The Cryosphere Discuss., 6, C1819–C1836, 2012
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The Cryosphere Discussions

Interactive comment on "Constraining projections of summer Arctic sea ice" *by* F. Massonnet et al.

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Foreword to Editor

Dear Editor,

Please find below the answer to the two Reviewers comments, for the paper *Constraining projections of summer Arctic sea ice*, submitted to The Cryosphere. We warmfully thank both reviewers for their constructive comments, as well as your contribution as an Editor.

Both reviewers recommend publication pending some revisions, that are addressed in this document. Reviewer 1 (D. Notz) made a very pertinent remark about the model selection procedure, that we have now adapted. In addition, he suggested to re-structure the paper, what we did in the new version. Reviewer 2 (anonymous) proposed to

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discuss the hypothesis that current model performance is a guarantee for future performance. The first paragraph of the discussion in the revised version is now devoted to that issue in more detail. He/she also asked what would happen if a random selection of models was conducted. We now present such an analysis in a new Figure, Fig. 6 of the revised manuscript, that also summarizes the results of the paper.

Along with our comments to the reviews, we provide the new version of the manuscript with tracked changes. We have followed this color code for the text: RED=TEXT DELETED; GREEN = TEXT ADDED. BLUE=TEXT MOVED.

The present document contains a point-by-point response to the Reviewers' comments. In case any further question arises, please don't hesitate to contact me as the lead author of this paper.

On behalf of the co-authors,

François Massonnet

Comments on Referee 2's comments

(The Referee's comments are in italic font and the Author response in upright font.)

General Comments

The paper presents a recipe/set of criteria to constrain projections of September sea ice extent from models in the CMIP5 archive. The work builds on other studies that evaluate CMIP5, CMIP3 and CMIP2 archives. It contributes to the discussion of how to constrain these model projections. The paper should be published but requires some revisions and more detail.

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Specific Comments

1. *The authors gloss over the underlying assumptions of their paper. Whether or not to throw out models and reduce uncertainty is an ongoing debate (see Tebaldi and Knutti 2007, Knutti et al, 2010). I think there should be some discussion of this debate in the introduction. The problem with rejecting models is that one runs the risk of "throwing the baby out with the bath water". Furthermore, in the general context of GCMs, current model performance is not necessarily a guarantee of future model performance. One of the assumptions with any model rejection strategy, including the one presented here, is that current performance is a predictor of future performance. Another assumption is that the model climate for the evaluation period is representative of the actual climate. Some discussion of the aforementioned assumptions is necessary. The discussion of averaging period (page 2942, line 25) goes some way towards this but this discussion could be broadened to include the phase plane plots in figure 4.*

   Answer: The reviewer is right: two implicit hypotheses in the original manuscript is that (1) the 1979-2010 time period is suitable for model evaluation, and (2) that the GCMs performance is not changing over time. We completely agree that this should be clearly stated in the text. We also want to stress that our approach does not aim at discriminating between "good" and "bad" models, whatever this means. For example, one apparent outlier of the CMIP5 models for the Arctic, the CSIRO model, is actually one of the best performers for simulating Antarctic sea ice (Zunz et al., 2012, The Cryosphere Discussions). In the present paper we want to show that all 29 CMIP5 models contribute to constrain the sea ice projections either by defining the correlations in Table 2, or by providing information about the uncertainty related to internal variability over the evaluation period.

   Action: The first paragraph of the Discussion is now devoted to the model selection debate, with emphasis on the particular case of Arctic sea ice which provides

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a good framework to apply constraints (Collins et al., 2012; Sanderson and Knutti, 2012). Please see also our answer to your comment 5.

2. *The phase plane plots (figure 4) offer new insight to the evolution of sea ice extent but I feel that the authors could make something more of this approach. For example, can the trajectories in these plots be shifted in time to "calibrate" the projections? What do the trajectories reveal about the model state with respect to the observed state of the sea ice.*

   Answer: In these phase plane plots, observations are only visible with one snapshot (the black cross in Figure 4). Again, under the hypothesis that the real September sea ice extent will follow a trajectory towards ice-free conditions that resembles the GCMs trajectories, then a requirement for GCMs is that their current position in this phase plane is correct. In other words, the mean and the trend in September sea ice extent need to be "correct" in order to properly simulate September sea ice in the future. The phase planes offer another view as why the current mean and trend are important parameters for sea ice projections. You suggest to resort to a "time calibration" of these phase planes. This is a good idea and Reviewer 1 also mentioned a similar point (see his comment 10). However, such a time-recalibration is valid if we assume that the climate forcing is steadily increasing, which is not the case for RCP4.5. For RCP8.5, this would be feasible and we are thinking of analyzing these curves from the phase plane as to better understand the September sea ice extent evolution under such a constant forcing. This is not the goal of the paper, which treats of selection rather than re-calibration, but we may take your suggestion for later work!

