Response to the comments by Reviewer 1

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

This is a review paper that summarizes recent advances in our understanding of small-scale physical processes in the Arctic atmosphere (section 2), sea ice and snow cover (section 3), and the ocean (section 4), and at the interfaces between. Small-scale processes are understood here to be those that need to be parameterized in climate models. It is an important and timely synthesis that focuses on advances since the start of the IPY. The paper is an impressive compilation of recent work and will be a useful, well-cited, contribution after some improvements. In general, I found the writing style to be quite scattered. We jump from one topic to the text in this long paper, often with no clear path. To provide an example of this: continental shelf waves seem to be introduced as an afterthought in the last paragraph of the discussion section, although they are not discussed previously. There are many more examples where the paper could use editing to maintain a clear, logical flow that is particularly expected of a review paper of this type.

We have added a short paragraph on continental shelf waves in the end of Section 4.5. Also in other parts of the paper we have better integrated the paragraphs and sections, among others by adding in the beginning on many sections short explanations on how the section is related to other parts of the manuscript. We have also tried to harmonize the writing style.

Below is detailed information on the additions.

The short paragraph on continental shelf waves reads as: ”More attention is also needed for continental shelf waves trapped above the continental shelf break region all around the Arctic Ocean where resonance occurs during spring tides. This mechanism has a great potential to trigger sea-ice break up during springtime in MIZ and consequently to enhance sea-ice melting and retreat.”

The new explanations on how various sections are related to other parts of the manuscript are as follows:

In the beginning of Section 2.1: ”Many of the small-scale processes in the Arctic atmosphere closely interact with the vertical structure of the atmosphere, modifying it and being constrained by it. The vertical structure of the Arctic atmosphere is characterized by an ABL capped by temperature and specific humidity inversions (hereafter ‘humidity inversions’), The inversions are generated by the combined effects of the negative radiation balance of the sea ice surface, the direct radiative cooling of the air, and the horizontal advection from lower latitudes (Figure 1). The temperature inversion layer has a strong stable stratification, whereas the ABL stratification is typically stable or near-neutral, the latter stage is most often due to wind shear but, in conditions of large downward radiation, also due to surface heating. Above the ABL, mixed layers can also occur inside and below clouds (Section 2.2).”

In the beginning of Section 2.1.2: ”Over sea ice in the central Arctic, the ABL is typically stably stratified during six winter months and is near-neutral or weakly stable during the other months (Persson et al., 2002; Section 2.1.1). Although cases of near-neutral stratification occur throughout the year, from the point of view of understanding and parameterization of the ABL over sea ice, the main challenges are related to stable stratification, and will be our focus here."

In the beginning of Section 2.1.2: "Although the Arctic ABL has a predominantly stable or near-neutral stratification, convection occurs as well. This is mostly due to the coexistence of ice and open water surfaces causing strong gradients in the surface temperatures."

In the beginning of Section 2.2.1: "Clouds are ubiquitous in the Arctic. As mentioned in Section 2.1, clouds interact with the temperature and humidity inversions and affect the ABL stratification (Figures 1 and 4), and fog (sea smoke) is often formed over leads and polynyas (Figure 3)."

In the end of Section 2.3.1: "Orographic effects are sometimes responsible for the genesis of polar mesoscale cyclones, e.g. in the case of lee cyclones southeast off Greenland. In most cases, however, polar mesoscale cyclones are not directly related to orographic forcing and are discussed in a separate section below."

In the end of Section 3.1.4: "Radiative processes in sea ice and snow closely interact with sea ice structure and other processes, such as snow and ice melt, heat conduction, refreezing of melt water, and gravity drainage of salt (Figure 7). "

In the beginning of Section 3.3: "Sea ice dynamics is closely tied to the processes discussed above; it is forced by the air-ice momentum flux (Section 2.1.4), and affects the regional albedo (Section 3.1.2), heat fluxes from the ocean to the atmosphere via leads and polynyas (Section 2.1.3), as well as sea ice growth via rafting and ridging (Figure 8), which further affects sea ice thermodynamics (Sections 3.1 and 3.2)."

In the beginning of Section 4.4: "The role of double diffusion at the ice-ocean interface is discussed in Section 4.1. Here we address double diffusion deeper in the ocean, far from the effects of the ice-ocean boundary layer (Figure 1)."

Improvements to the figures could help the focus and flow. The multitude of box diagrams were not very helpful here. In a discussion of small scale processes, a schematic that builds on Figure 1 would be much more helpful; Figure 1 is a little too basic to be of much use. I recommend a more detailed schematic that sketches and labels some of the processes discussed. A good example is provided by the schematic in this paper: [Padman, L. (1995). Small-Scale Physical Processes in the Arctic Ocean. Arctic Oceanography: Marginal Ice Zones and Continental Shelves, 97-129]. The authors could think about color coding the processes to be a certain color depending on the section in which they are discussed. A table might also be effective, with separate sections for the atmosphere, sea ice & snow, and ocean and a list of the small scale processes (with relevant temporal and spatial scales) discussed in the paper.

We have reduced the number of box diagrams by removing the previous Figures 4 and 6. We have also improved Figure 1 so that it sketches and labels many of the processes discussed, as suggested by the reviewer.

Related to the paper cited above, it would be very helpful if this review paper was set in context with previous papers reviewing some of the small-scale processes discussed here.

We have added a reference to Padman (1995) in the second-last paragraph of the Introduction, where we have also added references to several other relevant reviews. Also, we refer to Padman (1995) in the second paragraph of Section 4.3 and first paragraph of Section 4.5.

