2014-Sept-07

Authors comments to the referees

concerning the manuscript
“Recent advances in understanding the Arctic climate system state and change from a sea ice perspective: a review. By R. Döscher, T. Vihma, and E. Maksimovich

We appreciate the reviewers comments and have addressed the suggestions and points of criticism. We received 3 reviews, two anonymous (#1 and #2) and one by the public referee F. Pithan (here called referee #3). The review article constitutes a challenge to balance plenty of existing results and scientific literature with the necessity to provide a comprehensive overview and concluding new angles of insight. We received requests to put more weight on both sides of this balance. While following most of the detailed requests, we also improved on the overall picture.

Response to Referee #1

“First, I would suggest the authors to make the paper more concise that will improve its readability. The paper is very long with 77 pages of the single spaced text. Many duplicated descriptions can be trimmed. For example, Albedo feedback, cloud effects, atmospheric circulation, cyclones, snow impacts, and so one, have been repeated in more than one section. The lengthy text and duplicated descriptions may make the readers to get lost, perhaps reducing the importance of this paper.”

The referee's suggestion for a more concise paper is addressed by reducing double descriptions where possible. This has been done for example by removing the previous section 3.6 on the tipping point. The essential contents of the previous Section 3.6 is now merged into section 6. However, the complex nature of Arctic change prevents a significant effect of those measures on the overall length of the paper. Both referee #1 and the public referee argue for more elaborated descriptions of oscillation patterns and feedbacks. We feel that a broad review needs to cover all components relevant for the sea ice change and we tend to follow that line with adequate descriptions rather than too simple explanations. While keeping the text on roughly the same length, we try at the same time strengthen the big picture by additional summaries, conclusions and evaluations within the sections.

“Second, I would suggest the authors to double check consistence of cited results from the literature. For example, increase in Pacific water and Atlantic water inflow into the Arctic Ocean have been discovered in the late 1990s and early 2000s. Later publications just reconfirm this finding. The review in this paper seems simply ignore the earlier, original finding. The similar thing also happens in the review of other scientific findings, such as changes in Arctic cyclones.”

Our motivation for a focus on recent findings is to give a picture of the current situation with ongoing changes in the ice and in the knowledge about changes, rather than giving a complete historical overview. A brief historical background on the sea ice is given in the introduction section. Following the referee's recommendation, we have now added brief information on findings from previous decades before. Before the late 1990s, direct observation on ocean inflow transports into the Arctic were scarce, and we rely on water mass observations. To further illustrate the Pacific inflow, we are now referring also to Aagaard and Carmack (1989). For the Atlantic part, we find indication for increased inflow in the paper of McLaughlin et al. (1996) and Smith et al. (1999). We also added information on cyclone trends since 1952. Approaches to Arctic cyclone statistics exit since the 1950 with very limited observations. More complete surveys were undertaken by e.g. Serreze (1993), and McCabe et al. (2001), revealing a positive trend of Arctic cyclone frequency for the period 1952 - 1997 for the winter. Those findings are now integrated in the text.

“Finally, I would suggest the authors to provide some discussions and evaluations when reviewing previous results, instead of simply listing what have been published. The Arctic sea ice study includes so many complicated processes. If the authors can help evaluate what results make greater sense in physics and what may not be robust, this review paper would be much helpful for new researchers, especially young students, to correctly understand Arctic climate. ”

We agree that this is a good suggestion. In many cases, the manuscript includes short summaries and discussions of the implications of the findings. We have increased the number of those text passages. Below please find a list of the most essential paragraphs containing discussions and evaluations:
- last paragraph of Section 2.1 (new)
- Section 3.6 (new)
- second half of the first paragraph of Section 4
- last paragraph of Section 4.1 (old and new)
- last paragraph of Section 4.2 (new)
- last paragraph of Section 4.3 (new)
- an extended section 6

“Here I would provide the following additional comments for the authors to consider.”

