the discussion short and focused highlighting also connections among processes, while other sections are too lengthy and sometimes inconsistent.

We admit that our original manuscript suffered from the above-mentioned weaknesses. We have improved the text throughout the manuscript, but particularly in Sections 2, 4, 5, and 6.

The paper also contains a lot of text versus only a few figures - almost all as schematics. Figure 7 is an example of a very helpful and clear schematic, well thought over, including all key processes with the links among them, all abbreviations explained and the schematic discussed in the text. Schematic presented in Fig. 6 on the other hand is too vague, and lacking explanations - neither in the caption, nor in the section text. Fig. 3 concerns only mixed-phase clouds while referred in the text to as explaining all cloud processes, misses some key micro-physical processes in cloud physics (aerosols and CCN/IN for example), needs explanation of color coding and abbreviations.

We have removed the previous Figures 4 and 6, as Reviewers 1 and 2 criticised that there were too many box diagrams. We have added several more references to the present Figure 4, explaining that it focuses on mixed-phase clouds only and also explained the role of aerosols.

Discussion of the feedbacks is somewhat hidden within sections. Eg, section 3.1.4. gives an interesting and comprehensive discussion of the surface albedo feedback and its interaction with water vapor, clouds, precipitation, aerosols and mechanical processes. Also, section 4.3 "Diapycnal mixing" includes a paragraph discussing the role of the oceanic and atmospheric fluxes on sea ice growth/melt (p. 32762, " The oceanic heat is found to affect the sea ice growth and melt..."). At the same time the section "Cross-disciplinary aspects", where I was expecting feedbacks and interactions to be discussed in details, simply gives a list of possible feedbacks. I suggest combining feedbacks and interactions at the interfaces into the concluding section with a focus on sea ice. One of the important conclusions from this paper can be compiling the knowledge about processes leading to the Arctic sea ice melt.

Thank you for the good suggestion! We have modified the manuscript with a separate Section 5.3 on feedbacks.

I recommend the following substantial revisions before considering publishing this manuscript:

1) critically revising the text making it more focused and consistent
We have tried to improve the text to make it more focused and consistent. There are major revisions especially in Sections 2, 4, 5 and 6.

2) including figures illustrating key points raised in each section (see eg, Bromwich et al. 2012 "Tropospheric clouds in Antarctica", Rev. Geophys.)
We have added five new figures (Figures 2, 6, 9, 10, and 12) and improved the old ones (especially Figures 1 and 4). The Bromwich et al. paper was indeed a good example.

3) improving the schematics - include discussion of the processes shown in the schematics in the text, providing all necessary information (including abbreviations) in the captions.
We have improved the schematics in the present Figures 1 and 4 and removed the previous Figures 4 and 6, as Reviewers 1 and 2 thought that there were too many schematics.

Specific comments

1.Abstract: "Uncertainty in the parameterization of small-scale processes continues to be among the largest challenges facing climate modeling, and nowhere is this more true than in the Arctic." - I find this sentence a bit "Arctic biased" and "nowhere is this more true" is a strong statement so I suggest rephrasing this part keeping in mind that there are other equally challenging regions and important processes for both measurements and modeling (eg Antarctic climate or carbon cycle related to the African equatorial forest...).

We have rephrased the sentence. (Now we refer in Section 6 to a study that shows that the model uncertainty is more important in high than low latitudes.)

2.Intro: "The relative importance of the above- mentioned processes is not well known, with a recent study finding a dominating role of the water-vapor feedback (Mauritsen et al., 2013)." - it is not clear if Mauritsen is about Arctic or global climate

We have clarified the sentence and dropped the reference to Mauritsen et al., who addressed global feedbacks.

3. Fig. 1: seems to be very basic/incomplete - I suggest either to remove it or modify it including the most important processes and interactions discussed in the paper. The title of the figure is "Simplified schematic vertical profiles of temperature, air humidity, and ocean salinity in the marine Arctic climate system.", while it shows also radiative fluxes and turbulence. If the goal is to show the vertical gradients and related processes - then why not show horizontal heat and moisture advection, which is important for the temperature and humidity inversions. In its present state the figure is more confusing. Regarding atmospheric processes, for example, it shows only the cloudy state - what about the clear-sky cold regime? I suppose the green arrows on the right show LW fluxes during clear-sky - it is strange to see them with the same length as for the cloudy sky. Vertical profiles and corresponding flux relative magnitudes (affecting also turbulence) change substantially between the two atmospheric states - cloudy and clear-sky (see Stramler et al. 2011 for example).

