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Discussions

Interactive comment on "Limitations of a coupled regional climate model in the reproduction of the observed Arctic sea-ice retreat" *by* W. Dorn et al.

Anonymous Referee #2

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Review of "Limitations of a coupled regional climate model in the reproduction of the observed Arctic sea-ice retreat " by W. Dorn, K. Dethloff, and A. Rinke

In my view this is a very useful manuscript illustrating uncertainties in Arctic climate models. It should be published after giving the authors a chance to reconsider certain formulations and interpretations of their findings. See my detailed views below.

The finding that the "ability to reproduce observed summer sea-ice retreat depends mainly on two factors: the correct simulation of the atmospheric circulation during the summer months and the sea-ice volume at the beginning of the melting period" is well in line with earlier findings (e.g. by Kauker et al., 2009) and thus strengthens knowledge of seasonal processes. In addition, valuable results with respect to interannual signal storage and internal vs. external driving of processes are presented.

The article clearly supports the idea that a tipping point for Arctic sea ice is not existing, which is also very well in line with recent research. Maybe it is worthwhile to highlight this point even more to create awareness on existing misinterpretations of the tipping-point concept.

I cannot completely agree with the paragraph page 1278, 23 ... : "The conclusion is that the large-scale atmospheric forcing via the lateral boundaries determines the time of occurrence of high-ice and low-ice periods, while the initial ocean and sea-ice state determines the extent of the response to the forcing via internal feedbacks. The initialization of the model with more realistic ocean and sea-ice states might therefore be an essential condition for more realistic simulations of the total volume of Arctic sea ice." I fully agree with the first part that the large scale forcing determines the timing of specific periods. But concerning the role on initialization, don't we need another experimental setup to determine whether the initialization dominates the total ice volume of a 60-year integration? This could be e.g. an ensemble with several similar thin initializations compared to another ensemble with several thick initializations. By doing so, we would be able to establish statistical support. It would be interesting to see the two ensembles behavior at the bifurcation point in 1960. I am not suggesting to do such additional runs for the current paper, but maybe the authors consider modifying the formulation of the sentence above.

Page 1280, 1-7: The coupled model's summer sea ice extent shows quite some agreement with observations after 1980. The overall level of ice extent is realistic. Year-to-year variability cannot be expected to be covered by the large model domain. As the authors state, internal variability plays a role (SDR=1.79). The model also shows the observed drop in summer sea ice extent after 2000, although delayed by two years. A perfect timing of rapid ice drop events cannot be expected for the same reason as stated above. Thus, overall the models performance after 1980 is encouraging. It remains the question why the model gives too little summer extent before 1980. Part of the explanation can be the coupled spinup in response to restart fields moved from different years to 1948 (?). Both ocean and sea ice need to adjust. Based on experience with other coupled models, such a spinup can take 10-20 years (i.e. Döscher et al. 2010) involving the sea ice and upper ocean adjustments. The authors might want to consider mentioning the spinup argument.

Page 1282/83: The authors document a correlation between high pressure over the Arctic ocean and strong summer sea ice extent reduction from May to September. Also, high pressure is connected to reduced ice extent compared to the previous year. It is further argued that is the high pressure anticyclonic conditions itself responsible for low ice conditions, rather than pressure gradients promoting inflow of warm air. The latter case is characterized as exception from the rule, using the observed 2002 minimum. At this point, I need to object: Considering the meridional structure in the upper rows of figures 5 and 6 (correlation of ice extent with NCEP pressure fields), there is a gradient in the correlation fields indicating a pressure gradient promoting outflow of air from the Arctic into Nordic Seas, and from the Pacific into the Siberian Sea. In contrast, the model cases show strongly negative correlations only over the Arctic ocean with largely meridional gradients. Thus, the NCEP data suggests influence from subpolar latitudes, while the model does not. I suggest to better distinguish the discussion on this page in modeled and “observed” (NCEP) cases.