Additional comments in no particular order, but with a focus on the discussion of ocean processes:

1. "When sea ice is present, this layer extends down to 300m into the ocean (Dmitrenko et al., 2008)..." Why only when sea ice is present?

We have corrected the sentence: ”This layer extends down to 300 m into the ocean (Dmitrenko et al., 2008) and typically up to 100-1000 m in the atmosphere (Tjernström and Graversen 2009), but seasonal and regional variations are large.”.

2. There are sentences like the following throughout the manuscript. These really need to be much clearer. "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)."

We have clarified this and several other unclear sentences. The above-mentioned sentence now reads: ”Compared to a dry atmosphere, the ocean, sea ice, snow, and clouds have a much higher longwave emissivity and a much lower shortwave transmissivity (Perovich et al., 2007a,b).”

3. Section 4.2: Here is a good place to note that the salinity of sea ice is a function of its age/thickness.

We have added this note: ”The salinity (S) of sea ice depends on the ice age and thickness (Notz and Worster, 2009) and rarely exceeds S = 15, measured in the practical salinity scale, whereas the average salinity of polar surface water is about S = 30.”

4. Section 4.2: Where are dense brine flows observed to ventilate the deep Arctic Ocean (deeper than the halocline)? A reference is required here. The latter part of the first paragraph in this section is poorly constrained and needs citations.

The paragraph is now divided in two; we have added more information and citations in the new second paragraph of Section 4.2, which now reads as:

”Different processes contributing to the formation and evolution of the cold halocline layer (CHL) are described and discussed in Rudels et al. (1996). Salinization of cold water by brine rejection over shelves produces waters of varying salinities which can sink along the slope and interleave at their corresponding density levels (Aagaard et al, 1981). Depending on the density deficit, this process contributes partly to the formation and maintenance of the cold halocline, or to the ventilation of the deeper waters. Middag et al. (2009) used dissolved aluminium concentrations in the Eurasian Basin that indicate deep reaching convection of shelf waters. Paleoclimatologists (e.g. Dokken and Jansen 1999) argued that this type of ventilation was predominant in the Arctic Ocean during ice age in contrast with warm period where ocean deep convection is the dominant ventilation factor for deep waters. Because of the strong upper layer stratification of the Arctic, brine rejection in the central Arctic (in e.g., leads) cannot lead to deep reaching convection. This process, however, can contribute to the stratification in the upper CHL; an example from the Laptev Sea is given in Figure 10. During the IPY, Bauch et al (2011) collected an extensive data set on the oxygen isotope ratio δ18O in the Eurasian and Makarov Basins that led them to identify layers of the CHL influenced by brine release in coastal polynyas and layers of the CHL influenced by sea ice formation over the open ocean where vertical convection is more dominant. Both processes are active in the present climate but it is not clear if one process dominates over the other."

5. "In addition, efficient lateral mixed layer re-stratification also impedes mixed layer deepening (Toole et al., 2010)." The restratification is shown in a manuscript by Timmermans et al. (2012), JPO. This restratification is related to the submesoscale (O(1km)) flow field in the mixed layer, the effects or which are parameterized in GCMs.

We have added this important information, writing: "Re-stratification as a result of submesoscale (order of 1 km) instabilities within the surface layer is reported using ice-tethered profiler measurements from the Canada Basin (Timmermans et al., 2012)."

We added text on the parameterization of sub-mesoscale features in Section 4.5: "In pan-Arctic and global models, the SCVs are yet not resolved and must be parameterized. Their dynamics and resulting impact on vertical mixing are not properly understood or accounted for in the numerical models. Recent progress include the promising implementation by Fox-Kemper et al. (2011), however, the application in Arctic, under sea ice merit further research."

6. There is some speculation at the end of section 4.4 (the final three sentences). Related to the purported large steps, the section on double diffusion would not be complete without at least some mention of the double diffusive intrusions (with thicknesses much larger than typical double-diffusive layer thickness) via which AW heat and salt are propagated long distances. The following two papers (and references therein) will help: Rudels, B., Björk, G., Muench, R. D., & Schauer, U. (1999). Double-diffusive layering in the Eurasian Basin of the Arctic Ocean. Journal of marine systems, 21(1), 3-27. Walsh, D., & Carmack, E. (2003). The nested structure of Arctic thermohaline intrusions. Ocean Modelling, 5(3), 267-289.

We have removed the speculation and added a few sentences on the intrusions, also referring to the above-mentioned papers. The new text is as follows:

"The vertical scales of the steps that are much larger than the typical diffusive layer thicknesses, however, are comparable to the double diffusive, thermohaline intrusions frequently observed in the Arctic (Carmack et al, 1997, Rudels et al, 1999, Kuzmina et al 2011). The intrusions are laterally coherent over thousands of km, with nested temperature-salinity structure, and are proposed to be driven and organized by double-diffusive processes (Walsh and Carmack, 2003). The intrusions emanate from the core of the AW in the slope current, and spread into the interior basin propagating heat and salt over long distances. An example of the intrusive features at three stations taken across the Lomonosov Ridge is shown in Figure 12c."

7. Ocean eddies are brought in only in the discussion section (where there may be differing definitions here of what constitutes "submesoscale" and "mesoscale"). Discussion of eddies (submescale/mesoscale) seems an important omission.

We have added a whole new Section 4.5 on submesoscale eddies, fronts, and other processes.

8. An ordered section with a separate title "feedbacks" would be very useful here (reformulating some of the content of section 5.2, for example).

We have added a whole new Section 5.3 on feedback mechanisms, moving here the previous text on albedo feedbacks (previous Section 3.1.4) and adding text on several other feedbacks.