We have improved Figure 1 including the most important processes and interactions discussed in the paper, and clarifying the figure legend.

4. p. 32707: " Clouds absorb and scatter solar shortwave radiation, and snow cover strongly reflects solar radiation, whereas sea ice has a lower albedo, and the ocean absorbs significant amounts of solar radiation, but only through the ice-free areas and very thin ice (Perovich et al., 2007a, b)." - the sentence is too long (suggest breaking into two)

We have simplified the sentence.

5. it is not clear if the reference by (Steeneveld et al., 2010) refers to only the last part or the entire large sentence

Only to the last part; we have clarified the sentence.

6. p. 32710: "observations of liquid water present in clouds at temperatures down to -34°C during SHEBA came as a major surprise to the science community (Beesley et al., 2000; Intrieri et al., 2002)." - this is an overstatement. Existence of supercooled liquid down to -34°C was not a surprise to the scientific community, but rather it was a question of how to parameterize ice/liquid fraction of mixed-phase clouds in GCMs. One of the problems was that every model was using different temperature ranges, some models down to -40°C (eg, Gorodetskaya, I. V., L.-B. Tremblay, B. Liepert, M. A. Cane, and R. I. Cullather, 2008: The influence of cloud and surface properties on the Arctic Ocean shortwave radiation budget in coupled models. J. Climate, 21, 866–882.)

We have modified the sentence according to the reviewer's suggestion. Now it reads: "Also, the common presence of mixed-phase clouds in the Arctic marks a drastic difference from lower latitudes; observations of liquid water present in clouds at temperatures down to -34°C during SHEBA (Beesley et al., 2000; Intrieri et al., 2002) demonstrated the need to develop better parameterization schemes for the ice and liquid water fractions (Gorodetskaya et al., 2008)."

7. pp. 32710-32711: description of field campaigns focuses only on one campaign DAMOCLES and goes into unnecessary details (why vessel names matter?). Any other important campaigns? As this is a review paper based on field campaigns, it will be helpful to include a table summarizing these campaigns (name, date, location, measured processes) and a map of the Arctic Ocean with marked locations of these campaigns, ship measurements, etc.

In fact, DAMOCLES was not one campaign; it was a large project that included many different field campaigns during 2005-2009. The reason for the attention to DAMOCLES is that the manuscript is submitted to the DAMOCLES Special Issue. We have now clarified this, but also dropped some unnecessary details, such as vessel names. We agree that a map of all campaigns would be very nice, but we found it too difficult to add. Our focus is on work made after the start of the IPY and there have simply been too many field campaigns to be presented in a single map.

Further I give some specific comments concerning mostly section 2, which I find needs serious revision. section 2. Atmosphere:

8. The way of presenting literature overview is not easy to follow and sometimes statements are controversial, eg: "... in SHEBA data surface inversions were most common in winter and autumn, accounting for roughly 50 % of the cases, while near-neutral stratification completely dominates in summer, when stable cases are almost nonexistent." and a bit later it says: "Raddatz et al. (2011) found similar temperature inversion frequencies for a Canadian polynya region, whereas Tjernström and Graversen (2009) reported, based on the year-long SHEBA experiment, that the inversions are practically always present in the central Arctic."

We have reworded the sentences, clarifying the difference between the boundary layer and the inversion layer.

9. "The frequency, depth, and strength of temperature inversions have been found to correlate positively (among each other? or with which parameter?) both spatially and temporally, and correlate negatively with the surface temperature (Devasthale et al., 2010; Zhang et al., 2011)." I suggest rephrasing making it clearer that all three are positively correlated among each other as found by Zhang et al. Also Devasthale et al. 2010 refers to Pavelsky et al. (2010) who "recently showed that the inversion strength and sea ice concentration are tightly correlated".

We have clarified the sentence and made the addition as suggested.

10. Here two contradictory statements need to be supported by explanations: "Vihma et al. (2011) reported that compared to temperature inversions, humidity inversions on average had their base at a higher level and were thicker than temperature inversions." ... "On the other hand, humidity inversions have been found to coincide with temperature inversions (Wetzel and Brummer, 2011; Sedlar et al., 2012; Tjernström et al., 2012)" - so why observations differ?

Now we explicitly state that different measurement campaigns have yielded different results, and present potential reasons for these differences. We would also like to stress that there is nothing untoward or strange obtaining different results from observations at different locations, over different years and of different lengths of the observation periods. Only more data can help here and it is our intention that pointing these things out can help focus observations in the future.

