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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Recent advances in understanding the Arctic climate system state and change from a sea ice perspective: a review" *by* R. Döscher et al.

Anonymous Referee #1

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Research on Arctic sea ice has developed rapidly in recent couple of decades. Sea ice is highly sensitive, subject to strong interactions with atmosphere and ocean processes, as well as external forcing. Researches on sea ice are therefore largely distributed across different climate system components, and the research results are very diversified though some agreements have been reached. It is really a challenging work to review progresses of sea ice researches. The authors here took this challenge and the review paper covers a large amount of literature published during last decades. The paper deserves a publication after a careful revision.

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.

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.

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. Towards this end, one colleague has put comments online about feedback processes. 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: Kwok, R., G. F. Cunningham, and S. S. Pang (2004), Fram Strait sea ice outflow, J. Geophys. Res., 109, C01009, doi:10.1029/2003JC001785; Rigor, I. G., J. M. Wallace, and R. L. Colony (2002), Response of sea ice to the Arctic

Oscillation. J. Clim., 15, 2648 – 2668; Zhang, X., M. Ikeda, and J. E. Walsh (2003), Arctic sea ice and freshwater changes driven by the atmospheric leading mode in a coupled sea ice-ocean model J. Clim., 16, 2159-2177.

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.

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.

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.

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.

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.

A couple of minor comments:

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

waves . . .".

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

Interactive comment on Atmos. Chem. Phys. Discuss., 14, 10929, 2014.