9. I would have liked to see more discussion on the theme of the last paragraph in section 5 related to the relevance of point measurements. This has been discussed in some previous studies (e.g. see a paper by Richter-Menge et al. (2006), Ann. Glaciol., that examines this with respect to Ice-Mass Balance buoy measurements); more discussion on this would be practical in the context of recommendations for future studies as outlined in this paper.

We have expanded the discussion having now a separate Section 5.4 for the topic. We note that most previous papers have addressed the representativeness of observations from the point of view of climatology of the observed variable. It is, however, more challenging to evaluate the representativeness of observations from the point of view of advances in process understanding.

10. Finally, there are grammatical errors/missing articles and so on throughout the manuscript. I suggest careful editing.

A co-author native in English has carefully checked and improved the language.

Response to the comments by Reviewer 2

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

This is a review paper that discusses recent advancements in the understanding of processes related to the Arctic climate system. With a focus on the ocean-ice-atmosphere interface, this paper covers a wide variety of important and timely topics, and contains a mind-boggling amount of information. Unfortunately, I believe that this is detrimental to the ultimate usefulness of this paper, as the large amount of information included results in a relatively scattered collection of summaries of various topics. Each of these various topics could (and most do) have individual review papers compiled summarizing major relevant advancements.
Unfortunately, I elected to suggest rejection of this paper. This was not a result of the paper's aim or because the subject matter discussed was not useful and interesting, but rather it was because I believe that as the current paper is written, it is simply too long (In the end, I spent more than a day reading through and thinking about this manuscript) and needs to be divided into multiple publications to be of use to the community.
In addition to its length, I do not feel as though the current version provides adequate connections between the individual topics discussed (or even within individual subtopics). This results in a long summary of papers that does not add much to our understanding beyond brief discussion of needed research efforts to address missing pieces, as covered in the discussion section. In my opinion, this has the topical coverage of a text book (though it does not contain the level of detail that would be required in a textbook), rather than a journal review paper. If there is only limited discussion on the connections between the main subsections (Atmosphere, Sea ice and snow, and Ocean), why not break it up into three papers? I don't believe that section 5.2 (cross-disciplinary aspects) provides sufficient justification for cramming it all together into one very long paper.
It is my opinion that the publication(s) stemming from this effort would be significantly more useful if the authors:

- Divide the current paper into multiple sub-discipline papers in order to reduce the overall length and more efficiently reach the intended audiences. This doesn't mean that there can not be interdisciplinary discussion or links, but they would be specific to one of the three current topic areas instead of one section that attempts to draw links between all of these disciplines.

We understand the reviewer's point of view, as our original manuscript indeed did not have enough interdisciplinary discussion. We have, however, kept all the material in the single manuscript. The main motivation of the DAMOCLES project, this Special Issue, and writing of this manuscript was to address the Arctic atmosphere, sea ice, and ocean as a single, interactive system. We feel that the main challenges in understanding the changing climate system in the marine Arctic are related to the interaction of its components, and we want to encourage colleagues to have a broader perspective than traditionally taken. We also note that Reviewer 3 favoured our approach of addressing the atmosphere, sea ice, and ocean in a single manuscript.

We have improved and significantly extended Sections 5 and 6 on interdisciplinary aspects, and added a note in the end of Introduction that a reader needs not necessarily read through all the manuscript, but may focus on sections of his/her interest and then on the concluding sections 5 and 6.

The extensions related to interdisciplinary aspects in Sections 5 are as follows:

Section 5.2 on cross-disciplinary analogies includes both old and new text; the new one is as follows: ”Furthermore the dominant vertical structures controlling stratification in the Arctic atmosphere and ocean, the temperature inversion and ocean halocline, have an analogy in the sense that both are strongly affected by the horizontal advection (of heat and salt, respectively). Challenges remain in better quantifying these advective fluxes, their vertical profiles, and their interaction with small-scale processes. Differences between the atmosphere and ocean include double diffusion that only occurs in the ocean and the strong stabilizing role of melt water at the ice bottom. The latter makes double diffusion an important limiting factor in the OBL during the melt season (in addition to its importance in the quiescent interior of the ocean).”

Section 5.3 on feedback mechanisms is a new section; all other text is new, except the text related to surface albedo feedback, which was previously in Section 3.1.4.

Section 5.4 on representativeness of the results is a new section; previously this issue was covered by only ten lines of text in the end of the old Section 5.2.

- More explicitly draw connections between individual sub-topics within the new subdisciplines to improve flow and readability.

We have better integrated the paragraphs and sections, among others by adding in the beginning or end on many sections short explanations on how the section is related to other parts of the manuscript. Below is detailed information on these additions.

In the beginning of Section 2.1: ”Many of the small-scale processes in the Arctic atmosphere closely interact with the vertical structure of the atmosphere, modifying it and being constrained by it. The vertical structure of the Arctic atmosphere is characterized by an ABL capped by temperature and specific humidity inversions (hereafter ‘humidity inversions’), The inversions are generated by the combined effects of the negative radiation balance of the sea ice surface, the direct radiative cooling of the air, and the horizontal advection from lower latitudes (Figure 1). The temperature inversion layer has a strong stable stratification, whereas the ABL stratification is typically stable or near-neutral, the latter stage is most often due to wind shear but, in conditions of large downward radiation, also due to surface heating. Above the ABL, mixed layers can also occur inside and below clouds (Section 2.2).”