11. It seems to me confusing to put together various simplified statements trying to generalize quite complicated mechanisms. For example, the following statements: "Bintanja et al. (2011) demonstrated that atmospheric cooling efficiency decreases markedly with temperature inversion strength, which means that the surface is warmed by temperature inversions. Boé et al. (2009) obtained somewhat contradicting results for the surface temperature of the open ocean, but they too came to the conclusion that a strong temperature inversion tends to increase the near-surface surface air temperature via longwave radiation." To my opinion, these two papers are somewhat

misinterpreted here: main conclusion of Bintanja et al. 2011 indeed was that the near-surface temperature inversion damps the infrared cooling to space, however not because the surface is warmed by the temperature inversions. Rather the surface warming is not compensated by the radiative loss to space as the latter is largely controlled by the layers where the temperature and humidity inversion peaks are located. Then, while referring to Boé et al. paper, the "near -surface air temperature" or "surface temperature" are mixed together making it incomprehensible (or was it a typo). Boé et al. (2009) refers to the oceanic temperature of the mixed layer, and not the near-surface air temperature. And their main conclusion was that the extra heat stored in the mixed-ocean and increasing its surface temperature is not radiated back to space efficiently due to the temperature inversions. So the conclusions of Boé et al. and Bintanja et al. are similar and both do not refer to the increased LW down to the surface but rather damping of the cooling of the surface due to the association of the radiatively important layer with the inversion peak located above the surface.

Thank you for the valuable clarification! We have modified the text accordingly.

Section 2.2.1 Cloud physics 12. "An obvious connection between cloud phase and atmospheric temperature is present. However, cloud liquid water has been observed at temperatures below -34^{o}C (Intrieri et al., 2002). In fact, MPS are often the preferential cloud class when temperatures range between -15 to near 0^{o}C (Shupe, 2011; de Boer et al., 2009)." - these statements should be better linked

We have linked the statements more clearly: “An obvious connection between cloud phase and atmospheric temperature is present. MPS clouds are often the preferential cloud class when temperatures range between -15 to near 0□C (Shupe, 2011; de Boer et al., 2009), but liquid water has been observed in clouds at temperatures as low as below -34^{o}C (Intrieri et al., 2002).”

13. "If RHliq becomes sub-saturated in the presence of ice crystals, liquid droplets must evaporate following the WBF process, causing rapid depositional ice growth and cloud layer glaciation." As shown by Korolev 2007 ("Limitations of the Wegener–Bergeron–Findeisen Mechanism in the Evolution of Mixed-Phase Clouds", J. Atmos. Sci 64), WBF process depends on specific local thermodynamic conditions, and other processes involving simultaneous growth/evaporation of ice and liquid can maintain mixed-phase clouds in equilibrium. Later, the authors come back to this topic stating that "The key difference in the Arctic is the presence of liquid and ice simultaneously." further explained with in-cloud turbulence. This leaves it unclear to the reader which message the authors want to convey - rapid conversion of liquid to ice following WBF or their co-existence. This should be better discussed and linked as these are among the major recent advancements in understanding mixed-phase cloud microphysics.

We have modified the text to make the message clearer. In short, we do not see any contradiction here; the WBF process controls what the cloud does locally, given a certain (e.g. parcel) condition, for example RH. This is, on the other hand, controlled by dynamics such as turbulence (cloud scale motions). Hence the turbulence provides the flux of moisture that sets the stage in terms of RH, but the presence of liquid, on the other hand, drives the cloud-top cooling that drives the small scale dynamics. They are therefore interdependent; without the liquid there wouldn’t be enough moisture to form the liquid layer since there wouldn’t be any transport to balance the precipitation; clouds would glaciate and fall out of the sky.

14. The above paragraph ends with a conclusion that the cloud-surface coupling depends on the cloud processes, rather than near-surface turbulence, and the existence of bi-modality in the

boundary layer structure depending on cloud presence/properties. A more in-depth explanation of mechanisms here is needed to clarify this important connection. Also I suggest including a reference to the work by Stramler et al. 2011 (Stramler, Del Genio, Rossow, 2011: Synoptically Driven Arctic Winter States. J. Climate, 24, 1747–1762), who described in details the synoptic influence and cloud properties causing the bimodal nature of the Arctic ocean–ice–snow–atmosphere column. And a connection is needed to the earlier statement that the surface-based humidity inversions maintain mixed-phase clouds and their decoupling from the surface.

We have added text to make our argument clearer; however, the Stramler reference, although interesting in and by itself, is actually not appropriate here.

