General Comment to reviews of

“Arctic Rapid Sea Ice Loss Events in Regional coupled Climate Scenario Experiments”

by Ralf Döscher and Torben Koenigk

We thank both reviewers for helpful ideas to improve the manuscript.

Much of the reviewers criticism is focused on missing figures of actual SLP instead of only showing SLP anomalies (comparing the event with a 10-year reference period). We agree that SLP anomalies do not inform about the absolute flow direction and its changes. Still, SLP anomaly plots have its value in showing relative changes of flow direction. The flow component along anomaly contours has increased compared to the reference field in the specific comparison. We admit that we have been too unspecific in distinguishing relative flow direction from changes of absolute flow direction. The formulations in the text will be adjusted accordingly. In many cases we will show actual SLP fields in addition to the anomalies. Partly, this leads to modified conclusions in detail. As a result, it becomes now even more clear that negative ice thickness anomalies in many cases are compatible with advection of air from warmer areas.

A revised version of the manuscript and more specific response to the reviewers will be submitted after the given deadline in 1 month after Oct. 12.

Replies to major comments of referee #1:

“(1) The paper is mostly well written, its structure is sound, and its topic is scientifically relevant. For the reader, however, it is partly impossible to reproduce the conclusions from the figures presented in the paper. For instance, the conclusion that rapid ice loss events are often accompanied or even caused by the inflow of warm air from the Atlantic sector cannot be deduced solely from difference/anomaly plots of SLP. These plots do not indicate the basic direction of the flow; they only show the change relative to the mean flow of the reference period. If the mean flow had a strong component from north to south, the difference would indicate a weaker outflow of (cold) air into the Atlantic sector. Such flow conditions cannot be excluded a priori and would invalidate the conclusion. To demonstrate that it is a matter of warm air inflow in this particular case (as well as in other similar cases), I would suggest showing the actual SLP fields of the rapid ice loss events in addition to or instead of the difference fields. ”

As stated above, we agree that SLP anomalies do not inform about the absolute flow direction and its changes. The formulations in the text will be adjusted accordingly. In many cases we will show actual SLP fields.

“(2) Another questionable point is the interpretation of the 2 m air temperature anomaly fields in terms of advective warming. Especially in the cold season (winter as well as autumn), the 2 m air temperature over sea ice is more strongly affected by the sea-ice conditions (ice thickness and concentration) than by advection. Anomalies in the 2 m air temperatures over sea ice can best be seen as an indicator of sea-ice anomalies – and this is clearly visible in most of the figures. In contrast, any contribution of advective warming can hardly be identified given the dominant signature of ice anomalies. For this purpose, it would have been better to have shown the temperature in higher levels (e.g. 850 hPa) where the impact of the surface conditions is minor. I do not suggest replacing all the 2 m air temperature figures, but I think it might be advisable to reconsider the statements on the indication of advective warming.”

We agree that sea ice conditions have a strong impact on 2 m air temperature. However, the geographical fit between sea ice thickness and T2M pattern is not perfect, opening for other influences. We find that long wave radiation at the surface as a more vertically integrated quantity can be indicative even for advective temperature anomalies. The text concerning advectice warming will be reworked and fields other than T2M will be included in the discussion.

“(3) Page 2333, Line 3: “Our six experiments were initially designed as a sensitivity study and climate change projection experiment. The set-up varies in forcing and sea ice parameters.” Precisely because the experiments were designed as a sensitivity study, it would be interesting to know how sensitive the model reacts to the varying forcing and sea ice parameters. As one can see from Fig. 1, rapid ice loss events in the experiment with thicker ice (E2) are less pronounced. I can imagine that there are also differences in the prevailing mechanisms for initiating such events. ”

This question is partly addressed in Koenigk et al. (2011) which partly uses identical model runs: “The deviations in the response between the regional simulations corresponding to the same global model are small. Using ocean GCM forcing at the lateral domain boundaries compared to climatology leads to a moderate warming of the Arctic Ocean ... and a slightly enhanced sea ice melting. Most of the additional heat input into the ocean is lost to the atmosphere or distributed in deeper layers before it can affect the ice-covered Arctic Ocean. The regional simulations using salinity flux correction instead of salinity restoring show particularly in the nearer future (2020–2040) a tendency to an amplification of the response. This signal is most pronounced in the Barents Sea ... . A faster reduction of sea ice in the Barents Sea leads to enhanced ocean to atmosphere heat fluxes and locally reduced SLP. As a consequence, anomalous winds transport more sea ice to the south in winter time and lead to a temperature reduction in the Greenland Sea in 2020–2040 compared to 1980–2000. ”

The model run with thicker ice (E2) is giving largest sea ice extents during most of the simulated 100 years. RILE events of E2 are not less pronounced, but first RILE events occur at a relatively late time compared to the other ensemble members. In the upcoming revised version of our paper, we group the six events with thickest ice in one composite and compare with events under the conditions of thinner ice.

