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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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General: The article "Arctic sea ice area in CMIP3 and CMIP5 climate model ensembles – variability and change" by Semenov, Martin, Behrens and Latif analyzes seasonal and interannual sea ice area variations in three different Arctic regions in the 20th and 21st century in CMIP3 and CMIP5 models. The study shows that CMIP5 models simulate in general a somewhat more realistic Arctic sea ice area and variation compared to the CMIP3 for September. However, particularly in winter, biases are larger in CMIP5 than CMIP3. The CMIP5 models sea ice area seems to be more sensitive to greenhouse gas forcing compared to the CMIP3 ensemble. Uncertainties and errors in CMIP5 and CMIP3 are large. Links of the sea ice area to NAO and AMOC have been investigated. Many processes stay similar in the 20th and 21st century until most sea ice has disappeared in the respective region. The article is interesting and well written and structured. To my knowledge, it is the first study comparing sea ice

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area reductions and variations in CMIP3 and CMIP5 in more detail; most exiting studies focus on total Arctic extent only. The article is therefore of interest for the scientific community and I suggest accepting the article after responding to the mainly minor points below.

Main points: 1. One general problem with comparing sea ice variation fields averaged over the variations of the individual models is the spread in the position of the ice edge in the individual models. The sea ice concentration variations are normally largest near the ice edge. Thus, if a few models have e.g. very little sea ice during summer, the ice edge is situated far to the north in the Central Arctic. These models will then show high ice variations in the Central Arctic. As a consequence, also the ensemble mean shows too high ice variations in the Central Arctic compared to observed values and too little ice variations in the area where observations show the largest variability. The overestimated ice variations in the Central Arctic and underestimations along the observed ice edges in CMIP models are thus probably at least partly due to the spread in the models. It is thus problematic to draw from the ensemble mean the general conclusion that CMIP models overestimate sea ice variations in the Central Arctic and underestimate sea ice variations along the observed ice edges as done by the authors (e.g. Page 1087, lines 7/8 and in the summary-section). To validate this conclusion it is necessary to compare the variations at the respective ice edges of the individual models with the variations at the ice edge in observations. This would generate insight if models generally underestimate variations along their ice edge. Since it is difficult to show all CMIP3 and CMIP5 models in the figures, I would suggest showing a few "representative single models", e.g. (one with very low ice extent, one with average and one with high ice extent) or extract the area with largest variations in each individual model and showing time periods. Similar, even the comparison between CMIP3 and CMIP5 might suffer from the fact that CMIP5 consists of about twice as many models than CMIP3, thus the likelihood for extreme positions of the ice area is larger and we could expect a more smoothed variation pattern in CMIP5.

TCD
9, C194–C201, 2015

2. AMOC and SST-gradient between Scandinavia and Svalbard are used in this article mainly as index for the ocean heat transport. I have problems with this for the following reasons: AMOC (section Pages 1097 and 1098): I agree, the AMOC is highly related to heat transport at 30N and also up to 50N (or maybe even 60N). But the northern tip of the AMOC normally ends in the area of the convection regions in the North Atlantic and ocean heat fluxes north of this into the Arctic Ocean is not necessarily very good correlated to the AMOC. Processes in the northern North Atlantic, e.g. in the sub-polar gyre and atmospheric circulation (e.g. Karcher et al. 2003; Sandö et al. 2010) in these area strongly affect the heat transport into the Barents Sea as well. Koenigk and Brodeau (2014) found e.g. no significant correlation between the AMOC and the ocean heat transport into the Barents Sea on decadal time scales in their model. This is obvious different in different models (e.g. in ECHAM5-MPIOM, Semenov 2008), however, indicates that the relation between AMOC and ocean heat transport into the Arctic is not as clear as formulated here. Furthermore, the statement "reduced AMOC implies reduced ocean heat transport into the Arctic" in the future is neither supported by models or recent observations. AMOC might already (there are no consistent observations) or is at least expected to weaken in future while ocean heat transport into the Barents Sea is according to observations (e.g. Skagseth et al. 2008) showing positive trends in the last decades and model simulations tend to project increase for the future, mainly due to increased ocean temperatures. This clearly indicates that the assumption made by many : "larger AMOC = more heat transport into the Arctic" is too easy. The AMOC is further very difficult to measure in real world; it is much easier to measure the ocean heat transport into the Barents Sea directly. This makes the AMOC in the real world to an index, which is very difficult to use for e.g. prediction of sea ice variations. SLP-gradient (Figure 13/ section 3.7.2): Although this SLP-gradient is important for the oceanic inflow into Barents Sea, it is likely also important for the atmospheric heat inflow. Further, a stronger gradient and thus stronger winds will transport the ice to the northeast, which would also reduce the SIA in Barents Sea. Thus, this gradient is not only reflecting the ocean heat inflow into