3. *The authors need to state explicitly why the criteria were applied in the order given in the paper. Was it the strength of the correlations in table 2. If so should different criteria be used for rejection RCP4.5 and RCP8.5? Sea ice extent, volume and extent of thin ice are all likely to be related. What are the covariances between these metrics and how might these covariances influence the rejection of models*

and the order in which rejection criteria are applied. What is the result if just volume or trend in the extent is used as criteria?

Answer: We provide attached to this document a detailed table (Fig. 1) with the outcome of the four selection criteria, following the new procedure (see Answer to Reviewer 1 comment 9), with a tolerance of 20%. Green grid cells (marked with a 1) mean that the model was successful in simulating the corresponding metric. As you point out, these metrics are not fully independent and we made this remark in the original manuscript (p. 2942, l. 12) and give an indicative correlation. Nonetheless, all criteria are useful and complementary: a model that is successful in simulating the average 1979-2010 September sea ice extent is not necessary selected for sea ice volume (e.g., EC-Earth, CCSM4, FGOALS-g2, GFDL-ESM2M and many others). Conversely, there are models with a reasonable sea ice volume in the annual mean but with too low or high a sea ice extent in September (GISS-E2-R, HadGEM2-AO). The table also shows that the rejection of models may be due to the incorrect representation of only one parameter (e.g., CNRM-CM5, CCSM4), or two (BCC-CSM1.1, CanESM2), or three (FGOALS-g2, NorESM1-M), or four (CSIRO-Mk3.6.0, NorESM1-ME). In this sense the selection procedure is not too crude. As presented here, the order of the criteria does not impact the final decision since we look at models that simulate properly all four parameters.

Action: We agree that the way selection is presented is somewhat confusing. Following Reviewer 1's remarks and your comment, we present now the result of the model selection in a new figure, Fig. 6. of the new manuscript, for all criteria (the last column of the table shown in Fig. 1 attached to this document). In the text we keep mentioning why a selection based on sea ice volume is important: with only sea ice extent-related metrics, the year of disappearance would range from 2029 to 2076 for RCP8.5, whereas it ranges from 2041 to 2068 with sea ice volume included as a metric.

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4. *Their conclusion is that the criteria they have selected reduces uncertainty by excluding 23 of the 29 models. Would another set of criteria or even a random selection of 6 models give a similar result?*

   Answer: This is a very good point. To test the validity of the selection, we have

   (a) Generated 10,000 **random** selections of k CMIP5 models among the 27 CMIP5 models that are ice-free before 2100 under RCP8.5. Here, k is equal to the number of models actually selected in the paper (for a 20% tolerance around observations, $k = 6$),

   (b) Averaged the earliest and latest years of disappearance from these random selections.

   The outcome of this random selection is now presented with blue lines in the Fig. 6 of the new manuscript, also reproduced in attachement to this document (Fig. 2). Note that in this figure, the models going to ice-free conditions after 2100 are not considered, although they also contribute to the initial uncertainty.

   Action: A new figure (Fig. 6 in the revised manuscript) summarizing these results is now shown. It is also given in attachement to this document (Fig. 2).

5. *Is there any way to test their approach using other time periods. E.g. can 1900 to 1930 sea ice extent be used to predict the timing of the 4x10*6 September ice extent? Again this seems to be related to the location in the trajectory.*

   Answer: Using the 1900-1930 time period to validate the method is difficult given that (1) we don't have sea ice volume estimates from the PIOMAS reanalysis for that time period, and (2) no reliable observations of sea ice extent are available for this time period. But your suggestion is insightful, and we have performed the following test. We hope this will partly answer your question. First, we apply the same selection procedure as the one in the paper, with a tolerance θ of 20%, over the 1979-1995 period. Then, we address the skill of models by

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comparing the simulated and observed mean 1996-2011 SSIE. This approach has limitations: we are not looking at long-term projections as is the case in the paper. The ENSO and NAO should for example be taken into account on such "short" time scales compared to the "long" time scales that we deal with in the paper, for which internal variability has a much weaker influence.