“(4) Page 2333, Line 23: “We are excluding events with strong variability during the 10-yr-reference period.” What exactly does strong variability mean in this context? ”

A better definition of the events will be given in the upcoming revised version.

“(5) Page 2335, Line 27: “Those are the conditions of sea ice loss events during a certain time period of strong large scale control. The anomalies shown explain the forcing mechanism of the events, but not why all ensemble members generate an event at about the same time.” When there is large-scale control, as the authors diagnose, it should not be surprising that all ensemble members generate an event at about the same time, given that the large-scale control may enter the regional model domain via the lateral boundaries. ”

The large scale forcing given at the lateral boundaries can potentially control the inner model behavior if not internal non-linear processes dominate and diminish the regional effects of large scale forcing. When downscaling a global model by a regional model, the circulation in the regional model can diverge strongly from the GCM circulation in the same region depending on the regional domain size (Leduc and Laprise, 2009 and Rummukainen 2010) and the large scale setting. On interannual time scale and in out model's rather large domain, Arctic-internal process are as important for the interior behavior as the large scale forcing (Döscher et al, 2010). Large scale control of regional events during certain time periods indicates a interior mechanisms that might be dominant during that time period but not during others. We modified the sentence in question to avoid misunderstanding:

“Those are the conditions during a certain time period of strong large scale control of sea ice loss events. The anomalies shown explain the forcing mechanism of the events, but not why the mechanism is so powerful that all ensemble members actually generate an event at about the same time. Most of the 30 events do not show such a synchronicity. “

“(6) Page 2336, Line 10: “Taking into account that thick ice off Siberia might be an unrealistic feature, given existing high pressure biases (Cassano et al., 2011) . . . ” To my mind, the high pressure bias in the Pacific sector is a specific feature of Polar WRF and not a general feature of climate models, as the sentence implies due to the reference. If the existence of a high pressure bias is just a common feature of RCAO and WRF, it should be indicated as such. Of course, it would also be interesting to know the reason for this bias. ”

Arctic high-pressure biases are not only occurring in polar WRF and RCAO. The bias is typical even in global climate models (GCMs). The problem is documented in Chapman and Walsh (2007), Vancoppenolle (2008) and Blanchard et al. (2011).

It is speculated about the reason for this bias. Possible explanations are a lack of low pressure system advection into the Arctic, or problems in the radiation schemes. Another explanation is the effect of insufficient treatment of ice classes (Mårtensson et al. 2012, Vancoppenolle, 2008)

In the revised manuscript, we add the references to support the claim.

“(7) Page 2339, Line 15: “This can potentially contribute to the still warmer T2M, but horizontal patterns do not coincide well.” On closer inspection of Fig. 3b, the winter T2M anomaly coincides quite well with the combination of contributions of the ice thickness anomaly (Chukchi and East Siberian Seas) and the SIC anomaly (between Svalbard and Severnaya Zemlya). It is much more difficult to identify any indication of a signal storage in the ocean surface temperature or of advective warming from southern latitudes, as suggested several times in the paper. ”

The winter discussed here is the winter after the summer event. Indeed there is a coinciding between T2m anomaly on the one hand side and SIC (between Svalbard and Severnaya Zemlya) and thickness (Chukchi and East Siberian Seas) on the other hand side. In addition, the concentration anomaly of the previous fall coincides well with the winter T2m anomaly (Chukchi and East Siberian Seas). In the manuscript, we are not showing SST, but reduced ice concentration during fall must be connected to warmer SST, most likely due to the summer event which allows for anomalous heat storage in the ocean. We need to avoid additional figures. Therefore we do not show the SST anomaly. In the upcoming new version of the paper, we modify the sentence in the following way:

"The T2M anomaly coincides partly with the negative SIC (between Svalbard and Severnaya Zemlya) and partly with the thickness anomaly (Chukchi and East Siberian Seas). In addition, the T2M warming is reminiscent of the previous fall SIC anomaly pattern, indicating a possible signal storage in the ocean surface temperature, later converted to a thickness anomaly. The SLP anomaly shows lower pressure over large parts of the Arctic and Nordic Sea area. The pattern is reminiscent of the positive phase of the Arctic Oscillation. The specific shaping tends to decouple the Arctic from the Pacific sector circulation."

