The Cryosphere Discuss., 9, C289–C292, 2015
www.the-cryosphere-discuss.net/9/C289/2015/

The Cryosphere Discussions
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Interactive comment on "Arctic sea ice area in CMIP3 and CMIP5 climate model ensembles – variability and change" *by* V. A. Semenov et al.

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Received and published: 31 March 2015

The authors discuss the variability and change of Arctic sea ice area (SIA) in the 20th and 21st centuries as simulated by the CMIP3 and the CMIP5 ensembles. They discuss the relation between projected changes in SIA and changes in Northern Hemisphere surface air temperature (SAT) and Atlantic Meridional Overturnung Circulation at 30N (AMOC). Lastly, they discuss the relation between natural variability of the North Atlantic Oscillation index (NAO) and sea ice in the Barents Sea.

They find that some observed aspects of the SIA are better represented in CMIP5 than they were in CMIP3, while others are worse. As expected, there seems to be a robust correlation between an increase in SAT and a decrease in SIA across the CMIP ensembles. The links in the CMIP ensembles between changes and variability of SIA and changes and variability of AMOC and NAO remain unclear.

General comments:

I would like to make three distinct points, each relating to different parts of the manuscript.

1) Large parts of the manuscript are a purely descriptive discussion of SIA in the CMIP3 and CMIP5 ensembles (Sections 3.1, 3.2, 3.4, 3.5) and thus are hardly original. Nevertheless, this description goes into more detail than previous publications, and therefore these parts could be useful to the community provided that the methodological flaws described in the specific comments are addressed. 2) The relation between changes in SIA and SAT (Section 3.3) has been documented many times in the literature (e.g. Li et al. 2013 (their Fig. 5), West et al. 2013 (their Fig. 3b), Ridley et al. 2012 (their Fig. 2)). It is therefore to be expected and of little novelty. Still, as in the comment above, it could be worth to document this relationship specifically for the CMIP ensembles. 3) The most questionable part of the paper lies in Sections 3.6 and 3.7 corresponding to Figures 10-13. Here, both the statistical inference and the physical interpretations are unsound, and I would reject any conclusions drawn here.

In summary, I would only recommended publication of this manuscript if the authors completely re-invent or delete the fatally flawed Sections 3.6 and 3.7, and address substantial flaws in Sections 3.1 - 3.5, as detailed in the specific comments below.

Specific comments:

1) The observational uncertainty of SIA is not discussed at all, yet it is used as the truth to evaluate all model results. In the pre-satellite era until 1979, SIA observations are highly uncertain throughout. In the satellite era from 1979 on, there is still a sizable gap in the observations around the north pole, where different assumptions about filling this gap can lead to differences in SIA. Thirdly, as discussed by Notz et al. (2014), there are substantial differences in SIA between different retreival algorithms and satellites (their Figures 2 and 3). These observational uncertainties of SIA need to be integral part of an evaluation of modelling uncertainties of SIA.

2) The interpolation of model and observational data to a 2x2 degree grid is not appropriate for analysing SIA in the Barents Sea. The meridional extent of this region is then resolved by only 5 (five) grid cells. Large interpolation errors are to be expected that might dominate a regional comparison between different models. Given that most models in the CMIP5 ensembles (certainly in the ocean component) will have a higher resolution, I request re-doing all the analysis on a 1x1 degree grid or finer to lend credibility to regional assessments.

3) pp. 1097f. and Fig. 10: The physical mechanism that links the AMOC at 30N and SIA in the Arctic is not established. Furthermore, the correlations are weak at best. For this part to be acceptable, two things need to happen: (i) a clear physical pathway needs to be suggested how AMOC changes are related to SIA changes (aided by some appropriate analysis), and (ii) rigorous significance testing against the null hypothesis of zero correlation between AMOC and SIA changes needs to be performed, to demonstrate that there is actually a signal.

4) pp. 1098f. and Fig. 11: The same criticism as in my previous comment applies. It needs to be established which correlations in Fig. 11 are actually significant at all (NB reduction of degrees of freedom by 9 year running means), and which of the correlation changes are significant.

5) pp. 1099-1101 and Figs. 12-13: The same criticism as in my previous comment applies. Additionally, there is doubt about the reliability of the Barents Sea SIA values given the 2x2 degree interpolation. I agree with the authors (ll. 27f. on p. 1099) that there is a physical mechanism how the NAO has an impact on SIA in the Barents Sea. This is supported by a more consistent model ensemble than for the AMOC: simulated correlation coefficients are mostly between 0 and 0.6 in Fig. 12. However, it is quite a stretch to call this a "strong" impact, and some discussion is necessary as to why the models simulate quite different correlations. I appreciate the idea of the authors to look at the sea level pressure difference Scandinavia-Svalbard. If the hypothesis of wind-driven SIA changes in the Barents Sea was correct, Fig. 13 should depict negative

correlations that are strong and consistent between the models. However, there is a large model spread, which needs to be discussed. Diagnosing the ocean heat transport through the Barents Sea Opening might help to understand what is going on here.

References:

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Interactive comment on The Cryosphere Discuss., 9, 1077, 2015.