“1. Throughout reading the paper, I found that the role of AO or NAO is largely missed or understated. Both observational and modeling studies have demonstrated that the positive polarity of AO or NAO drove decrease in sea ice extent or thickness and increase in sea ice from the mid 1980s to the mid 1990s. The AO or NAO driven sea ice decrease during this period preconditioned later acceleration of sea ice declining from the late 1990s. The results about AO or NAO’s role in sea ice can be found in the following publications: ...”

We have added the influence of AO and NAO by adding a new first paragraph in section 4.1.:

“Large scale oscillation patterns have been influential in preconditioning and forcing the observed sea ice decline at times. Both observational and modelling studies have demonstrated that the positive polarity of AO or NAO drove a decrease in sea ice extent or thickness between 1980 and the mid 1990s. Before 1980, the relation was less efficient because the NAO pattern was shifting in space around 1980 (Hilmer and Jung 2000). Such spatial shifts have been shown to impact on Arctic temperatures throughout the 20th century, characterized by varying angles of the axis between the NAO's centres of action (Jung et al., 2003; Wang et al., 2012). During the positive NAO/AO years after 1980, and especially during the most positive years 1989 – 1995, altered surface winds resulted in a more cyclonic ice motion and a more pronounced Transpolar Drift Stream (TDS) connected to enhanced ice openings, thinner coastal ice during spring and summer, and to increased sea ice export (Rigor et al. 2002; Serreze et al. 2007). The continued downward trend of sea ice extent after the mid 1990s are interpreted as delayed response in addition to other effects such as the ongoing increase of atmospheric temperatures (Lindsay and Zhang 2005). In the winter 2010/2011, a strongly negative AO was observed (Stroeve et al., 2011). Maslanik et al. (2011) argue that this explains a recent partial recovery of multiyear ice extent (see section 3.3).”

“2. The paper mentions the second EOF mode many times throughout the paper using different terminology, but the definition of the second mode is messy and the role of the second mode is not

convincing. In the paper, the second mode is either defined as anti-correlated anomalous sea level pressures on the both sides of Fram Strait or between the Beaufort Sea and the Kara Sea or between the Canadian Arctic and the Russian Arctic. When searching the literature, I found that Skeie (2000) first defined the second mode as the "Barents Sea Oscillation (BO)" and discussed this mode. Skeie (2000) indicates "Patterns reminiscent of the BO emerge in the two composites when AO related variability is removed". This makes sense to me in physics because AO explains about 25% of variance of the atmospheric circulation systems (Icelandic low, Azores high, and Aleutian low) variability, while BO only explains less than half of the variability variance without clear physical expression in circulation systems. Considering the large difference of the variances they explained, it may not make sense that the leading mode AO does not play a role, while BO plays a role, for the basin scale changes. Meanwhile, Timo Vihama (2013), which is cited in this review paper, also indicates that the second mode is not robust."

Our original manuscript introduces the second EOF model as " … Dipole Anomaly (DA; the second leading mode of sea-level pressure anomaly in the Arctic; Wu et al., 2006)". Other interpretations such as regional pressure differences occur in this paper as consequence of the DA, but not as a definition. Wu et al. (2006) which we are referring to, give the somewhat more detailed definition: "The dipole anomaly corresponds to the second-leading mode of EOF of monthly mean sea level pressure (SLP) north of 70°N during the winter season (October–March)"

Vihma et al.'s (2012) message is that the DA, as the second mode of a principal component analysis, is sensitive to the area of calculations (as also seen in the different shapes of differently defined second modes by Wu et al. (2006) and Skeie (2000)). In addition, Vihma et al. (2012) show that shape and amplitude naturally depends very much on the analysis time period chosen and thus might not be robust or relevant for different climate periods, in particular not when using the DA as indicator for sea ice drift. For explaining the sea ice drift variability, Vihma et al.'s (2012) suggest the Central Arctic Index (CAI) as a more suitable index, because it follows the variability of the Trans Arctic Drift.