We are dropping the speculation about post-event meridional flow here.

"(8) Page 2343, Line 3: "For this composite we find a winter SLP anomaly pattern quite similar to the "T2M > 3.85 K" composite, indicating that winter atmospheric warming by atmospheric circulation indeed plays a major role for the most extreme summer sea ice concentrations." In this case, it is really needed to show the atmospheric circulation, for instance in the form of the actual SLP field. "

The upcoming revised version of the paper shows even actual SLP fields. For this composite it appears that the circular Beaufort high is replaced by an elongated high pressure bridge between Siberia and North America. This means that an anticyclonic almost closed circulation is replaced by stronger meridional components, partly originating from warmer areas. All composites will be re-discussed in the light of actual SLP fields.

"(9) Page 2343, Line 22: "In addition to the pure DA pattern, it brings warm air from the Pacific to the Arctic." As the authors has stated earlier, this is exactly what a positive DA does. It seems to me that there is no significant difference between the DA and the tripod-like pattern in the Pacific sector of the Arctic."

The referee is correct. The sentence is modified to:
"As the pure DA pattern, the tripod in the Pacific sector brings warm air from the Pacific to the Arctic."

"(10) Page 2345, Line 18: "An elongated low pressure anomaly centered over the pole is connected to ice drift away from the Chukchi Sea towards the Canada Basin." I do not fully understand this conclusion. As I see it, the low pressure anomaly is likely to weaken the Beaufort Gyre, resulting in slower ice drift from the Canada Basin towards the Chukchi Sea, unless the usually anticyclonic Beaufort Gyre reverses to cyclonic circulation. If there was a reversal of the Beaufort Gyre in summer 1997, this would be an interesting side-effect and should be mentioned."

Our language was not emphasizing the anomaly character of the discussed fields. Indeed, the Beaufort high-pressure is weakened during spring and summer. During the summer before the 1998 event, atmospheric driving is actually directed directly from Alaska towards Greenland. The discussion of this case 1 is largely reformulated under consideration of absolute SLP fields during the seasons and the respective reference seasons (average of the 10 years before the event).

The Beaufore gyre question is addressed in the revised text as follows:

"An elongated summer low pressure anomaly centered over the pole is connected to ice drift away from Alaska towards Greenland. In terms of absolute SLP, this means a reduction of the Beaufort high with a remaining anticyclonic winds off the American and Northern Greenland coast"

"(11) Page 2348, Line 26: "We see an event which is preconditioned by early sea ice anomalies in the seasons before and later maintained by supportive wind patterns. The start signal of the actual event year is given in spring with opening up coastal areas and continued by strong air inflow from the Atlantic sector." Neither the wind patterns nor the strong air inflow from the Atlantic sector can be deduced from the figure. Furthermore, the SLP anomaly patterns in the various seasons are so different that it would be a remarkable coincidence if all of them were supportive of the event."

This formulation is a summary of processes described in more detail in the preceding paragraphs. Both the complete description of case 26 as well as the summary is now revised and more specific. Additional SLP figures do now allow for a better verification of the different factors influencing the event. The revised summary is now:

"We see an event which is preconditioned by early sea ice anomalies in the seasons before and later maintained by supportive wind patterns during specific periods. The start signal of the actual event year is given in spring with opening up coastal areas and continued by strong air inflow from the Fram Strait area."

"(12) Page 2349, Line 25: ". . . long wave downward radiation . . . is also affected by warmer air of southern origin appearing over the areas during winter." During winter, near-surface air of southern origin does not necessarily need to be warmer, especially when coming from the cold continental regions of Asia or North America. I would agree if the air was of Atlantic or Pacific origin, since maritime air is not only warmer but also more humid during winter. Higher humidity might play an important role as well."

In specific cases, we can actually see anomalous warm air over a continent with an advective connection to the Arctic ocean (e.g. case 16 during the two winters before the summer event). Clearly, effects are more distinct when the inflow comes fro a maritime area. Those cases are now discussed in more detail. As the referee states, higher humidity might also play a role. We increase the number of pictures with addition SLP and radiation fields. It appears that indeed the inflow from both Atlantic and Pacific sector is intensified. We feel we should not further increase the number of pictures. Thus we refrain from showing pictures of humidity.

