

Interactive
Comment

Interactive comment on “Arctic sea ice area in CMIP3 and CMIP5 climate model ensembles – variability and change” by V. A. Semenov et al.

Anonymous Referee #1

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Authors compare different characteristics of Arctic sea ice concentration and area in CMIP3 and CMIP5 simulations for the Entire Arctic Ocean, central Arctic and Barents Sea. Paper also explore connections between sea ice area variability in this simulations and different climate indexes, such as Northern Hemisphere average temperature, AMOC, NAO, SLPG.

##General comments

Sea ice results of CMIP3 and CMIP5 experiments were already investigated and compared in several studies. Authors try to distinguish from them by using mainly sea ice area instead of concentration, by looking at some regional aspects of sea ice variability in projections and by comparing observed and projected sea ice variability to different climate indexes. I think if done properly it could be an interesting exercise that enhance

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our understanding of sea ice variability mechanisms in CMIP projections. However I find the paper purely descriptive. Authors make some attempts to explain what they observe, but all of them are speculative, and not supported by any additional analysis. Moreover I find several serious flaws in the methodology.

I do not recommend to publish the paper in the present form in “The Cryosphere”.

##Specific comments

Authors were not able to properly explain advantages, or any additional benefits of using SIA instead of SIC for this kind of analysis. On contrary, I believe that for the Entire Arctic model/data intercomparison use of SIA is a disadvantage, since there are no proper observations of SIA around the North Pole, and we can't make assumptions, that are valid in case of SIE in this region.

Decisions about choice of the data sets or specific scenarios to include or exclude from the analysis are not justified and look almost as made randomly, or by just choosing ones that are most convenient for authors or show better results. Most importantly I question choice of HadISST sea ice data set as source of information about sea ice area in the Entire Arctic. I also believe, that errors introduced by data interpolation to the 2x2 degree grid have to be discussed.

Analysis of SIA relation to AMOC, NAO and other indices rely on correlations that do not have information about their statistical significance. Most of the correlations are small, and taking in to account shortness of the time series and applied smoothing (running mean) I suspect that only very few correlations are actually statistically significant. Moreover there are no clear conclusions made from described relations.

##Detailed comments

###Abstract

The abstract is very long and do not really serve the purpose of giving short condense information about the paper. I suggest you rework it considerably.

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P 1078

5-6: ... to anthropogenic forcing, the models ... You don't need "the models" here

7-12: this paragraph does not belong to the abstract

17: ... termed Entire Arctic ... Remove

18-23: You can fit it in to one sentence, without mentioning every index you have investigated, since you not talking about results here anyway.

P 1079

2: ... response to anthropogenic forcing is different ... I think "different" is too strong a word here, sounds like they are really very far apart, while in fact there are just some variations, but, say, the sign, or spatial pattern are not really changed.

5-7: Opposite between September and March or between CMIP3 and CMIP5?

###Introduction

Introduction is also very long. I suggest several cuts, but I think authors should put more effort to make it shorter and clearer.

16: Better cite the original paper where this is stated than IPCC report in general. Or at least give a chapter in the report.

24-27: I think this kind of statements belong rather to popular literature than to technical scientific paper. Remove.

P 1080

1-2: Antarctic is not considered in the paper. Remove.

18-21: You do not discuss thickness in the rest of the paper. Here mentioning the thickness decrease might be appropriate if connected to SIE retreat. But I would just remove the whole paragraph.

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23: You use the term “Entire Arctic” here, but define it only at the page 1082.

P 1081

7: replace sea ice extent with SIE.

8: replace sea ice extent with SIE.

18: may be? I guess it IS strongly influenced, as supported by the references you list afterwards.

P 1083

20-22: Any references to support this statement?

###Data and methods

This section completely lacks discussion on data uncertainties. The fact that satellite data were not used directly is very unfortunate.

P 1085

3-6: I would not call HadISST1 data “observations”. They are reconstructions based on observations, but as you mentioned further, very sparse observations. Quality of pan Arctic estimates of the sea ice concentration obtained before “satellite era” is very questionable, and they are certainly hardly qualifies to be “observations”

3-6: When using HadISST data for similar study Stroeve et al. 2012, make an adjustment to reduce overestimated sea ice concentration. Why you do not do this?

11: What is the end of the period?

17-18: Why this size for the target grid is chosen? What method of interpolation was used? What are the errors introduced by this interpolation (e.g. compare SIE and SIA calculated on original grid and on coarse resolution grid)?

21-23: You also should mention, that in your “observations” missing measurements

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from the satellites around the North Pole (due to satellite inclination) are just replaced by 100% concentrations. This is relatively reasonable approach for sea ice extent calculations, although in recent years there are more and more events with < 15% SIC around the North Pole. However for sea ice area calculations this is not acceptable, and this is why most of the papers dealing with pan Arctic sea ice only consider sea ice extent.

27-28: It would be actually very interesting to estimate how big the impact of this error can be for different resolutions.

###Results

Discussions on the figures are very long with a lot of unnecessary details. I would concentrate more on the interesting features, and their implications for the analysis rather than on lengthy descriptions of the things that the reader can see on the pictures by himself.

P 1086

15 Before showing variability, it would be nice to show at least the mean ice edge for models and HadISST if not the mean distribution of SIC. You can put the ice edge line over the panels of Fig. 1.