We realize that we need to be more specific on the different definitions of the DA and meridional oscillation patterns. We have modified the DA definition in section 3.4 and added also a short description of the history behind the definition of the DA including the work of Skeie (2000). Section 3.4 now includes the paragraph:

"Interannual variability in the monthly mean ice drift has been attributed to the predominant atmospheric circulation patterns, such as the Arctic Oscillation (AO), the North Atlantic Oscillation (NAO), the Dipole Anomaly (DA; the second leading mode of sea-level pressure anomaly in the Arctic), and the Central Arctic Index (CAI). Wu et al. (2006) define the DA as a dipole anomaly corresponding to "the second-leading mode of EOF of monthly mean sea level pressure (SLP) north of 70°N ... ". Earlier, Skeie (2000) found the second EOF of monthly winter SLP anomalies poleward of 30°N, named "Barents Sea anomaly", to be highly influential on Eurasian climate. Overland and Wang (2010), referring to an analysis area north of 20°N, find a third EOF mode, which they called the Arctic Dipole (AD), reminiscent of the "Barents Sea anomaly" of Skeie (2000). Thus, the definitions of second or third modes vary. All versions commonly point at variability modes introducing meridional circulation components."

When reviewing the 2007 sea ice minimum, the paper mentioned that sea ice export via Fram Strait by the second EOF mode made contribution. Actually, in 2007, no much summer sea ice reached Fram Strait. Surface wind mainly blew from the Atlantic to the Arctic from 2001-2006. Sea ice flux via Fram Strait also indicates a decrease in sea ice flux before 2007. So, sea ice export is not a contributor to the 2007 sea ice minimum. Why was there a record minimum of summer sea ice in 2007? A number of other studies have well explained that it is a cumulative result of a significant pattern shift of AO from the late 1990s that caused a zonally dominant wind flow to a medionally dominant wind flow (e.g., Zhang et al. 2008; Overland and Wang 2010). This pattern shift is beyond *what traditional AO can describe. This is why no record minimum sea ice occurred in the past although AO, or even the second EOF mode, varies year by year. More discussions about NAO pattern shift can also be found in an earlier study by Jung et al. (2003) and a recent study by Wang et al. (2012). Here are some publications I used for my comments: Jung, T., M. Hilmer, E. Ruprecht, S. Kleppek, S. K. Gulev, O. Zolina (2003), Characteristics of the recent eastward shift of interannual NAO variability. J. Clim., 16, 3371–3382; Overland, J. E., and M. Wang (2010), Large-scale atmospheric circulation changes are associated with the recent loss of Arctic sea ice. Tellus A, 62, 1–9, doi:10.1111/j.1600-0870.2009.00421.x; Skeie, P. (2000), Meridional flow variability over the Nordic seas in the Arctic Oscillation framework, Geophys. Res. Lett., 27, 2569, doi:10.1029/2000GL011529; Wang, Y.-H., G. Magnusdottir, H. Stern, X. Tian, and Y. Yu (2012), Decadal variability of the NAO: Introducing an augmented NAO index, Geophys. Res. Lett., 39, L21702, doi:10.1029/2012GL053413; Zhang X, A. Sorteberg, J. Zhang, R. Gerdes, and J. C. Comiso (2008), Recent radical shifts of atmospheric circulations and rapid changes in Arctic climate system. Geophys. Res. Lett., 35, L22701, doi: 10.1029/2008GL035607.*

The discussion of the 2007 record sea ice event in our paper is distributed over several sections, rather than concentrated in a single dedicated section. There is a combination of various reasons leading to the event, including changed atmospheric forcing with enhanced meridional components and generally thinner and younger ice. One component of the event was anomalously high sea ice coverage in Fram Strait and south of Fram Strait during summer 2007. This was accompanied by sea ice export rates stronger than during the period 2000-2006. Those anomalies are documented e.g. by Zhang et al. (2008). We have now added that reference to section 3.4 (sea ice motion).