All cases will be described in more detail in the light of newly shown actual SLP and radiation patterns.

"(13) Page 2350, Line 6: "In at least one case (case 26) we see indication for . . . " It would be

interesting to know whether the mechanisms for initiating rapid ice loss events change in their importance over time. Given the fact that the study is based on a climate change projection experiment, I am wondering why this key aspect remains undiscussed."

This is an interesting question. A careful examination would need another set of figures and would extent the paper. Here we chose to add two composites with events before and after the year 2035. The basic lesson from that comparison is that seasonal mean SLP anomalies are smaller during the later time period, indicating a reduced role of seasonal advective patterns under the conditions of the most thin ice.

"(14) Page 2352, Line 4: "Warmer air temperatures are caused by atmospheric circulation anomalies . . . " This seems natural, but should be specified and substantiated by additional or modified figures. "

In the upcoming revised version, the statement refers to specific cases and composites discussed earlier in the manuscript with the help of figures.
The statement now reads:

"On average, rapid reduction events are characterized by increased temperatures over the ice during the winter before a summer event (Fig. 3a). Warmer air temperatures are mostly reflecting sea ice thickness anomalies, but are in many cases also consistent with atmospheric circulation anomalies connecting warmer areas with the Arctic ocean and impede sea ice growth, thus leading to reduced thickness already during that winter. Examples are the winter 1997/98 in case 1, the two winters before the summer event in case 16, and the 1923/24 winter in case 26. ... In cases with increased winter surface temperature only over the ocean, anomalous meridional winds are often consistent with ice thinning in the areas of anomalous off-shore winds, indicating ice drift. Examples for the latter are the last winter before the summer event in case 26"

"(15) Page 2353, Line 24: "In observations, both patterns (DA and PNA) are not correlated (e.g. Overland et al., 2008). Thus oscillations of the PNA can potentially support or damp the amplitude of the DA which represents an important RILE forcing." Two distinct EOF modes do not correlate by definition. Therefore, it is unclear to me how the PNA can support or damp the amplitude of the DA. But besides that, the summer PNA itself might contribute to rapid ice loss events as discussed by L'Heureux et al. (2008), indicating a further possibility of large-scale atmospheric control."

Our formulation was misleading. We have two oscillation patterns based on EOF analysis in different geographical domains. Thus, situations can occur during times with e.g. high pressure over Northern North America due to PNA that coincides with either low or high pressure from the DA oscillation. Thus oscillations of the PNA can potentially enhance or reduce the impact of the DA for the total pressure field or vice versa.

(16) "Figures 3a, 3b, 6–9: All these figures consist of so many tiny subfigures for slightly different geographic areas and with different signal colors for different variables that it is difficult to capture the essence. I would suggest showing all variables for completely identical areas using identical colors for positive, negative, and zero anomalies. The authors should also consider reducing the number of subfigures, especially with respect to Figures 7–9. "

We completely understand this desire. Part of the problem might arise from the standard layout of the discussion paper generated during upload. The original figures are actually larger and thus can convey the messages better. The different subareas arise from the different component model domains. The atmosphere's domain is larger than the ocean's. In addition we have the dilemma of a need to reduce the number of subfigures while at the same time show more absolute SLP fields.
We chose this solution: The number of subfigures in Fig. 3a and 3b is unchanged, but the total number of figures is increased. The domain limits for SLP and T2m are unified in a way that most sub-figures are now zoomed. Even in the new setup, we chose to show sea ice on a smaller area for the sake of better visibility. Showing areas south of the ice margins would not be helpful.
We further introduce an additional figure with absolute SLP values.

Replies to major comments of referee #2:

The reviewer is throughout the comments suggesting an analysis much more detailed than what can be the scope of the present paper. We are aiming at a qualitative analysis of a larger number of rapid ice change events based on seasonal means.

Several suggestions of the reviewer would require additional work with huge amounts of 4D-data (such as atmospheric heat flux figures and budgets), and a significant expansion of the paper. We conform with the view that the additional information would be helpful in better quantitatively understanding processes, however they are not essential for the qualitative conclusions of the paper, and the additional work required cannot be done in the time given and is neither the intention and scope of this paper.

However, we are confident that we can address most comments of the reviewer by showing (as also reviewer #1 suggested) more absolute values of the variables instead of only showing anomalies. We also add additional figures, clarifications and discussions.

We greatly appreciate the careful and inspiring work done by the reviewer.