P 1088

1: show instead of simulate

11-13: That would be a very strange idea, indeed.

15: remove "in response to anthropogenic forcing" – change in temperature is a response to anthropogenic forcing, change in the sea ice is a consequence.

15: Are there really some SD differences in March around 60N in the Atlantic to the south of Island (Fig.2 b,d,f)? I can't see any values there on Fig 1 e, or Fig 2 a,c,e.

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26-27: I would remove (mask) places where SIC is zero in the projections, because otherwise this negative differences in SD are confusing, especially in September (Fig. 3). We would like to see changes in the sea ice variability, not changes between sea ice variability and open water. In other words if there is no ice – there is nothing to compare with (now it seems like there is ice with 0 variability).

P 1089

9-10: Why only this scenario? Please explain.

16: remove of

24: You can only talk about improvement for the period where you have “observations”, which is from 1950 to something like 2014 (you did not specify this exactly in your data description).

25: decreases compared to what?

P 1090

20: You forgot to mention that you also consider different RCP scenarios.

21-23: Your results are also depends on the grid cell area (especially considering that you interpolate to the very low resolution grid) and include many cells with low concentrations. I guess you have to mention this as well, if you start to compare your methodology to the one from Strouve et al., 2007, 2012. And I should mention once again – there is a reason why models more often compared to SIE rather than to SIA, it is lack of sea ice observations around the North Pole.

23-24: Why it is better to keep outliers? This requires an explanation.

28: Decrease compared to what?

P 1091

3-5: Or this indicates, that your “observations” overestimate sea ice in central

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Arctic. For example here is comparison of September SIC for HadISST , NASA team algorithm (<http://nsidc.org/data/nsidc-0051>) and OSI SAF reprocessed data set (<http://osisaf.met.no/p/ice/>) (Fig.1). One can see that NASA team algorithm show lower concentrations in the central Arctic. It might not be true, of course, in reality (and “more advanced” OSI SAF seems to confirm higher concentrations), but this possibility should be discussed and possible errors should be evaluated.

7-8: The 3 decades from present day is 2045, and your mean still show about 1×10^6 . Some models show sea ice removal, but it's not a majority.

P 1092

4: It is not really clear to me what sharp decrease you talking about. It is for March or September? In the model data or in the HadISST?

25: Why you all over sudden switch to Summer/Winter analysis, while before only use September and March? How we can relate results obtained in this section to your previous analysis?

28: Why you didn't include RCP 4.5 results on Fig. 6 and 7?

P 1093

24: Do you mean SIA instead of SIC?

26: Which century?

28: Do you mean 2070-2100? If not please elaborate.

29: Once again – why you did not plot RCP 4.5 data?

P 1095

23: “may be” instead of “may are”

P 1098

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10: With such a low number of points is it really meaningful to talk about correlation? Are your correlation values statistically significant?

20: Can you support this statement with a reference?

24: You have to provide information about statistical significance of your correlations. I guess with 9 year running mean and a lot of very small correlations that you show, only few (if any) models will pass.

26: Why 9 year running mean?

P 1100

14: Why in this case you use running mean with 5 year window? It is not consistent with your previous 9 year window for AMOC.

20: I don't really see the point of showing correlations that are not statistically significant.

21: Here is the only time you mention statistical significance of your correlations. However you have to do it for all your correlations. Below you discuss results for the data that were smoothed by 5 year running mean (Fig. 12 b,d), making the R that is needed to pass %5 about 0.54. For your Fig. 11 it is 0.71 (taking in to account 9 year window for running mean, so you reduce degrees of freedom considerably). Hardly any model on this figures pass this tests.

###Figures

Figure 4. It is better to have same 10^6 for y axis on every panel, otherwise it is hard to compare.

Figure 5. Same as for Fig. 4.

Figure 8. Please provide names of the scenarios.

Figure 9. Is in really a "change", or just SD values?

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

TCD

9, C176–C185, 2015

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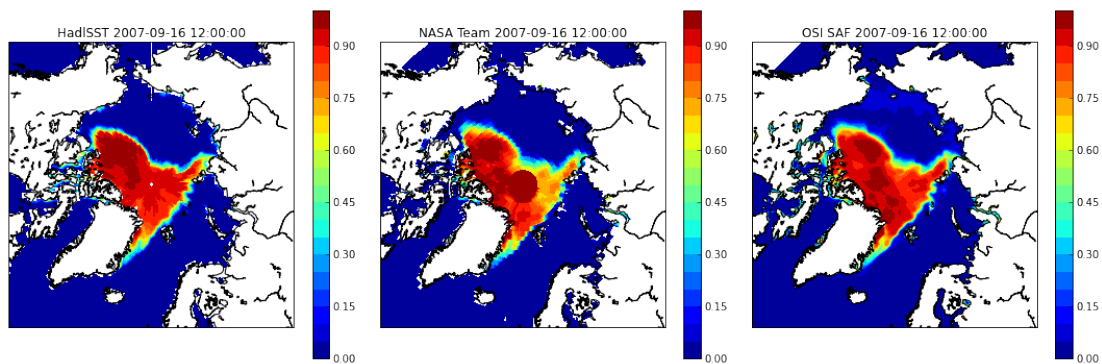


Fig. 1.

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