What they discuss is a bi-modality due to having or not having clouds; the effect of the clouds in the surface energy balance and hence on surface temperature. What we discuss here is cloudy cases only and the bi-modality arising from the cloud being connected to the surface or not, which is a different feature.

15. One sentence in this section refers to schematic 3: " The difficulties in modelling clouds over the Arctic are related to the numerous interactive processes, schematically illustrated in Fig. 3.". This is the only figure for Cloud Physics section. What do we learn from this schematic? What are particular advances in our understanding of clouds? The figure is not discussed in the text. Moreover, the figure includes only mixed-phase clouds, ignoring other cloud/fog types occurring in the Arctic and their importance for surface energy budget and precipitation (ice-only clouds, liquid-only clouds, ice fog...). If this is because mixed-phase clouds are found very common, and still the authors acknowledge that during winter and early spring (thus at least half of the year) ice-only clouds dominate. However, their importance is overall ignored in this review paper, while advances in their understanding have been also achieved since SHEBA and other campaigns. Finally, abbreviations used in schematic need explanation.

We have improved Figure 4 (previous Figure 3) and attempted to use it more and reference to it throughout the text. The text is also edited to make clear that this text mostly deals with mixed-phase, or optically thin, low-level clouds. Not only are these the most common cloud type generally, but also those with the largest impact on the surface energy balance and hence on the rest of the Arctic climate system.

Finally the statement on ice clouds is also modified; there is a period in winter and spring when ice-only clouds occur somewhat more often than MPS, but the difference is of the order of 10-15% and ice-only cloud occurrence is never dominating except in December at SHEBA; typical occurrence of ice-only clouds is <50%.

16. Some theoretical conclusions based on other literature are stated abruptly without referring to the mechanism behind, for example: "The local net temperature tendency from latent heat release [due to ice growth] is generally smaller than radiative cooling from liquid cloud top (Harrington et al., 1999). Thus cloud droplets can persist (disregarding large-scale controls such as subsidence, frontal passages, etc.) as long as a moisture source is present." - It is not clear how this conclusion about persistence of liquid was drawn based on the previous sentence. A full description of the mechanism should be included - that cloud top cooling helps production of vertical motions, which in turn drive the condensation/evaporation processes - as shown by Harrington et al. (1999)

We have tried to edit the text to be clearer. Basically what we argue here is that in terms of individual temperature controlling processes, nothing even comes close to the effect of cloud top cooling. If not balanced (mainly by mixing) the cloud top temperature could easily drop by > 50 K per day. Hence as long as mixing provides moisture, cloud droplets will continue to form to replace the condensate precipitating out and will not be evaporated by any other mechanism.

17. p. 32727: "Depending on the relative strength of in-cloud turbulence production and that driven by surface processes, the cloud-induced turbulent eddies may penetrate to the surface, or not; Tjernström (2007) suggested that most of the boundary-layer turbulence is in fact generated by the boundary-layer clouds, at least in summer." - sentence needs rephrasing

We have clarified the text. Now it reads: "Tjernström (2007) suggested that most of the boundary-layer turbulence in the Arctic is in fact generated by boundary-layer clouds, at least in summer. If the in-cloud turbulence production is strong and stratification below the cloud layer is weak, the cloud-induced turbulent eddies may penetrate to the surface, hence affecting the surface fluxes of momentum, heat, and moisture (Figure 4)."

18. I disagree with the statement about the temperature dependence on p. 32730: "Historically, models typically distinguish between cloud liquid and ice based only on temperature and thus fail to maintain liquid in very cold winter clouds (e.g. Beesley et al., 2000)". This statement generalizes all models, but in fact is based only on one paper by Beesley et al, 2000, which is about ECMWF model. Distinguishing between cloud liquid and ice based only on temperatures doesn't mean necessarily lack of liquid at very cold temperatures if the temperature range for ice/liquid partitioning used in a model extends down to these cold temperatures. There are several GCMs, which simulate too much liquid at low temperatures as shown for example by Gorodetskaya et al. 2008. mentioned above.

We have modified that sentence. We do believe that up until only a few years ago, the majority of weather forecast models, including the best model in the world (ECMWF/IFS) and a also some climate models had this set-up (e.g. ECHAM & HIRHAM). Admittedly, climate modelling has been ahead of weather forecasting in this sense, mostly because of the time-critical aspects of the latter.

19. Some paragraphs appear without any connection to the previous text, for example on p. 32729, paragraph 20 about droplet size goes without any connection to the previous paragraph about aerosols.