In the beginning of Section 2.1.2: ”Over sea ice in the central Arctic, the ABL is typically stably stratified during six winter months and is near-neutral or weakly stable during the other months (Persson et al., 2002; Section 2.1.1). Although cases of near-neutral stratification occur throughout the year, from the point of view of understanding and parameterization of the ABL over sea ice, the main challenges are related to stable stratification, and will be our focus here.”

In the beginning of Section 2.1.2: ”Although the Arctic ABL has a predominantly stable or near-neutral stratification, convection occurs as well. This is mostly due to the coexistence of ice and open water surfaces causing strong gradients in the surface temperatures.”

In the beginning of Section 2.2.1: ”Clouds are ubiquitous in the Arctic. As mentioned in Section 2.1, clouds interact with the temperature and humidity inversions and affect the ABL stratification (Figures 1 and 4), and fog (sea smoke) is often formed over leads and polynyas (Figure 3).”

In the end of Section 2.3.1: ”Orographic effects are sometimes responsible for the genesis of polar mesoscale cyclones, e.g. in the case of lee cyclones southeast off Greenland. In most cases, however, polar mesoscale cyclones are not directly related to orographic forcing and are discussed in a separate section below.”

In the end of Section 3.1.4: ”Radiative processes in sea ice and snow closely interact with sea ice structure and other processes, such as snow and ice melt, heat conduction, refreezing of melt water, and gravity drainage of salt (Figure 7). ”

In the beginning of Section 3.3: ”Sea ice dynamics is closely tied to the processes discussed above; it is forced by the air-ice momentum flux (Section 2.1.4), and affects the regional albedo (Section 3.1.2), heat fluxes from the ocean to the atmosphere via leads and polynyas (Section 2.1.3), as well as sea ice growth via rafting and ridging (Figure 8), which further affects sea ice thermodynamics (Sections 3.1 and 3.2).”

In the beginning of Section 4.4: ”The role of double diffusion at the ice-ocean interface is discussed in Section 4.1. Here we address double diffusion deeper in the ocean, far from the effects of the ice-ocean boundary layer (Figure 1).”

- Make sure to add integrating conclusions that can only be made by synergistic evaluation of multiple individual publications and clearly bring those conclusions out in discussion on what we know and what we have yet to discover (this is done in limited fashion, but I believe more is required to make this paper/these papers really stand out in their own right). Without this it is my opinion that a review paper does not add much to the literature beyond a listing of useful references.

As explained above, we have significantly extended Section 5. Section 5.1 summarizes what we know and what we have yet to discover in the individual research fields. Section 5.2 adds important aspects on cross-disciplinary analogies. The new Section 5.3 summarizes the state of understanding of feedback mechanisms, and the new Section 5.4 evaluates the representativeness of the results, providing a more cross-disciplinary contribution to the question on what we still need to discover.

We have also made the following additions to Section 6.

First paragraph: ”We have reported advances in the development of parameterizations for the surface albedo, melt ponds, turbulent surface fluxes, desalination of sea ice, snow thermal conductivity, ablation rate at the ice bottom, double-diffusive transport, and submesoscale coherent vortices. In cloud physics, radiative transfer in the atmosphere, sea ice small-scale dynamics, and diapycnal mixing in the ocean, the recent advance in physical understanding has not yet yielded remarkable improvements in parameterizations. Ideally, the advance in physical understanding and parameterization should progress hand in hand: large model errors may suggest that something is wrong or insufficient in the physical understanding, which generates a need for more process studies, which improve the physical understanding and further result in improved parameterizations. In practice, however, the improvement of large-scale models often takes place after some delay. The reasons for this are manifold, including (a) the limited computational power, (b) the need to prioritize among the large number of issues that need improvements in models, (c) too little communication between observationalists and large-scale modellers, (d) too little communication between disciplines, and (e) compensating errors in models, which stop balancing each other out. The development of parameterizations is further complicated by the lack of understanding on how much complexity is cost-effective.”

Third paragraph: ”Considering climate modelling for this century, the sources of uncertainty can be roughly divided into three groups: (1) internal variability of the system, (2) model uncertainty, and (3) scenario uncertainty. According to Hawkins and Sutton (2009), the uncertainty related to internal variability dominates over the first decade of a model run, the model uncertainty dominates over the fourth decade, and the scenario uncertainty dominates over the ninth decade, except in high-latitudes. There the model uncertainty is so large that it still dominates over the ninth decade. A major challenge for the Arctic research community is to reduce the dominating model uncertainty.”

End of fifth paragraph: ”To improve the representativeness of observations (Section 5.4) a large spatial coverage of observations will be essential, so that observations at the main ice station will need to be supported by a network of autonomous ice-based stations, airborne observations (research aircraft, helicopters, unmanned aerial vehicles), underwater gliders, other research vessels, and intensive campaigns at coastal stations.”

Last paragraph (moved from previous Section 5.2 with improvements): ”It is noteworthy that better understanding and modeling of small-scale processes in the Arctic is essential not only for the Arctic climate system but also for the mid-latitudes. Sea ice decline in the Arctic has had some, although mostly poorly understood, effects on the large-scale atmospheric circulation (see Vihma (2014) and Walsh (2014) for recent reviews). The effects reaching mid-latitudes originate from changes in small-scale processes in the Arctic, including interaction of convection and baroclinic processes (Petoukhov and Semenov, 2010), destruction of the low-level temperature inversion (Deser et al., 2010), a deepening of the ABL (Francis et al., 2009), and destabilization of the lower troposphere (Jaiser et al., 2012). Bearing in mind the large errors still present in reanalyses and climate models (see the Introduction), these findings call for more research on small-scale processes in the Arctic.”

