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The Cryosphere Discussions

Interactive comment on "The Arctic Sea ice in the CMIP3 climate model ensemble – variability and anthropogenic change" *by* L. K. Behrens et al.

L. K. Behrens et al.

tmartin@geomar.de

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The authors comments on the two anonymous reviews:

Both referees agree that focusing on Central Arctic and Barents Sea is novel and important feature of our study. Barents Sea (BS) is a region of the strongest inter-annual to inter-decadal variability of the sea ice cover according to observations and in the majority of climate models. In many models, this is the region of the strongest warming in future climate projections under anthropogenic greenhouse forcing. At the same time this is the region where uncertainty of simulated climate variability and change is the highest among the IPCC model ensemble. This is why we focus on this region and believe the results can be interesting even in light of extensive previous analysis of the CMIP3 and recently released CMIP5 data. We do agree that in the abstract and con-

clusions more weight should be given to those results that are most novel and relate to the regional aspects. We did expect a question about using CMIP3 data when currently CMIP5 ensemble has become available. Our analysis has been preformed more than a year ago when not all CMIP5 data were at hand. More importantly, however, is that we have performed some additional analysis in contrast to what has been done before. Also, an appearance of new dataset does not exclude a usage of older data. The scenarios of anthropogenic impact are not comparable in these two model ensembles and CMIP5 does not a priori provide better data. One can consider CMIP3 and CMIP5 as two largely independent model datasets. The new data did become more extensive. See Knutti 2010 for related problems' discussion.

Answers to anonymous Referee #1

General comments: The Referee #1 agrees that regional focus of our study is novel but concludes that main funding do not add to what has been already known. We agree that we did not put enough emphasis on the novel findings and now we rewritten Abstract and Conclusions to include more specific new results.

The second comment on "to take into account sea ice cover biases across the models, and thus conclusions about regional changes" is repeated as one of the following major comments. See the answer below.

Major comments: We may not agree with the comment of referee #1. All models use the same external forcing, so the timing of the forcing is prescribed. Therefore we are not flexible for adjusting model results to some more realistic state or considering anomalies instead of absolute values. Sea ice is additive and limited (0-100% in terms of percentage coverage) variable. If some models show, e.g., too much regional sea ice, this is what we want to show to a reader. Large spread (of both mean state and variability) among the models particular on the regional scale is one of important results that one should be aware of.

The description of large variability or intermodal spread as well as magnitude of the

changes simply follows from the corresponding figures. E.g., Fig. 6c clearly shows increased variations and model spread to the end of the 21st century. The analysis is self-consistent. We always provide area mean values for the region of interest. Figs. 3, 6, 8 do show larger variability and intermodal spread for the BS region. We do not conclude that BS ice relation to the temperature changes is non-linear. We wrote “sea ice area does not show any linear dependence”. This is merely a description of what is seen in the Fig. 5c. No strict implication is made about “ocean dynamical feedback”. We formulate it rather suggestive: “may suggest that ocean dynamical feedbacks play an important role in this region”.

Minor comments: In March and September, we see the sea ice area maximum and the sea ice area minimum. In the first section we are interested in changes in sea ice area maximum and minimum. In the following section we want to discuss the sensitivity for the whole season. So we have to switch JAS and JFM, which also contains the maximum and the minimum of sea ice area.

We agree with the comment and change the scale of Figure 1 a) and b

We fully agree with that we can trust Arctic sea ice area observations from before the satellite era. In particular, the very recent study by Semenov and Latif (2012, The Cryosphere) strongly suggested that HadISST1 sea ice data prior to 1950s are unreliable in winter time consisting basically of climatological values without interannual variations. Model simulations and comparison to temperature observations suggest a strong multi-decadal variation of the Arctic sea ice cover in the Early Century Warming period comparable to the present day decrease. We added some more discussion to this issue in the text. It was not clearly indicated in the text but we used only the sea ice data since 1950s when regular instrumental observations have started.

We will include a sentence like: “The observational data prior to 1953 are very sparse and highly inhomogeneous. The recent study by Semenov and Latif (2012) suggested that there must have been a strong negative sea ice anomaly (comparable to the cur-

rent decrease) in winter time that is not present in HadISST1 dataset. Therefore we used HadISST1 data only starting from 1950 for the analysis.” Also add citation: Semenov, V. A., and Latif, M.: The early twentieth century warming and winter Arctic sea ice, The Cryosphere, 6, 1231–1237, 2012.

Concerning the comment - page 5325, Lines 2-8: This must have been a misunderstanding. We do not state otherwise. We discuss a difference between regressions for summer and winter seasons. For winter there is a stronger sensitivity for the whole Arctic, whereas for summer there is a stronger sensitivity in the Central Arctic. We modified the text to make it clearer.

In summer, the sea ice area in the Central Arctic is more sensitive to temperature than the entire Arctic.”

To comment page 5328, Lines 12-13: We agree. And this is what exactly what we mean in this sentence.

Answers to anonymous Referee #2

We agree with the general comment: The abstract is now modified to include more information about novel findings and less concerning already known results.

When we started with the analysis only few CMIP5 models data were available. Also, please see the first paragraph of our response. We aim to discuss the spread between the individual models and not the differences between the models due to different initial conditions. Another problem is that not all models provide ensemble simulations. A major part has just one run. This makes it difficult then to compare ensemble mean results with individual experiments. This is why we take one run from each model.

For the Barents, there are strong differences between the individual models. The models which fit in the best way for September and March are INM-CM3.0, NCAR-CCSM3.0, MPI_ECHAM5 and UKMO-HadGEM1. This is now also included in the text.

Interactive comment on The Cryosphere Discuss., 6, 5317, 2012.