The Fig. 3 in attachement to this document shows the absolute difference between the observed and CMIP5 simulated mean 1996-2011 SSIE, ranked by descending order. CMIP5 models that have been selected over the 1979-1995 period are preceded by the string *******. We note:

- Models that are less skillful over 1996-2011 are never selected over 1979-1995;
- All selected models have a bias less than the mean bias (1.3 million km^2) and less than 1 million km^2 (17% of the observations). Thus, the models lying in the $\pm$20% interval around observations/reanalysis over 1979-1995 for the criteria defined in the paper, remain within a 20% interval for SSIE over 1996-2011;
- Still, there are models that show little bias compared to the 1996-2011 mean SSIE, but that were not selected. This is coherent with the last sentence of the original manuscript: "Our study [...] indicates that simulating a correct sea ice state [...] is a necessary condition to reasonably anticipate future sea ice evolution, as it has a clear influence on the variability and response of the sea ice cover".

Action: We include a brief summary of these results in the first paragraph of the discussion.

6. *The authors rely heavily on statements in parentheses. These break up the flow of the text making it difficult to follow. I would suggest that the authors rewrite these portions of the text, removing the parentheses.*

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Answer: This is right. Reviewer 1 actually made a similar remark!

Action: We have browsed the text, and improved the readability.

Technical Comments

7. *Abstract; line 15. "For these reasons...". This sentence doesn't make sense to me.*

   Action: The sentence now reads: "We also find that the year at which SSIE drops below a certain threshold is likely to be constrained [...]"

8. *Abstract; line 10. Change "compared to the 1979-2010 model SSIE" to "with respect to the 1979-2010 mean SSIE"*

   Action: Done

9. *Abstract; line 13. Would "Phase plane diagram/plots" be a better description of these "new" diagrams. I realize that these are not strictly phase plane plots but the phrase conveys the idea of visualizing the dynamics of the system.*

   Answer: Good idea. "New" refers to the way SSIE evolution is presented, but of course the use of phase plane is not new at all.

   Action: We changed "new diagram" by "phase plane plots".

10. *Page 2933, line 10. "magnitude and timing" seems vague. Would rate of decline and timing of ice free Arctic be a better description?*

    Answer: This is indeed vague. We take your suggestion.

    Action: "magnitude and timing" changed in "rate of decline of sea ice extent and timing of ice-free Arctic"

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11. *Page 2934, line 9. What do they mean by robust? I'm not sure how one can test the robustness of the method.*

    Answer: By "robust", we mean that there is a strong relationship between present-day sea ice parameters (Table 2 of the original manuscript) and sea ice evolution in the coming century, either nonlinear (changes in SSIE) or linear (year of disappearance).

    Action: Perhaps "robust" is not an appropriate word here. We have changed the sentence as: "In this work, we [...] show that several variables related to the current 1979-2010 sea ice state are influencing the most recent generation of summer Arctic sea ice projections.".

12. *Page 2934, line 11. This is an example of where parentheses could be removed. I would suggest: "Long-term means of September sea ice extent and annual mean sea ice volume, and trend in September sea ice extent for the 1979-2010 period are considered as metrics to constrain sea ice projections."*

    Action: Your suggestion is taken, and we have added the amplitude of the seasonal cycle of sea ice extent in the list of metrics that you enumerate.

13. *Page 2935, line 9. Some models have very thin ice across the Arctic. What is the mean/median and range of model ice thickness. For example, did any models only have mean Arctic thickness below 0.5m.*

    Answer: Figure 4 attached to this document shows the distribution of the annual mean 1979-2010 Arctic sea ice thickness of the CMIP5 models. The mean thickness is 1.62 m, the median is 1.35 m, and the standard deviation is 0.65 m. No model has less than 0.5 m in the annual mean over 1979-2010.

14. *Page 2935, line 13. I would suggest (defined here as north of 65N).*

    Action: Changed.

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15. *Section 2 would be a good place to introduce PIOMAS.*

    Action: Done

16. *Page 2936, line 14. "The CMIP5 multi-model mean trend underestimates..."*

    Action: Done.

17. *Page 2937, line 8. "... under RCP8.5, the question is to determine when exactly the Arctic is first ice-free." Isn't this the objective for all scenarios? The statement in this sentence needs to be better explained. I would also avoid using "exactly". There is always going to be uncertainty.*

    Answer: You are right, the question is relevant for all scenarios. We wanted to distinguish between two levels of uncertainty: first, is the Arctic going to be ice-free before 2100? Second: if so, when may this occur? For RCP4.5, the second question does not even make sense because 50% of the models are not ice-free in 2100. For RCP8.5, the second question deserves attention because about 90% of them are ice-free at this time. But as you say, the problem is essentially the same.