We completely agree with the notion of increased meridional atmospheric flow components. That change is even more emphasized in the new version of the manuscript. (section 3.4, sea ice motion):

"... Overland and Wang (2010), referring to an analysis area north of 20°N, find a third EOF mode, which they called the Arctic Dipole (AD), reminiscent of the "Barents Sea anomaly" of Skeie (2000). Thus, the definitions of second or third modes vary. All versions commonly point at variability modes introducing meridional circulation components...."

In section 4.1 (Large-scale circulation and cyclones), the manuscript also contains information on patterns shifts:

"During this century, the large-scale circulation in the Arctic has changed from a zonally dominated circulation type, which can be well characterized by the AO, to a more meridional pattern characterized by the AD, where a high-pressure center is typically located in the Canadian Arctic and a low in the Russian Arctic (Overland and Wang, 2010)".

The role of the changing NAO in a static and dynamic interpretation (variability of amplitude and spatial pattern) is now better acknowledged in section 4.1:

"... Before 1980, the relation was less efficient because the NAO pattern was shifting in space around 1980 (Hilmer and Jung 2000). Such spatial shifts have been shown to impact on Arctic temperatures throughout the 20^{th} century, characterized by varying angles of the axis between the NAO's centres of action (Jung et al., 2003; Wang et al., 2012). ...".

3. The paper discusses "new state" of the Arctic. I like this idea. Here I would draw authors' attention that the "new state" should not only be confined to sea ice. The Arctic atmosphere (e.g., the meridionally transitioned circulation pattern) and ocean (e.g., unprecedented warming of Atlantic layer) may also characterize the "new state" of the Arctic climate. The proposition of the "new state" can be also found in a number of recent publications. For example, Zhang et al. (2008) suggest ". . .implying *a new era of global-warming-forced climate change and shedding light on recent arguments about a tipping point of Arctic climate system change toward a qualitatively different new state". Jeffries et al. (2013) also discuss the Arctic shifts to a new normal through summarizing recent systematic changes.*

Jeffries, M. O., J. E. Overland, and D. K. Perovich (2013), The Arctic shifts to a new normal. Physics Today, 66, 35, doi:10.1063/PT.3.2147;

Zhang X, A. Sorteberg, J. Zhang, R. Gerdes, and J. C. Comiso (2008), Recent radical shifts of atmospheric circulations and rapid changes in Arctic climate system. Geophys. Res. Lett., 35, L22701, doi: 10.1029/2008GL035607.

In addition, the paper proposes to continually investigate AO and the second mode in the future. This does not sound new and exciting. As commented above, the role of AO has been well documented and the second mode is questionable. There would also be a mismatch if using traditional, long-term-data-defined AO to explore unusual variability or changes of the "new state" of the Arctic climate. It would be great and useful if the authors can propose or speculate a brand new idea based on this systematic review.

Concerning the definition of the term, "new Arctic", qualitative changes of Arctic quantities other than sea ice, taking a multi-disciplinary view, can certainly be included. In the new version of the manuscript, we are widening the description of the "new Arctic" in the introduction section by linking to Atlantic water temperatures and wind patterns.
The definition of a "new Arctic" is a tough one, because the physical changes are not static and neither irreversible. Here we mean to characterize the current state after qualitative changes of sea ice and related atmosphere and ocean conditions during the last few decades. The physical changes are also constraining bio-geo-chemical changes in the Arctic. Those, we chose to exclude from the definition of the new Arctic, and leave it to the expertise of the respective communities to integrate an extended definition with the physical changes.