Page 1286 “A realistic simulation of the atmospheric circulation during summer appears to be an essential but not sufficient prerequisite for a realistic simulation of the ice extent at the end of the summer. In contrast to the still relatively moderate ice retreat in 1995, atmospherically driven sea-ice drift can not be regarded as the crucial factor for the massive retreat of sea ice in 2007” This statement is based on the roughly realistic atmospheric circulation in all ensemble members contrasting with less realistic ice cover. However, when inspecting the mean sea level pressure patterns more closely, a general high-pressure bias compared to NCEP is visible with special emphasis on the Laptev Sea. While the NCEP circulations supports ice drift from the east Siberian Sea towards the pole and Fram Straits, that component is less intense in all simulations. In

my view this indicates that the exaggerated ice cover between the East Siberian Sea and the pole might be at least partly due to unrealistic local atmospheric circulation. So the atmospheric circulation might well be crucial. But clearly the atmospheric circulation is unlikely to represent the only influence. As the authors mention, potential influences from Pacific inflow are not represented in the model. I would suggest to rethink that paragraph an to find a more in-depth formulation concerning the role of atmospheric circulation in the 2007 case. As a side remark, I like to note that high pressure biases over the Arctic are a frequent feature in many models. (e.g. Chapman and Walsh, 2007).

Page 1289, 24-26 “The implication of this ïňĄnding is that both the description of the inner-Arctic feedback processes and the initial state need to be close to reality in order to have a reasonable chance of a realistic sea-ice simulation with coupled climate models .

There is no dispute on this statement. However this result does not rule out useful applications of coupled climate models for the Arctic. Also, I suggest to replace “reasonable chance of a realistic sea-ice simulation” by “reasonable chance of a realistic seasonal-to-decadal sea-ice simulation” to be more clear.

Further the authors state: “The two requirements are currently still unresolved problems in Arctic climate modeling that necessitate continuous improvements of the models and detailed knowledge about the actual state of the Arctic Ocean.”

I cannot confirm that the two problems are unsolved. I think the different fields of seasonal prediction to decadal prediction vs long-term climate modelling/projections are mismatched here. A known initial state of sea ice is definitely a requirement for seasonal-to decadal prediction, and this problem is unsolved.

In long-term climate modelling, we do not have the requirement to describe individual years realistically. Wrong initial states are subject to a spin-up process. Thus, the initial state of sea ice is rather not an unsolved problem for climate modelling. It is addressed by spin-up procedures. In line with the tipping-point-discussion in the manuscript, Tietsche et al. (2011) find that even a completely removed sea ice will recover back to the appropriate climate state within a few years. Of course it would be a much better solution to initialize with real conditions, and the simulations would benefit very much from various improvements of model descriptions of coupled processes.

Rather than a year-to-year timing, a realistic long-term trend and the occurrence of ice cover situations similar to the observed ones, is an important requirement to climate models. Even within the framework of a regional coupled model with realistic forcing at the outer model boundaries, realistic year-to-year variability must have limited skill due to internal non-linear interaction. It might be worthwhile to mention even the potential of the coupled model in the field of ensemble climate simulation, quantifying inherent uncertainties.

Minor remarks

page 1271: 12-15: The grammatical construction is misleading: It is not the composite in which feedbacks can be disregarded.

Page 1277, 15: The reason for large scatter during ice-high periods and low scatter during low-ice periods could be a larges scale control of ice loss (as the authors state) and a more isolated Arctic shielded from southern influence. This is the reviewers speculation.

Page 1278, 20-23: I do not understand the sentence “Even though only one ensemble member takes the high-ice path, it would agree better with the path of the real climate system, given that observational data suggest a general thinning of the Arctic ice cover over the last decades.” Please explain why the “high-ice-path” is more real given that observations show thinning ?

Page 1283, 1-2 “The atmospheric circulation in summer must consequently also play a dominant role in year-to-year changes of sea ice.” Using the word “dominating” would

imply the by far most important influence. As we see earlier in the paper, winter conditions are also very influential for the summer sea ice extent. Thus I suggest to replace “dominating” with a weaker term.

References used here but not occuring in the manuscript

Chapman, William L., John E. Walsh, 2007: Simulations of Arctic Temperature and Pressure by Global Coupled Models. J. Climate, 20, 609–632. doi: http://dx.doi.org/10.1175/JCLI4026.1

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