We believe that our revised version of Sections 5 and 6 present reasonably good integrating conclusions.

- Make improvements to the figures, which, currently are relatively dry, sometimes confusing, potentially incomplete (e.g. no discussion of aerosols in the interactions between clouds and radiative transfer: : : Or maybe that’s included in “condensation/evaporation” and “ice crystals”?). In my opinion, the fact that there are five complex flow charts/block diagrams is a clear indication that too much material is being covered for one paper (even one review paper!).

We have reduced the number of box diagrams by removing the previous Figures 4 and 6. We have also improved figures, especially present figures Figures 1 and 4, and added five new figures (Figures 2, 6, 9, 10, and 12). Considering aerosols, the legend of Figure 4 reads as follows:

”Figure 4. Schematic diagram on the effects and interactions related to mixed-phase stratocumulus clouds and radiative transfer. Macro- and microphysical processes and interactions are shown as arrows, the green arrow representing numerous microphysical processes related to aerosols, nucleation, evaporation, depositional ice growth, cloud layer glaciation, and effects of saturation vapour pressure differences of liquid and ice (see e.g. Morrison et al. 2012)."

The second-last paragraph of Section 2.2.1 includes text on the role of aerosols in cloud physics, now also referring to Figure 4.

In summary, while there is a ton of useful material contained within this paper, I just don't see very many people sitting down to read the whole thing. This would be a shame because I do believe that well-written publications summarizing our advances in understanding sub-grid-scale processes in the Arctic climate system would represent useful and necessary contributions to the current literature. Because I ultimately believe that the paper needs to be divided into multiple shorter papers with the abovementioned improvements, I can not recommend anything other than rejection of the current manuscript.

We felt the reviewer's comments very useful in improving the manuscript, and can only hope that the revised manuscript and the points of view we presented in this response will turn his/her attitude more positive towards our work.

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. To make the focus more clear, in several places we have added short texts to clarify the purpose of the section to follow and its links with the other parts of the manuscript.

Section 2: We have added the following paragraph for introduction and motivation:
”Many of the small-scale processes in the Arctic atmosphere closely interact with the vertical structure of the atmosphere, modifying it and being constrained by it. The vertical structure of the Arctic atmosphere is characterized by an ABL capped by temperature and specific humidity inversions (hereafter ”humidity inversions”), The inversions are generated by the combined effects of the negative radiation balance of the sea ice surface, the direct radiative cooling of the air, and the horizontal advection from lower latitudes (Figure 1). The temperature inversion layer has a strong stable stratification, whereas the ABL stratification is typically stable or near-neutral, the latter stage is most often due to wind shear but, in conditions of large downward radiation, also due to surface heating. Above the ABL, mixed layers can also occur inside and below clouds (Section 2.2).”

We have made several improvements to Section 2.1.1 Temperature and humidity inversions. These are mostly related to the 'Specific comments' presented below; our detailed responses are also below. These improvements hopefully clarify the parts that appeared inconsistent or contradictory.

In Section 2.1.2 Stable boundary layer, we have added the following text for introduction and motivation: "Over sea ice in the central Arctic, the ABL is typically stably stratified during six winter months and is near-neutral or weakly stable during the other months (Persson et al., 2002; Section 2.1.1). Although cases of near-neutral stratification occur throughout the year, from the point of view of understanding and parameterization of the ABL over sea ice, the main challenges are related to stable stratification, and will be our focus here."

To avoid jumping from topic to topic, we have made a major re-organization in Section 2.1.2. There is some new text, but even more important is the improved organization of the old text. An essential aspect is that in the second paragraph of the section we now classify the advances as follows: "Important issues addressed in recent research include (a) scaling of SBL turbulence and (b) presence of turbulence under very stable stratification." After that the text addresses first (a) and then (b), before going to low-level jets and modelling issues.

To avoid jumping from topic to topic, we have organized the text more logically in Section 2.1.3 (and dropped some details). Now we first address the occurrence of convection and the surface fluxes (1st paragraph), then the effects on air temperature (2nd paragraph), plume rise (3rd paragraph), downstream effects (4th paragraph), modelling challenges (5th paragraph), and cold-air outbreaks (last three paragraphs)

We have dropped unnecessary details from Section 2.1.4 to make it more focused.

We have made many improvements in Section 2.2 Clouds and radiation (see responses under 'Specific comments' below).

To clarify the link between Sections 2.3.1 and 2.3.2, we have added the following text: "Orographic effects are sometimes responsible for the genesis of Polar mesoscale cyclones, e.g. in the case of lee cyclones southeast off Greenland. In most cases, however, Polar mesoscale cyclones are not directly related to orographic forcing and will be dicsussed in a separate section below."

Section 3.1 on radiative processes and properties is made more focused by deleting Section 3.1.4 on surface-albedo feedback, and including its content in the new Section 5.3 Feedback mechanisms.

In Section 4 the changes are mostly related to suggestions by Reviewer 1, but we have also taken into account the request by Reviewer 3 to make the text more focused and consistent. The essential improvements are as follows:

We have removed details from Section 4.1 by deleting the last seven lines. In the same paragraph we have added more essential information: "In MIZ in the Barents Sea, Fer and Sundfjord (2007) observed dissipations rates in the upper ocean elevated above the levels expected from the wind-stress scaling, down to 2.5 times the keel depth, associated with the pressure-ridge keels."

We have made major revisions in Section 4.2: The second paragraph is almost entirely new, and strongly clarifies the issue.