TCD
9, C194–C201, 2015

the Arctic but a combined effect of ocean and atmosphere. If the main goal with both AMOC and SLP-gradient is, as it appears to me from the manuscript, to represent the ocean heat flux into the Arctic, it would be much better to directly use the ocean heat flux as index. I am aware that this would mean handling of a lot of data and a lot work to calculate the ocean heat fluxes from all CMIP models. Therefore, if the authors decide to keep AMOC and SLP-gradient as index, they should discuss the points mentioned above and should avoid the impression that these indexes excellently represent the ocean heat transport into the Arctic. Also interpretation of the correlations found should be made with care.

3. In some parts, particularly in the introduction, the article would profit from some shortening.

Minor points: 1. Page 1079, line 5: what is meant with "worse results for winter SIA characteristics"?

2. Page 1080, line 10/11: what is meant here: that recent winter SIE decline is similar to ETCW or that winter SIE decline was smaller than summer decline in the ETCW as well?

3. Page 1081, lines 1-3 and following lines are a bit contradicting each other. First, it is stated that global models reproduce the decline, then it is argued that they are noticeably underestimating the decline. Maybe rephrase to make clear.

4. Page 1081, line 12. Please mention that this is only valid under the assumption of a high emission scenario. For A1B or B1/B2, the ensemble mean does not project a total September SIE loss until 2100.

5. Page 1082, line 3: although Wang and Overland (2009) is a very nice paper, it does not really fit here in my eyes since it does not compare CMIP5 and CMIP3. It could be cited in the section before where CMIP3 model results are discussed.

6. Page 1085, line 5/6: The use of sea ice concentration poleward of the marginal ice

TCD
9, C194–C201, 2015

area before the satellite-era is problematic since observations are extremely scarce. This is why many ice data sets (e.g. the Walsh data) use ice concentrations of 1 in these areas before 1978. Although HadISST made corrections to this (if I am right), the ice concentrations in the Central Arctic and especially the ice concentration variability is very uncertain before 1978 (even after 1950). I thus would suggest to use as comparison for SIC (figure 1) 1979-2005 (or 1979-2010 using e.g. RCP4.5 or A1B after 2005) and not 1950-2000. Of course the time period is shorter but ice concentrations away from the ice edges are not sufficient reliable to be used as reference "observations".

7. Page 1087, line 22: I am not entirely convinced we could conclude from larger variations in the Central Arctic that CMIP5 models are generally more sensitive to heat balance variations. I would think, the main reason is generally thinner ice in CMIP5 compared to CMIP3. And thinner ice is more sensitive to variations of heat fluxes than thicker ice (as correctly stated on page 1088, line 19/20). Thus the same ice thickness anomalies in CMIP5 lead to larger affect on the sea ice concentration than in CMIP3.

8. Page 1088, line 23-25: Again, I feel it is not straight away to draw this conclusion just from looking at the ensemble mean. Although it is not unlikely that CMIP5 models are more sensitive to heat anomalies due to the fact that more ice models in CMIP5 use e.g. multiple ice categories, melt ponds or improved rheologies compared to CMIP3, more detailed analyses of single models is needed. I agree that RCP4.5 and A1B look relatively similar despite the fact that A1B is a stronger emission scenario (best comparable to RCP6.0). But as discussed before, this could also be due to the fact that historical simulations in CMIP3 have thicker ice, which is less sensitive to heat anomalies.

9. Page 1090, lines 11/12: Given the two last summers with some recovery of the ice extent and almost return to the linear trend of the last 2-3 decades, an ice-free Arctic around 2020 seems to be relatively unlikely. I would also suggest replacing "very recent observed accelerated Arctic sea ice loss" by " the accelerated Arctic sea ice loss in the

TCD
9, C194–C201, 2015

last decade. “

10. Page 1093, lines 17/18: Is there any speculation why the spread in the Barents Sea is so much larger in CMIP5 compared to CMIP3. Is there a stronger mixing of sophisticated and more basic ice models in CMIP5 while in CMIP3 almost all models overestimated the ice in Barents Sea (because all CMIP3 ice models were still quite simple)?