General Comments by referee #2:

"1. The model used has a number of limitations (e.g. lateral boundary conditions, surface salinity restoring/flux correction, representation of clouds, precip/snow, sea ice and ocean processes) which could affect its simulation of the relative importance of specific factors, forcings and feedbacks leading to RILE events. It would help to include a few figures such as mean seasonal SLP, SST and sea ice thickness maps to demonstrate the realism of model surface climate (see comments on L109-110) and modeled versus observed to-date RILEs."

As any model, this model has limitations and shortcomings. Thus, mechanisms inferred from model results are not necessarily identical to natural processes. Even models with a real-like surface climate can in principle fail to describe changes, or changes could be described for wrong reasons.

The models performance has been described in two previous papers (Döscher et al., 2010 and Koenigk et al., 2011) including overall sea ice extent, volume and T2M. In response to the reviewers request, we add 2D figures of sea ice concentration, thickness and SLP as a means of the period 1980-2000 together with biases to better illustrate the underlying climate in the revised version of the paper.

The focus of this paper is on a description of processes with respect to rapid ice changes under the conditions and limitations given by this model, which includes sea ice generally thinner than recently observed and atmospheric temperatures warmer than today.

As stated already in the initial version of the paper, a major limitation of the model is seen in sea ice thickness fields generally thinner than observed during recent climate. In addition, sea ice is much too thick in the East Siberian Sea. Reasons have been named before: In our model, ice in the Siberian Sea tends to be artificially thick as a result of insufficient treatment of ice classes (Mårtensson et al. 2012) and due to a high pressure bias over the Eurasian part of the Arctic ocean. This problem is shared with several GCMs (e.g. Chapman and Walsh (2007), Vancoppenolle (2008) and Blanchard et al. (2011)). Thus, results of this paper might help interpreting GCMs. The problem is also shared with Arctic-WRF (Cassano et al., 2011).

This model deficiency of generally too thin ice and unrealistically thick ice off Siberia, must have effects on results of this paper. Given that the artificially thick Siberian ice blocks rapid ice loss events under atmospheric circulation regimes which do not oppose that exaggerated thickness, it might be speculated that a more realistic geographical ice thickness distribution could lead to even more frequent rapid ice loss events in our model. Taken that into consideration, the mechanisms described in principle in this paper are rather applicable to conditions of thinner ice such as we observe after 2010 and might see during the coming years.

The principle of academic studies allows for experimental setups which are limited and unrealistic to an extent. We describe the setup and dominating process under the specific conditions of the setup. Such studies are as legitimate as idealized water-tank experiments or one-dimensional studies. We stress the experimental character of our scenario simulations. Those are not describing reality, but possible climates under certain given conditions.

The sea ice thickness problem is solved in a forthcoming development version of the model and results from coming experiments will likely be presented in a later paper.

The reviewer views lateral boundary conditions as another model limitation. Such boundary conditions are a necessity for regional models. Boundary data is retrieved from GCM runs to reflect the conditions of global climate change scenario.

Many models are suffering from drifting salinity. Several AOMIP models apply flux corrections “necessary to make model, prescribed forcing data sets and climatological surface salinity compatible with each other” (e.g. Gerdes and Köberle, 2007). We have shown that the choice between salinity restoring and flux correction does not have a major influence on ice cover results, however, flux corrections are allowing for more consistent salinity variability (Koenigk et al., 2011).

“2. Conclusions are made several times regarding inflow of warm air from lower latitudes based on plots of SLP and T2M anomalies. While this might sometimes be the case it is hard for a reader to follow such arguments without actual maps of SLP, T2M as well as sea ice concentration and thickness. A common argument made throughout the paper is that winter SLP anomalies bring warmer air from lower latitudes, which result in negative ice thickness anomalies, in winter. Such an argument,

if it holds at all requires further evidence (see detailed comments below). "

We agree that SLP anomalies do not inform about the absolute flow directions. Still, SLP anomaly plots have its value in showing relative changes of flow direction. The flow component along anomaly contour lines has increased compared to the reference field in the specific comparison.

We admit that we have been too unspecific in distinguishing relative flow information from absolute flow direction changes. The formulations in the text are adjusted accordingly. In many cases we show the actual SLP fields in the revised version of the paper. As a result, it becomes now more clear that advection of air from warmer areas is compatible with ice thickness anomalies in many cases. Even during wintertime, changed winds are compatible with reduced ice growth. In some cases, winds from open water regions carry heat which affects ice formation during wintertime.