We would have to agree with this point. This section is shortened, edited and moved to where it merges better with other background information, earlier in this section.

Or also in section on meso-scale cyclones on p. 32737 - explanation of the mechanism in 1st paragraph is dropped, while it would be logical to continue, i.e. move paragraph 25 after the sentence " In reality most polar mesoscale cyclones have a mixture of these forcing mechanisms at different stages of their life cycle."

We have kept section 2.3.2 in the same order as the final paragraph provides the links to sea-ice and ocean processes, which are addressed right after. The explanations of mechanisms paragraph is very brief, but appropriate references are provided for readers who wish to find out more.

20. I find there are too many statements, which need further explanations. Eg, on p. 32730 " "...de Boer et al. (2011) find evidence that liquid saturation occurs prior to ice crystal development even in a supersaturated environment with respect to ice. The authors suggest that ice nucleation

mechanisms in Arctic MPS thus tend to be controlled by processes that rely on the presence of liquid condensate." - leaves a question so which exactly processes control ice nucleation that were found by Boer et al (2011)?

This section was slightly rewritten to clarify but with all due respect, for details the reviewer would have to ask the authors of that paper about more information. The present paper is a review paper, and many of the things discussed here are taken from other journal papers and summarized; there is not room for lengthy discussion about each finding.

21. I find it missing a discussion about the ice fog formation in the Arctic and its relationship to temperature and humidity inversions - see Gultepe et al. "Ice fog in the Arctic during Fram-Ice Fog project: Aviation and nowcasting applications", Bull. Amer. Meteorol. Soc., 2013 doi: http://dx.doi.org/10.1175/BAMS-D-11-00071.1

As is now made clear in the early part of the cloud section, this section focusses on mixed-phase stratocumulus. Although fog is frequent, also in summer in fact (see Tjernström et al. 2012), we feel that it does not have the same strong impact on the Arctic climate system, and it is not as frequent as the mixed-phase clouds. Surely worthy of a paper by itself, but there is not space for everything in this paper.

p 32771: " too little communication between basic researchers and large-scale modellers," suggest rephrasing to "basic researchers" to "observationalists" is this is what the authors meant

Changed as suggested

p. 32772 I suggest to include also several recent papers on the connection between the Arctic sea ice and snow melt and extreme weather events in middle latitudes: Francis, J. A. and S. J. Vavrus, 2012: Evidence Linking Arctic Amplification to Extreme Weather in Mid-Latitudes, Geophys. Res. Lett., Vol. 39, L06801, doi:10.1029/2012GL051000 Tang, Q., X. Zhang, X. Yang, and J. A. Francis, 2013: Cold winter extremes in northern continents linked to Arctic sea ice loss.Environ. Res. Lett., 8, 014036. Tang, Q., X. Zhang, X. Yang, and J. A. Francis, 2014: Extreme summer weather in northern mid-latitudes linked to a vanishing cryosphere. Nature Climate Change, 4, 45–50, doi:10.1038/nclimate2065

There have been so many recent papers on this issue that we prefer to cite two recent reviews: Walsh (2014) and Vihma (2014). In addition, a few other papers are cited in this paragraph, because they demonstrate the role of ABL processes in modifying the synoptic and large-scale circulation, i.e., these papers show the link between our manuscript on small-scale processes and the effects of Arctic sea ice decline on mid-latitude weather.

22. Also, a list of acronyms used in the entire paper will be helpful
Good suggestion. We have added a Table.

Technical corrections
p. 32707: "Compared to a dry atmosphere, the ocean, sea ice, snow, and clouds have a much higher emissivity for longwave radiation." - "longwave emissivity"?
Corrected
p. 32709: "Although the above-mentioned model evaluation studies have been made for the Arctic, little is known about the quality of operational weather forecasts in the central

Arctic."- needs rephrasing
Clarified
p. 32714: "increase the near-surface surface air temperature via longwave radiation." - so near-surface air temperature or surface temperature?
Clarified
p. 32726: Wegner–Bergeron–Findeisen (WBF) process: should be Wegener-
Corrected
p. 32729: Sentence needs rephrasing: "In addition to moisture, clouds need suspended aerosol particles with which to condense and freeze upon."
Clarified
references to de Boer et al. (2009) and (2011) are given in the text as Boer et al. and should be corrected
Corrected
Several abbreviations used in schematics are not defined (eg. Fig. 6)
We have dropped the previous Figure 6.
Harpaintner et al., 2001 should be after Harden et al in the reference list
Corrected
p. 32763 a
Corrected
typo: Laptav Sea should be Laptev
Corrected