The role of double-diffusion is clarified by two sentences in the beginning of Section 4.4: ”The role of double diffusion at the ice-ocean interface ws discussed in Section 4.1. Here we address double diffusion deeper in the ocean, far from the effects of the ice-ocean boundary layer (Figure 1).” Also, the last eight lines of the section are new.

Due to comments by Reviewer 1, we have added a whole new Section 4.5 on submesoscale eddies, fronts, and other processes.

We have re-organized Section 5 so that it now has four sub-sections instead of two.

Section 5.2 on cross-disciplinary analogies includes both old and new text; the new one is as follows: ”Furthermore the dominant vertical structures controlling stratification in the Arctic atmosphere and ocean, the temperature inversion and ocean halocline, have an analogy in the sense that both are strongly affected by the horizontal advection (of heat and salt, respectively). Challenges remain in better quantifying these advective fluxes, their vertical profiles, and their interaction with small-scale processes. Differences between the atmosphere and ocean include double diffusion that only occurs in the ocean and the strong stabilizing role of melt water at the ice bottom. The latter makes double diffusion an important limiting factor in the OBL during the melt season (in addition to its importance in the quiescent interior of the ocean).”

Section 5.3 on feedback mechanisms is a new section; all other text is new, except the text related to surface albedo feedback, which was previously in Section 3.1.4.

Section 5.4 on representativeness of the results is a new section; previously this issue was covered by only ten lines of text in the end of the old Section 5.2.

Section 6: We have made the following additions.

First paragraph: ”We have reported advances in the development of parameterizations for the surface albedo, melt ponds, turbulent surface fluxes, desalination of sea ice, snow thermal conductivity, ablation rate at the ice bottom, double-diffusive transport, and submesoscale coherent vortices. In cloud physics, radiative transfer in the atmosphere, sea ice small-scale dynamics, and diapycnal mixing in the ocean, the recent advance in physical understanding has not yet yielded remarkable improvements in parameterizations. Ideally, the advance in physical understanding and parameterization should progress hand in hand: large model errors may suggest that something is wrong or insufficient in the physical understanding, which generates a need for more process studies, which improve the physical understanding and further result in improved parameterizations. In practice, however, the improvement of large-scale models often takes place after some delay. The reasons for this are manifold, including (a) the limited computational power, (b) the need to prioritize among the large number of issues that need improvements in models, (c) too little communication between observationalists and large-scale modellers, (d) too little communication between disciplines, and (e) compensating errors in models, which stop balancing each other out. The development of parameterizations is further complicated by the lack of understanding on how much complexity is cost-effective.”

Third paragraph: ”Considering climate modelling for this century, the sources of uncertainty can be roughly divided into three groups: (1) internal variability of the system, (2) model uncertainty, and (3) scenario uncertainty. According to Hawkins and Sutton (2009), the uncertainty related to internal variability dominates over the first decade of a model run, the model uncertainty dominates over the fourth decade, and the scenario uncertainty dominates over the ninth decade, except in high-latitudes. There the model uncertainty is so large that it still dominates over the ninth decade. A major challenge for the Arctic research community is to reduce the dominating model uncertainty.”

End of fifth paragraph: ”To improve the representativeness of observations (Section 5.4) a large spatial coverage of observations will be essential, so that observations at the main ice station will need to be supported by a network of autonomous ice-based stations, airborne observations (research aircraft, helicopters, unmanned aerial vehicles), underwater gliders, other research vessels, and intensive campaigns at coastal stations.”

Last paragraph: ”It is noteworthy that better understanding and modeling of small-scale processes in the Arctic is essential not only for the Arctic climate system but also for the mid-latitudes. Sea ice decline in the Arctic has had some, although mostly poorly understood, effects on the large-scale atmospheric circulation (see Vihma (2014) and Walsh (2014) for recent reviews). The effects reaching mid-latitudes originate from changes in small-scale processes in the Arctic, including interaction of convection and baroclinic processes (Petoukhov and Semenov, 2010), destruction of the low-level temperature inversion (Deser et al., 2010), a deepening of the ABL (Francis et al., 2009), and destabilization of the lower troposphere (Jaiser et al., 2012). Bearing in mind the large errors still present in reanalyses and climate models (see the Introduction), these findings call for more research on small-scale processes in the Arctic.”

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 (previous Figure 3). We now introduce the figure as follows:

”The difficulties in modelling clouds over the Arctic are related to the numerous interactive processes, schematically illustrated in Figure 4 for mixed-phase stratocumulus (MPS) clouds. Even though cloud fraction is relatively high year-round, Shupe (2011) has clearly shown that seasonally-dependent, vertical cloud phase preferences exist. Liquid-only clouds rarely exist above 2 km above ground level, occurring predominantly during the sunlit portions of the year. Unlike the rest of the globe, MPS clouds tend to be the most common in the lower Arctic troposphere, except during winter and early spring when ice-only clouds are somewhat more frequent. The MPS clouds have a profound impact on the surface energy balance, since liquid water generates significantly more longwave radiation to the surface than do ice clouds (Tjernström et al., 2008; Sedlar et al. 2011; Wesslen et al., 2013), and hence on the surface melt and freeze (Figure 4). Hence, MPS clouds will be a focus here.”

Considering ‘key micro-physical processes in cloud physics (aerosols and CCN/IN for example)’ the legend pf Figure 4 reads as: ”Figure 4. Schematic diagram on the effects and interactions related to mixed-phase stratocumulus clouds and radiative transfer. Macro- and microphysical processes and interactions are shown as arrows, the green arrow representing numerous microphysical processes related to aerosols, nucleation, evaporation, depositional ice growth, cloud layer glaciation, and effects of saturation vapour pressure differences of liquid and ice (see e.g. Morrison et al. 2012)."