"3. Sections 4.4 Composites and 4.5 Individual Cases can't be properly evaluated as the figures discussed in those sections are not described with enough details (Fig.6) or are incomplete (Fig.7-9) in addition to the need for clarification on the text there. "

In the revised version we have added maps of actual SLP to better interpret anomaly maps. The text in sections 4.4 and 4.5 is updated in the revised version with more detail concerning circulation differences.

"4. Evidence exists (e.g. Screen et al., GRL, 2012) that largest warming in the Arctic atmosphere is near the surface and it is related to loss of sea ice and SST changes. Other studies (e.g. Jackson et al., JGR, 2011) suggest that the Arctic Ocean has been warming as well, especially in the western Arctic where sea ice has retreated most recently and below the surface mixed layer. Ice-ocean interactions appear increasingly important (e.g. this summer 2012) and they should be considered in modeling studies as well, especially if model representation of such processes and feedbacks is limited and known (see #1).
My recommendation is to include some discussion of 'missing physics' in the model and how this may affect the simulated RILEs and conclusions of this paper. "

The observational situation concerning the vertical location of largest atmospheric warming is not completely clear. Although largest warming is certainly found near the surface during the colder part of the year, there is indication for a maximum warming higher up in the atmosphere (Graversen et al. 2008). Atmospheric surface temperatures are often dominated by sea ice conditions. This is even the case in our model. Often we see largest T2M anomalies roughly coinciding with sea ice thickness anomalies. However, the geographical fit is far from perfect, which points to a role for atmospheric conditions as well. Overland et al. (2008), based on observational data, find atmospheric advective contributions to play an important role for SAT anomalies. "... anomalous geostrophic winds" for 2000–2007 often tended to blow toward the central Arctic, a meridional wind circulation pattern. In spring 2000–2005, these winds were from the Bering Sea toward the North Pole, whereas in 2006–2007 they were mostly from the eastern Barents Sea. A meridional pattern was also seen in the late 1930s with anomalous winter (DJFM) SAT, at Spitzbergen, of greater than +4◦ C. Both periods suggest natural atmospheric advective contributions to the hot spots with regional loss of sea ice."
Graversen et al. (2011) find that "in summer 2007 there was an anomalous atmospheric flow of warm and humid air into the region that suffered severe melt."

Several studies concerning Arctic Ocean warming are addressing the Atlantic layer (e.g. Polyakov et al. 2005 and 2011). Sea ice is generally isolated from deeper ocean layers by a cold fresh layer underneath the ice. Impact of deep ocean warming on surface waters is rather unclear. In the western Arctic, studies point to a subtle interaction between pulse-like Bering Strait inflow and interaction with wind-driven surface mixing (Woodgate et al. 2010). For the Eurasian Arctic, local warming of surface layers allows for the speculation of Atlantic water mixing up into the surface, although mixing rates are very small (Polyakov et al. 2011)

The model is missing an open boundary in the Pacific sector at the Aleutian Island chain. Still, flow between the Bering Sea and Chukchi Sea is possible through the open Bering Strait. Ocean advection of positive heat anomalies as suggested by observations, is not seen in the model. Simulated ocean surface warming in the models western Arctic is always a response to reduced sea ice concentration. This is illustrated already in the initial version of the paper: “We also tested the idea of possible upward transport of ocean heat by vertical mixing in response to reduced ice concentration. Such a process was not found in this model although observations indicate import of warm ocean water from the Pacific Ocean (Woodgate, 2010) and proximate inclusion in vertical mixing. While those observed results are under discussion, we cannot expect to find them in the model due to coarse resolution and insufficient Bering Strait inflow”

RILEs in our model are evoked frequently without a major contribution initiated by the ocean. Rather than contradicting observation-based results on ocean influence, our model-based results illustrate the possibility of a prominent influence of atmospheric circulation variability. Even in the real world, variability in the atmospheric circulation has played an especially prominent role for rapid ice loss (e.g. Serreze and Barrett, 2011, Overland et al., 2008 and Graversen et al., 2011). Thus, the mechanisms found in this paper should be seen as possible contributors to RILEs in a real world. They are most likely not the only contributors to real-world ice loss events.

A discussion of the consequences of model shortcoming and limitations together with other unrealistic model features such as generally too thin sea ice, is already included in the “summary and discussion” section. In the revised version we will refocus this discussion to make the limitation of results even more clear.

References in addition to the initial version of the paper

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