We agree that the role of the AO and differently defined oscillation patterns (such as the CAI) have been explored in the recent past, as reviewed in this paper. A generalized conclusions is that atmospheric oscillation patterns play an important role. The importance of the specific oscillation patterns might change in the future, while different composition of oscillation patterns certainly will continue to play a role as the thinner sea ice is more susceptible to wind driving. Possibly, more robustly defined indicators (such as the CAI) might be relevant in the future.
The statement in question is part of a paragraph discussing the future prospect of Arctic climate prediction. Among other influences (storage capacity, ocean variability), atmospheric oscillation patterns are just one factor out of several. We have modified the statement in question to:

"Furthermore, studies on possible future transformation between various atmospheric oscillation patterns (such as AO, DA and CAI in the past) will be essential for understanding the real potential of Arctic climate prediction."

4. In Section 3.7, the paper discusses future sea ice change projections and state "Global climate models, when run for observed periods, tend to underestimate the sea ice decline . . .". This statement may not be appropriate if the model run is not the initialized prediction. In CMIP5 or CMIP3, the historical runs are based on at least a hundred year long model spin-up. Initial conditions cannot *persist into the observed time period, which is generally from 1979 – 2005, in particular for sea ice. Variability of sea ice therefore has different phase across different ensembles. So, multi-ensembles mean minimizes natural variability and mainly reflects externally forced changes. However, what the observations show includes both internal and externally forced signals. Direct comparison between the simulation results from CMIP-like models and the observation may not be appropriate.*

We fully agree and make sure the statements on global climate models can be understood correctly. In section 3.7, we added a note:

"...in contrast to climate prediction, those CMIP5 simulations are not initialized with recent observations and suffer from natural variability not necessarily in phase with reality...".

We feel this statement should be sufficient to make justice to the GCMs. More detailed background explanations on the different features of CMIP ensembles compared to climate prediction ensembles would distract the reader from the important point that a more realistic simulation of atmospheric circulation ensures better representation of the sea ice extent.

5. The paper also discusses radiative forcing on the 2007 sea ice minimum and found inconsistence between different studies. Actually, the major inconsistence mainly results from different research area. Kay et al. (2008) looked the Beaufort Sea, while Schweiger et al. (2008) examined the Chukchi Sea where the largest sea ice loss occurred.

We have now differentiated the statement in section 4.3 to reflect the different geographical areas.

A couple of minor comments:

"In line 15 on p. 10965: ". . . gravity waves . . ." should be changed to ". . . planetary waves . . ."."

Thank you, this is done now.

"In line 5 on p. 10970: ". . . highly negatively . . ." seems need to be changed to ". . . highly positively . . ."."

We think that "highly negatively" is correct, because the sea ice thickness reduces with stronger ocean heat transports into the Arctic.

Response to Referee #2

We are grateful for the detailed comments and suggestions. The vast majority of suggestions have been accepted for the new version.

Response to Referee #3

I have a few comments and suggestions regarding the description of atmospheric processes and climate feedbacks in the review paper.

1. Role of different feedback processes in causing Arctic amplification

The authors mention a number of studies that have looked at the role of single feedback mechanisms in causing Arctic amplification, but largely overlook the literature that attempts to quantify and compare the contribution of different mechanisms. The latter includes both arguments in favor of a dominant role of surface albedo feedback (eg Crook et al, 2011 Taylor et al. 2013) and studies concluding that atmospheric longwave feedbacks are dominant (Winton 2006, Pithan and Mauritsen 2014).

Thanks for the helpful hints. We acknowledge that both the description of the individual processes and the interaction between the different processes, although briefly discussed in the original version, benefit from an extension. We have substantially modified section 2.1 (arctic amplification) by describing the processes in more detail and by discussing their relative importance including possible reasons for opposing findings. As this is a broad review article and the processes responsible for the Arctic amplification are among many others to be described, we cannot discuss the processes at the same level of detail as in specialized articles.

The latter studies might also call into question the claim that "The Arctic sea ice is the central and essential component of the Arctic climate system" - in my view, that claim could be made more specific or better explained.