More text on aerosols can be found from the second-last paragraph of Section 2.2.1, now also referring to Figure 4.

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

See our first response above.

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). Thank you for mentioning the Bromwich et al. paper, which 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 as follows: ”Uncertainty in the parameterization of small-scale processes continues to be among the greatest challenges facing climate modeling, particularly in high-latitudes.”

We have added a small paragraph in Section 6, referring to a study that shows that the model uncertainty is more important in high than low latitudes: ”Considering climate modelling for this century, the sources of uncertainty can be roughly divided into three groups: (1) internal variability of the system, (2) model uncertainty, and (3) scenario uncertainty. According to Hawkins and Sutton (2009), the uncertainty related to internal variability dominates over the first decade of a model run, the model uncertainty dominates over the fourth decade, and the scenario uncertainty dominates over the ninth decade, except in high-latitudes. There the model uncertainty is so large that it still dominates over the ninth decade. A major challenge for the Arctic research community is to reduce the dominating model uncertainty.”

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 indeed addressed global feedbacks. Now we simply write: ”The relative importance of the above-mentioned processes in the Arctic is not well known.”

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. The legend is now as follows:

Figure 1. Simplified presentation of physical processes and vertical profiles of temperature (T), air humidity (q), and ocean salinity (S) in the marine Arctic climate system. In reality the shape of the profiles varies in time and space. The numbers indicate the following processes: 1. atmospheric advection of heat and moisture to the Arctic, 2. oceanic advection of heat and salt to the Arctic, 3. generation of temperature and humidity inversions, 4. turbulence in stable boundary layer, 5. convection over leads and polynyas, 6. cloud microphysics, 7. cloud-radiation-turbulence interactions, 8. reflection and penetration of solar radiation in snow/ice, 9. surface melt and pond formation, 10. formation of superimposed ice and snow ice, 11. gravity drainage of salt in sea ice, 12. brine formation, 13. turbulent exchange of momentum, heat and salt during ice growth, and 14. double-diffusive convection. More detailed illustration of small-scale processes is given in Figures 2-12.

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 as follows: ”Compared to a dry atmosphere, the ocean, sea ice, snow, and clouds have a much higher longwave emissivity and a much lower shortwave transmissivity (Perovich et al., 2007a,b).”

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 as follows: ”This is for several reasons: (a) processes are often so complicated that it is not possible to accurately describe them solely on the basis of resolved variables, (b) models have errors in the resolved variables, (c) the resolved variables represent a large volume (grid cell) but there are large variations in the sub-grid scale processes inside the grid cell, (d) the physics of small-scale processes is often not sufficiently well known, (e) parameterizations require experimental data to constrain closure assumptions and the amount of such data may not be sufficient (in volume or in range), and (f) parameterizations are often tuned to make the overall performance of models better, according to Steeneveld et al. (2010), even when this makes the description of the particular small-scale process worse.”

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.

Now we write: ”One of the major efforts was the European project “Developing Arctic Modeling and Observing Capabilities for Long-term Environmental Studies” (DAMOCLES, in 2005-2009), for which this Special Issue is dedicated. The project included an extensive amount of in-situ observations in the Arctic, supported by remote sensing, data analyses, and model experiments. During DAMOCLES, the drifting ice station Tara was a platform for oceanographic, sea ice, and meteorological research (Gascard et al., 2008). In addition, oceanographic and sea ice observations were carried out by several ships, meteorological research was made at ships, including short drift stations, by a research aircraft, and at coastal sites. Furthermore, drifting buoys, underwater gliders, and moorings collected extensive sets of oceanographic, sea ice, and meteorological observations.”

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. Now we write:
”There is also a pronounced annual cycle; in SHEBA data surface-based inversions were most common in winter and autumn, accounting for roughly 50% of the cases, whereas in summer practically all inversions were elevated ones on top of a near-neutral ABL. Since SHEBA, however, the occurrence of surface-based inversions in autumn has most probably decreased due to the sea ice decline.

Using the Atmospheric Infrared Sounder data, Devasthale et al. (2010) estimated that the area-averaged (70 to 90°N) clear-sky temperature inversion frequency is 70–90% for summer and approximately 90% for winter. Raddatz et al. (2011) found similar temperature inversion frequencies for a Canadian polynya region, whereas Tjernström and Graversen (2009) reported, based on SHEBA, that inversions, either surface based or elevated, are practically always present in the central Arctic.”

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. We now write: ”The frequency, depth, and strength of temperature inversions have been found to correlate positively among each other, both spatially and temporally, and correlate negatively with the surface temperature (Devasthale et al. 2010; Zhang et al. 2011). … In addition, during winter the temperature inversion strength over the ocean has a negative correlation with sea-ice concentration (Pavelsky et al., 2010).”

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 now write: ”Vihma et al. (2011) reported that, compared to temperature inversions, humidity inversions were on average thicker and had their base at a higher level. They concluded that this was mostly due to the role of the snow and sea ice surface as a sink for heat but commonly not for humidity (see also Persson et al., 2002). In other studies, however, humidity inversions have been found to usually coincide with temperature inversions (Sedlar et al. 2012; Tjernström et al. 2012). Differences in the observations may at least partly originate from different seasons (early spring in Vihma et al., (2011) and late summer in Tjernström et al. (2012)), while Sedlar et al. (2012) include SHEBA and several years of data from Barrow, hence possibly indicating that there may also be regional 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: ”Temperature and humidity inversions also have notable implications for the longwave radiation. Bintanja et al.