    Action: The sentence has been changed to: "Still, the spread in the projections remains large. For both scenarios, the September sea ice extent during a particular decade of the coming century and the decade at which an ice-free Arctic could be realized, are highly uncertain quantities if all models are considered"

18. *Page 2937, line 10 onwards. The authors make a good point here but I think they could make their message more clear. I think what they are trying to say is that change in sea ice extent expressed as a fraction of the initial state is a function of the initial state, so correlating sea ice extent with percentage change effectively correlates one variable with a function of itself. I would hope that the example in the footnote is unnecessary but have no problem with them including it. However, some more detail here would make a stronger case. In the example*

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they give, the variances are the same for X and delta X. However, the strength of the correlation is dependent on the ratio of these variances. The constant delta X is one extreme (zero variance) but a weaker correlation would occur if the variance of delta X was larger than the variance of X. It appears that they have used mean extent and mean change in extent. Are the variances of these variables in the example (1 million km2) similar to the variances of model extents and change in extents? The example would be more relevant if they used actual variances.

Answer: Reviewer 1 made a very similar remark (comment 4). Indeed, we think that this footnote is probably not necessary in such a level of detail, and most readers will understand the problem of getting spurious correlations if a variable is correlated with a function of itself.

Action: We dropped the note.

19. *Table 2. The authors need to explain in more detail how they dealt with extents used to calculate the anomalies. Did they use the first year that sea ice dropped below the threshold or did they smooth the time series to get this value? Did they assign 2100 to all RCP4.5 model runs that did not reach the threshold. How much can the poor/non significant correlations be attributed to the way the data was treated?*

Answer: In the left part of Table 2, the anomalies are calculated by substracting the 2030-2061 and 2069-2100 mean SSIEs from the 1979-2010 mean SSIE, respectively. Perhaps the use of "anomalies" is confusing here, we should use "changes". The year when sea ice drops below a threshold, is defined in the text (p. 2938 l.29-p.2939 l.1) as the first year when such an event occurs, without smoothing the time series. This can happen sooner or later depending on the models' own internal variability. Models that do not reach a given threshold before 2100 are not considered in the scatter plots used to make correlations of Table 2; assigning a 2100 would bias the relationships since most of them probably cross

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the threshold later than 2100. In Table 2 caption of the original manuscript, we stress: "Note that the number of models used for the calculation of correlations in the right part of the table can vary depending on the scenario and threshold (e.g., a limited number of models reach 1 million km^2 under RCP4.5 before 2100)".

Action: In Table 2 caption, we change "anomalies" by "changes" as to not confuse the readers. We also add "first" before "year at which SSIE drops [...]", and now write that "That is, only the models that cross the threshold before 2100 are considered in the correlations".

20. *Page 2938, line 20. It might be an idea to explain the basis for good selection criteria.*

    Answer: Sorry, but we don't understand your point here.

21. *Page 2939, line 11. Given that volume is used as a criteria, doesn't it also deserve attention?*

    Answer: We wanted to highlight in this paragraph the rather similar long-term slope of SSIE from CMIP5 models compared to their scattered trends on a shorter time period.

    Action: We decided to drop this paragraph anyway as it did not serve the flow of the text.

22. *Page 2940, line 13. Shouldn't this be figure 4?*

    Answer: Exactly! Thanks.

    Action: Changed.

23. *Page 2940, line 26. Of not if.*

    Answer: You suggest to change "if" by "of", but then the sentence reads bizarre.

    Action: We write "as" instead of if.

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24. *Page 2941, line 12. A zero trend does not imply no inter-annual variability. You could have a stationary climate (no trend) but still have inter-annual variability (e.g. ENSO, NAO).*

    Answer: We agree that a zero trend does not imply no interannual variability. Here we turn the statement the other way around: when the average SSIE is zero, then the time series of SSIE is by construction made up of zeroes only. Consequently, the trend is zero.

25. *Page 2941, line 21. What is "medium 1979-2010 SSIE"?*

    Answer: We meant "medium" for "not low, not high compared to observations".

    Action: We changed "medium" by "around the observed value"

26. *Page 2942, line 1. The predictand is not really modified. The timing of ice free conditions is a different predictand.*

    Action: The sentence has been changed to: "Nonetheless, a clear relationship exists if we choose another predictand such that the SSIE is now the independent coordinate."

27. *Page 2942, line 3. Why not use the enumeration in table 2. Moreover, I don't see ice thickness in table 2 – shouldn't this be volume? (also see page 2944, line 12).*

    Action: Good idea, we now use the same enumeration as in the table. And you are right, thickness is not supposed to be in the table. "Thinner ice" has been changed by "smaller ice volume".