That statement on the role of the sea ice in the Arctic needs to be compact in the abstract of the Article. Taking into account the reviewers comments, we modify it to "The sea ice is the central component and sensitive indicator of the Arctic climate system.", which is a weaker message and indicates a less active role of the sea ice.

2. The lapse-rate feedback

The explanation of the lapse-rate feedback and its contrast between low latitudes and the Arctic (mostly p. 10935) could be improved: The lapse-rate feedback is negative in the tropics (which dominate the global mean) not just because of mixing, but because moist convection keeps the tropical atmosphere close to a moist adiabat. As the climate warms, the moist adiabat becomes steeper, leading to stronger warming in the upper troposphere than at the surface. I also believe that a clear definition of the lapse-rate feedback as the change in TOA radiation caused by warming that deviates from the vertically uniform reference response (the Planck feedback) is missing in the manuscript.

In the new version we are describing the Planck feedback and we are refining the definition of the lapse rate feedback.

3.The planck feedback

The contribution of the Planck feedback to Arctic amplification , i.e. the weaker increase in blackbody radiation per unit warming at colder temperatures is not mentioned in the review. That contribution is smaller than that of the lapse-rate feedback, but still important enough to be mentioned (cf fig. 2a: http://www.nature.com/ngeo/journal/v7/n3/full/ngeo2071.html)

The Planck feedback is now mentioned as the vertically uniform contribution to the temperature feedback.

4. changes in atmospheric moisture

At several instances in the text, the authors discuss changes in atmospheric moisture as a result of changes in evaporation. However, changes in moisture largely follow temperature changes at constant RH, both in the Arctic and globally (nicely explained by Isaac Held: http://www.gfdl.noaa.gov/blog/isaac-held/2011/06/29/13-the-strength-of-the-hydrological-cycle/). If the Authors refer to changes in RH, this should be made more explicit in the text.

The current manuscript discusses changes in atmospheric water vapour content and cloudiness over the Arctic (section 4.3) which could be due to increased evaporation or due to changes large scale advection. Different studies (see section 4.3) give increasing trends in vertically integrated water vapour content and seasonal and regionally varying decreasing or increasing trends in evaporation. Results are based both on observations (using specific humidity) and reanalysis. Several studies also find changes in clouds. Thus, the RH must have increased. Effects of increased cloudiness and specific humidity are now mentioned e.g. in the first paragraph of section 4.2 and in the 5th paragraph of section 4.3.

Specific comments:

5. p 10932, ll 11ff: Manabe and Wetherald also mentioned the role of the vertical structure of warming, i.e. the lapse-rate feedback

Thanks, this is included in the new version

6. p 10936, l 15: water vapour feedback is indeed stronger in the Tropics than the Arctic and does not lead to AA, “even” and “likely” could be omitted here (see figure from comment 3)

This is done in the new version

7. p 10936, ll 21 ff: The referenced papers do not show that cloud feedbacks alone can cause Arctic amplification, since they disable the surface albedo feedback but not the lapse-rate feedback, planck feedback or changes in atmospheric heat transport.

We have modified the paragraph for the new version of the manuscript:

“The cloud feedback contribution is potentially capable of explaining an Arctic amplification on its own without the support of a sea ice albedo feedback. This is indicated in model studies with sea ice-albedo-feedback disabled by a fixed albedo (Langen and Alexeev, 2007; Graversen and Wang, 2009). Among the remaining mechanisms, the combined cloud feedback and the water vapour feedback (which not in itself generates an amplification) play the leading roles. Similar to the lapse rate feedback, the effect is supported by a generally stable stratification without convective mixing in the Arctic atmospheric boundary layer, hindering vertical mixing of humidity and thus keeping up increased humidity at lower levels. A more complete summary of the mechanisms involved in the Arctic amplification is given by Serreze and Barry (2011) and Pithan and Mauritsen (2014).”