(2011) and Pithan and Mauritsen (2014) demonstrated that atmospheric near-surface cooling efficiency decreases markedly with the temperature inversion strength, as the inversion layer damps the infrared cooling to space, and Boé et al. (2009) obtained analogous results for the role of air temperature inversion in reducing the radiative cooling of the ocean surface. Humidity inversions, in turn, can contribute up to 50% of the total amount of condensed water vapour in a relatively dry atmosphere in winter and spring, which can significantly influence the longwave radiative characteristics of the atmosphere (Devasthale et al. 2011), and they are presumably vital for the formation and maintenance of Arctic clouds (Section 2.2.1)."

See also our new Section 5.3 on feedback mechanisms.

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^{o}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.

We now write: "If RHliq becomes sub-saturated in the presence of ice crystals, liquid droplets must evaporate following the WBF process, and hence would cause a rapid depositional ice growth and cloud layer glaciation. Instead Shupe (2011) has shown that in-cloud RHliq and temperature distributions at a number of Arctic stations are in fact surprisingly similar, lending support for a system that is both conditioned for, and dependent upon, mixed-phase clouds."

and later on:

”The key difference in the Arctic is the presence of liquid and ice simultaneously. Shupe et al. (2008) show that ice production is generally limited to cloud-generated updrafts that increase the supersaturation with respect to ice. When downdrafts were observed, ice production generally ceased and fewer ice crystals grew to large sizes and fell from the still-present, yet slightly more tenuous, liquid layer. Hence the coexistence of liquid and ice is intimately linked to cloud scale motions, which in turn depends on the presence of liqud water.”

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. We now write: ”Tjernström (2007) suggested that most of the boundary-layer turbulence in the Arctic is in fact generated by the 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). Cloud-generated mixing is found beneath cloud base, but the extent to which these turbulent motions reach the surface is often limited by a sub-cloud stable layer (Shupe et al., 2013; Sedlar and Shupe, 2014) and is also dependent on the distance from the cloud base to the surface and the sublimation of precipitation in the layer below the cloud base (Figure 4). Hence the strongest but also most variable turbulence generations is due to buoyant cloud overturning due to cloud top cooling, which generates eddies that often persist below the cloud base. Mechanical generation of turbulence at the surface, on the other hand, is seldom very strong and intense boyant mixing is essentially absent over sea ice (other than over winter leads/polynyas), and the ABL is therefore most often shallow. Coupling, or the lack thereof, of MPS clouds to the surface and surface fluxes is therefore more often dependent on if the cloud generated turbulence can reach down to the ABL or not, rather than the other way around. This in turn is sensitive to the cloud generated turbulence but also to the cloud base height (Figure 4; Tjernström et al. 2012; Shupe et al. 2013; Sotiropoulou et al. 2014).

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 made more references 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%.

We now write: ”The difficulties in modelling clouds over the Arctic are related to the numerous interactive processes, schematically illustrated in Figure 4 for mixed-phase stratocumulus (MPS) clouds. Even though cloud fraction is relatively high year-round, Shupe (2011) has clearly shown that seasonally-dependent, vertical cloud phase preferences exist. Liquid-only clouds rarely exist above 2 km above ground level, occurring predominantly during the sunlit portions of the year. Unlike the rest of the globe, MPS clouds tend to be the most common in the lower Arctic troposphere, except during winter and early spring when ice-only clouds are somewhat more frequent. The MPS clouds have a profound impact on the surface energy balance, since liquid water generates significantly more longwave radiation to the surface than do ice clouds (Tjernström et al., 2008; Sedlar et al. 2011; Wesslen et al., 2013), and hence on the surface melt and freeze (Figure 4). Hence, MPS clouds will be a focus here.”

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 edited the text to be clearer: ”In terms of temperature, the radiative cooling from the liquid cloud top (Harrington et al., 1999) dominates over other local processes and hence, in the absence of frontal passages or other large-scale controls, cloud droplets will continuously form to replace the water that precipitates out.”

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, now writing: "Historically many models, especially weather forecast models, such as the ECMWF model, distinguish between cloud liquid and ice based only on temperature, having often failed to maintain liquid in very cold winter clouds (e.g. Beesley et al. 2000; Tjernström et al., 2008)."

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.

We now write: "Cloud top radiative cooling is typically very efficient as near-adiabatic liquid water content (LWC) profiles are common in the Arctic (Curry, 1986; Shupe et al., 2008). Arctic MPS droplet radii generally also increase with height (e.g. Curry, 1986) and droplet effective radii often range between 4 to 15 μm. Typical LWC in MPS peaks between 0.1 – 0.2 g m^{-3} (McFarquhar et al., 2007) and together with relatively thin liquid layers (Shupe et al., 2008; Shupe 2011), cloud liquid water path (LWP) is often below 100 g m^{-2} (de Boer et al., 2009; Sedlar et al., 2011; Shupe et al. 2011). In-cloud ice water contents (IWC) are generally largest between cloud mid-level and base, decreasing upwards towards cloud top where they are initially formed (Shupe et al., 2008). Recent campaigns report a wide spectrum of ice crystal effective diameters, ranging from 20 – 60 μm (McFarquhar et al., 2007; Shupe et al., 2008) and upwards of 100 μm when falling through the subcloud layer (de Boer et al., 2009)."

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.

We now write: ”The ratio of LWC to total water content is often larger than 0.8 (McFarquhar et al., 2007; Shupe et al., 2008) indicating the resilience of cloud liquid despite near-constant drizzle and ice precipitation. In fact, 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, further emphasising the importance of cloud motions in controlling the recilience of MPS.”

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