11. Figure 8: I am surprised by the small seasonal cycle of the observed NH ice area. If I understood correctly this should be the ice area difference between March and September. Sea ice area variations from e.g. http://arctic.atmos.uiuc.edu/cryosphere/IMAGES/seaice.recent.arctic.png suggest for the entire Arctic something like 9x10âĄű km2 (and from Figures 4 and 5 I would extract about 8x10âĄű km2) but the observed values in Figure 8 seem to suggest only 4-5 mill km2. Please check the results in Figure 8 (and in case something went wrong in Fig. 8 also the conclusions in section 3.4) or specify more detailed how you defined seasonal cycle if it is not March – September ice area.

12. Page 1095, line 5: what is meant with “most probable trend”? Please define.

13. Page 1096/1097: “Holland and Stroeve . . ..because a shift in the surface pressure (SLP) anomalies.” This sentence does not make much sense as it is now. Please clarify. Compared to what is the SLP shifted? March? Future and PD?

14. Figures 10 d) shows a strongly positive correlation between AMOC and sea ice reduction: This is interpreted as: “ models with strong sea ice reduction also simulate strong AMOC reduction”. I am unsure if you would like to state that the processes are not related for summer and a third process affects both AMOC and summer ice or do you want to state that a positive AMOC leads to more sea ice during summer? In winter a much weaker negative correlation (how large is this correlation and is it significant?) is interpreted as: AMOC slowing and associated reduction in oceanic poleward heat transport plays a more important role for . . .” . I have problems finding an explanation: If

TCD
9, C194–C201, 2015

a strong AMOC reduce winter ice by melting of ice, it is very surprising that the summer sea ice area should be larger in the following summer. Is there any speculation about the physical process behind? Did you also calculated lag-correlations, AMOC leading the ice? I would expect that the ocean heat would need a few years from 30N to the Arctic (if it reaches at all the Arctic).

15. Figure 11: Please indicate if the correlations are significant. Using 9-year running means and probably quite a high auto-correlation do not leave many degrees of freedom for one single model for 70-year periods. I would assume that quite a number of the correlations are not significant.

16. Figure 12: From the figure, it looks like a number models do not show significant correlations (below r= -0.24); in the text it is stated that many exceed -0.24. Please mark in the figure the 95% significance level (e.g. by a line at -0.24). However, even if correlations in many models really is just above -0.24, it should be stated that correlations are generally small (particular for annual values in a) and b)), hardly any correlation exceeds -0.5, many are between 0 and -0.3/ -0.4, which means that NAO is not explaining more than 10-15% of sea ice variance in the Barents Sea in most models.

17. Page 1102, line 21/22: I do not understand the sentence: “Regional SEA changes are characterized by much stronger uncertainties that changes in the Entire Arctic”. Please rephrase to make clear what is meant.

18. Page 1103 line 13: It is not entirely clear what is the hen and the egg. Warmer SAT will lead to more SIA reduction but on the other hand SIA-reduction, which could e.g. be due to increased oceanic or atmospheric heat fluxes, will also strongly increase SAT in the Arctic , reflected in increased NH-SAT. Thus, I would suggest replacing “dependence “by “relationship” or similar.

Typings etc.

TCD
9, C194–C201, 2015

General: I would suggest to introduce the abbreviations only the first time the term is mentioned or to not use the abbreviation at all. E.g. SLP and SIA are introduced several times throughout the script.

Page 1078, line 17: delete: “termed Entire Arctic”, not necessary in the Abstract.

Page 1078Line 18-23: long and complicated sentence, I would suggest splitting into two sentences.

Page 1082, line 24: The author is called: “Massonnet “

Page 1085 line 20: it should probably be: “.. sea ice extent (SIE)” or “.. sea ice concentration (SIC)”?

Page 1087, line 10: in the Barents Sea.

Page 1089, line 16: “The CMIP5 ensemble mean SIA of is. . .” something is missing or “of” needs to be deleted.

Page 1095 line 23: “may be” instead “may are”

Page 1102. Line 14: delete one “the”

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