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Interactive comment on “Arctic rapid sea ice loss events in regional coupled climate scenario experiments” by R. Döscher and T. Koenigk

Anonymous Referee #1

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The paper comprises an interesting and innovative study on potential mechanisms for the occurrence of rapid Arctic sea ice loss events. Based on a six-member ensemble of climate scenario simulations with the Rossby Centre Atmosphere Ocean model, the authors have found 30 of such events and discuss common features as well as differences between them. It is concluded that anomalous atmospheric circulation conditions and preconditioning of the sea ice during the seasons and years before the event are responsible for their occurrence. In particular, anomalous inflow of warm air from the Atlantic sector during summer is identified as one of the main reasons for initiating rapid ice loss events.

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General comments

(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.

(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.

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Specific comments

(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.

(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?

(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.

(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.

(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 win-

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ter 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.

(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.

(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.

(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.

(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

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remarkable coincidence if all of them were supportive of the event.

(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.

(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.

(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.

(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.

(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 re-

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spect to Figures 7–9.

Minor remarks and corrections

(17) Page 2329, Line 16: “Kwok” instead of “Kwock”.

(18) Page 2331, Line 14, 23, etc.: “(Döscher et al., 2009)”: As I see it, the final version of this paper was published in 2010.

(19) Page 2331, Line 20: “Fig. 3a”: One could also refer to Fig. 2 which appears earlier.

(20) Page 2332, Line 6: “Depending” instead of “Depeding”.

(21) Page 2333, Line 14: Maybe one should refer to Table 1 at this place.

(22) Page 2333, Line 16: “A rapid sea ice loss event (RILE) in this study is a temporary reduction of the annual minimum of sea ice extent during summer.” The term ‘temporary reduction’ sounds like it would be an unusually weak event. Maybe this basic definition should be reworded.

(23) Page 2337, Line 25: “During summer (JAS, AVD, Fig. 3a, third row) . . .” What is the meaning of AVD?

(24) Page 2341, Line 9: “(Fig. 5, mid panel)”: There is only an upper and a bottom panel.

(25) Page 2342, Line 9: “see Table 2” instead of “see Table 1”.

(26) Page 2342, Line 18: “. . . in section TWOD.” There should be a number in place of TWOD.

(27) Page 2342, Line 28: Maybe Δ_{SIC} instead of “d_SIC”.

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- (28) Page 2343, Line 8: Maybe Δ_{extent} or Δ_{SIE} instead of "d_extent".
- (29) Page 2343, Line 19: "... in section XX" (see above)
- (30) Page 2343, Line 29: "Fram Strait" instead of "Fram Straits".
- (31) Page 2344, Line 18: "dependent" instead of "dependet".
- (32) Page 2346, Line 26: "(Sect. DA)" (see above)
- (33) Page 2347, Line 9: "Winds drive away the ice from and Chukchi Sea." Maybe this should read: "... from the Chukchi Sea."
- (34) Page 2348, Line 5: "positive DA amplitude" instead of "positive AD amplitude".
- (35) Page 2348, Line 22: "compared to the 10-yr reference period" instead of "compared the 10-yr reference period".
- (36) Page 2350, Line 4: "this finding is based on seasonal means" instead of "this finding is based seasonal means".
- (37) Page 2350, Line 18: "as necessary" instead of "as a necessary".
- (38) Page 2350, Line 19: "help" instead of "helps".
- (39) Page 2351, Line 1: "trend of summer" instead of "trend of of summer".
- (40) Page 2352, Line 23: "anomaly" instead of "anomalies".
- (41) Page 2354, Line 12: "longer than 3 yr" instead of "longer that 3 yr".
- (42) Page 2354, Line 29: "feedbacks" instead of "feddbacks".
- (43) Page 2355, Line 17: "due to" instead of "due to to".
- (44) Page 2355, Line 28: "Serreze and Barrett, 2011" instead of "Serezze and Barret, 2010".
- (45) Page 2356, Line 14: "preconditioning" instead of "predonditioning".

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- (46) Caption of Fig. 2: summer SLP and winter SLP seem to be mixed up.
- (47) Caption of Fig. 3c: The unit of the bottom melting rate is missing.
- (48) Caption of Fig. 4: (a) “sea level pressure” instead of “sea ice pressure”. (b) There are only two panels. (c) The units are missing.
- (49) Caption of Fig. 5: (a) There are only two panels. (b) It remains unclear why there are partly multiple numbers of occurrences for each of the EOF-PC amplitudes in the upper panel.
- (50) Figures 7–9: Color bars are missing. The captions suggest that the figures show seasonal means rather than seasonal anomalies. I guess the latter is true.

Interactive comment on Ocean Sci. Discuss., 9, 2327, 2012.

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