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Atmospheric Chemistry and Physics
Discussions

# *Interactive comment on* "Advances in understanding and parameterization of small-scale physical processes in the marine Arctic climate system: a review" *by* T. Vihma et al.

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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.

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.

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.

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

1) critically revising the text making it more focused and consistent

2) including figures illustrating key points raised in each section (see eg, Bromwich et al. 2012 "Tropospheric clouds in Antarctica", Rev. Geophys.)

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.

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...).

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

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).

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)

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

6. p. 32710: "observations of liquid water present in clouds at temperatures down to $-34$ âŮęC 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 -34C 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.)

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.

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 non-existent." 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."

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".

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?

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.

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 $-34$âŮęC (Intrieri et al., 2002). In fact, MPS are often the preferential cloud class when temperatures range between $-15$ to near 0âŮęC (Shupe, 2011; de Boer et al., 2009)." - these statements should be better linked

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.

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. Cli-



mate, 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.

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.

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)

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

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.

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. 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."

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)?



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

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 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

22. Also, a list of acronyms used in the entire paper will be helpful

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"? 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 p. 32714: "increase the near-surface surface air temperature via longwave radiation." - so near-surface air temperature or surface temperature? p. 32726: Wegner–Bergeron–Findeisen (WBF) process: should be Wegener- p. 32729: Sentence needs rephrasing: "In addition to moisture, clouds need suspended aerosol particles with which to condense and freeze upon." references to de Boer et al. (2009) and (2011) are given in the text as Boer et al. and should be corrected Several abbreviations used in schematics are not defined (eg. Fig. 6)

Harpaintner et al., 2001 should be after Harden et al in the reference list p. 32763 a typo: Laptav Sea should be Laptev

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