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The Cryosphere 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.

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The referee's main comment is the recommendation to reconsider certain formulations and interpretations. We realize that there are some statements in the discussion paper which require clarification or reformulation, and we really appreciate the valuable suggestions and comments. In the following, a detailed response to all comments is given referring to the page and line numbers in the discussion paper as done by the referee (the consecutive numbering has been added).

(Page and line numbers in square brackets refer to the revised manuscript.)

1.) General comments: With respect to the tipping-point concept and the initial conditions, we have added a few sentences to enable a better interpretation [page 7,

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lines 212–219].

2.) Page 1278, 27 et seq.: We must admit that the role of the initialization has been overinterpreted. To establish statistical support, it is without doubt necessary to conduct coordinated sensitivity experiments as described by the referee. We have removed the doubtful conclusion and have merged the rest of the paragraph with the preceding paragraph [page 7, lines 228–229].

3.) Page 1280, 1–7: We certainly do not disagree that the overall performance of the model after 1980 is not so bad from a climatological point of view, at least not so bad as before 1980. The reason for too little summer ice extent before 1980 remains an open question. We can only make conjectures on the reason: Maybe because some model parameterizations were tuned towards observations of the 1980s and 1990s, maybe because the climatological ocean boundary (Levitus climatology) is not representative for the earlier decades, or maybe even because sea-ice data from the pre-satellite era are biased or at least inconsistent with satellite measurements. A further possible explanation could be due to a widespread warm bias over Eurasia in the NCEP/NCAR reanalysis in the 1950s as noted by Grant et al. (2009). However, it is rather unlikely that the sea-ice bias originates from a spin-up problem, since all ensemble runs (except for runG) were started from restart fields of a coupled model spin-up simulation. Previous experiments with HIRHAM-NAOSIM showed that the model needs about 6–10 years to arrive at a quasi-stationary cyclic state of equilibrium (Dorn et al., 2007). This is why the ensemble runs were initialized with restart fields taken after not less than 6 years spin-up simulation. We assume that sea ice and upper ocean are then adjusted. Furthermore, runG were started with the final state of runF (with a state after 11+61 years of coupled simulation) and shows the same underestimate of summer ice extent before 1980. We have now expanded Sect. 2.2 (Ensemble simulation setup) by a detailed explanation of the spin-up purpose [page 4, lines 102–106]. In addition, the reference to the warm bias in the NCEP/NCAR reanalysis has now been given as a possible explanation for the underestimate of ice volume in the 1950s [page 7,

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lines 226–228].

4.) Page 1282/83: We agree that the correlation patterns in the model and NCEP data are similar but certainly not identical. We have now emphasized the differences in terms of the pressure gradients [page 10/11, lines 333–342].

5.) Page 1286: Also for the mean sea level pressure in 2007, we can certainly not expect identical patterns. Nevertheless, runC shows almost the identical pattern as the NCEP data (pattern correlation coefficient of 0.95). To highlight local differences in this case would be misleading, especially since such a high correlation is quite unusual given the relatively large model domain. The important role of the atmospheric circulation for the sea-ice retreat in 2007 is beyond debate. The fact that all ensemble members show a minimum in September ice extent and almost the same atmospheric circulation as the NCEP data actually confirms the importance of the atmospheric circulation for the occurrence of the sea-ice minimum in 2007. The magnitude of sea-ice retreat, however, is depending not only on the atmospheric circulation, but also on the sea-ice distribution at the beginning of the melting period. This is one of the central conclusion of the paper. We already noted in the discussion paper that all ensemble members have too thick ice in the Beaufort, Chukchi, and East Siberian Seas in March 2007 (compared to ICESat data by Kwok et al., 2009, their Figure 7). What we did not mention so far is that the observed atmospheric circulation in summer 2006 is not reproduced in any of the ensemble members, contributing eventually to a wrong preconditioning of the sea-ice cover and consequently wrong sea-ice response to the right atmospheric circulation. The reference to the conditions in summer 2006 has now been added to the discussion of potential causes for the distinct model deviations in 2007 ice extent [page 14, lines 455–459].

6.) Page 1289, 24–26: We agree, the formulations in this paragraph are not well-chosen and actually not in line with our view that the internal variability arises from the nonlinear response to any disturbance or uncertainty in the initial state and not from the distinctiveness of the initial state itself. We also realize that we have not made clear

that the mentioned implication is only true for seasonal to multi-year sea-ice simulations and not for long-term climate modeling. Further, we absolutely agree that the coupled model has limited skill in reproducing realistic year-to-year variability due to the internal nonlinear interactions. However, it can not be claimed that a regional climate model has basically no skill in reproducing realistic year-to-year variability. In a few specific years, the Arctic atmosphere is heavily constrained by the large-scale forcing at the outer model boundaries, for instance in summer 2007. This offers at least some confidence that Arctic climate models can be possibly successfully applied for seasonal to multi-year sea-ice predictions. We do not claim that we are able to make predictions; on the contrary, we just want to point to existing model limitations for such purposes. We have now completely rewritten this paragraph, having regard to the above-mentioned comments and the suggestions of the referee [page 15, lines 505–511]. Furthermore, the initial state issue has been reworded in the whole paper, since it has led to some confusion [page 4, lines 111–114; page 6, lines 191–192; page 6/7, lines 197–198; page 7, lines 200–201].

Response to the minor remarks

7.) Page 1271, 12–15: The sentence has been split into two sentences to avoid the grammatical incorrectness [page 2, lines 32–35].

8.) Page 1277, 15: This is an interesting note in view of higher potential predictability of low-ice periods. A more isolated Arctic would be associated with a more zonal large-scale flow and vice versa. Indeed, there is some indication – but no clear proof – that the predictability is higher when the flow is more meridional. It is ongoing research to identify large-scale atmospheric teleconnection patterns that are accompanied by higher predictability of the Arctic climate.

9.) Page 1278, 20–23: Observational data suggest that the Arctic sea ice in the 1960s and 1970s was thicker than in the 1990s. The high-ice path would therefore

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correspond better to the observations. It has nothing to do with the thinning itself. We have replaced the reference to “thinning” by the reference to thicker ice in the 1960s and 1970s [page 7, lines 223–225].

10.) Page 1283, 1–2: We have replaced the term “dominant role” with the somewhat weaker term “important role” as suggested by the referee [page 10, line 323].

References

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