28. *Page 2945, line 1. What is a reasonable trend? What are the criteria here.*

    Answer: We referred to the selection procedure explained in p. 2943 l. 14 of the original manuscript.

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Action: This paragraph has been rewritten anyway, with more emphasis on the selection threshold. We now treat all metrics for our selection on the same basis, and at the same time (see also our answer to your comment 3).

29. *Page 2945, line 8. An alternative explanation is that there is not much difference between mean SSIE, thickness and trend for the two periods. I do not think that this indicates that the approach is robust.*

    Answer: OK. Following one of your previous remarks, and Reviewer 1's comment 11, we tested the validity of the method otherwise (see answers to your comments 4 and 5).

    Action:

30. *Figure 1. Make the lines for observed extent more visible. They get lost amongst the model trajectories*

    Action: Done

31. *Figure 2. plot (b) needs the y-axis range reduced to show detail in the scatter in means. The legend can be placed outside the plots.*

    Answer: We think you refer to Fig. 5 instead of Fig. 2. We wanted to show the same y-scale so that the relative scatter in means and trend can be compared easily. As it is currently, the graph also shows clearly that the relative spread in the mean is about 1 order of magnitude lower than the spread in trends when 30 yr-periods are used, which is precisely the message we would like to convey.

Please also note the supplement to this comment:
http://www.the-cryosphere-discuss.net/6/C1819/2012/tcd-6-C1819-2012-supplement.pdf

Interactive comment on The Cryosphere Discuss., 6, 2931, 2012.

	1979-2010 avg. SSIE	1979-2010 Amplitude seas. Cycle extent	1979-2010 trend SSIE	1979-2010 avg. annual sea ice volume	Model selected?
ACCESS1.0	1	1	1	1	1
ACCESS1.3	1	1	1	1	1
BCC-CSM1.1	1	0	0	1	0
CanESM2	0	1	1	0	0
CCSM4	1	1	1	0	0
CNRM-CM5	1	0	1	1	0
CSIRO-Mk3.6.0	0	0	0	0	0
EC-EARTH	1	1	1	0	0
FGOALS-g2	1	0	0	0	0
FGOALS-s2	1	0	1	1	0
GFDL-CM3	1	1	1	1	1
GFDL-ESM2G	1	0	1	1	0
GFDL-ESM2M	1	0	1	0	0
GISS-E2-R	0	0	1	1	0
HadGEM2-AO	0	1	1	1	0
HadGEM2-CC	1	1	1	0	0
HadGEM2-ES	1	0	1	1	0
INM-CM4	1	1	0	1	0
IPSL-CM5A-LR	1	1	1	1	1
IPSL-CM5A-MR	1	1	1	1	1
IPSL-CM5B-LR	0	1	1	0	0
MIROC5	1	0	1	0	0
MIROC-ESM	1	1	1	0	0
MIROC-ESM-CHEM	1	1	1	0	0
MPI-ESM-LR	1	1	0	1	0
MPI-ESM-MR	1	1	1	1	1
MRI-CGCM3	1	0	1	1	0
NorESM1-M	0	0	1	0	0
NorESM1-ME	0	0	0	0	0

Fig. 1. Detail of the selection of CMIP5 models with a tolerance of 20% around observations or reanalysis. Green cells (1) indicate that the model was successful in simulating the corresponding metric. The last column is the minimum of all columns; that is, a model is selected only if it simulates properly each individual metric.

Fig. 1.

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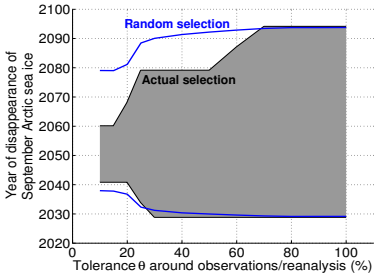

Fig. 2 (new Fig. 6 in the manuscript). Range of simulated years of disappearance of September Arctic sea ice, for RCP8.5. We define the year of disappearance of September Arctic sea ice as the first year during which the 5-yr smoothed September sea ice extent drops below 1 million km^2 for more than 5 years. A selection of models is applied following the methods defined in the paper for each tolerance θ around observations/reanalysis. The black lines show the earliest and latest years of disappearance for the selected models as a function of θ. The blue lines show the corresponding range that is obtained on average by selecting the same number of models randomly (10,000 draws) and ignoring the two models that are not ice-free by 2100 for which we do not have the year of summer Arctic sea ice disappearance.

Fig. 2.

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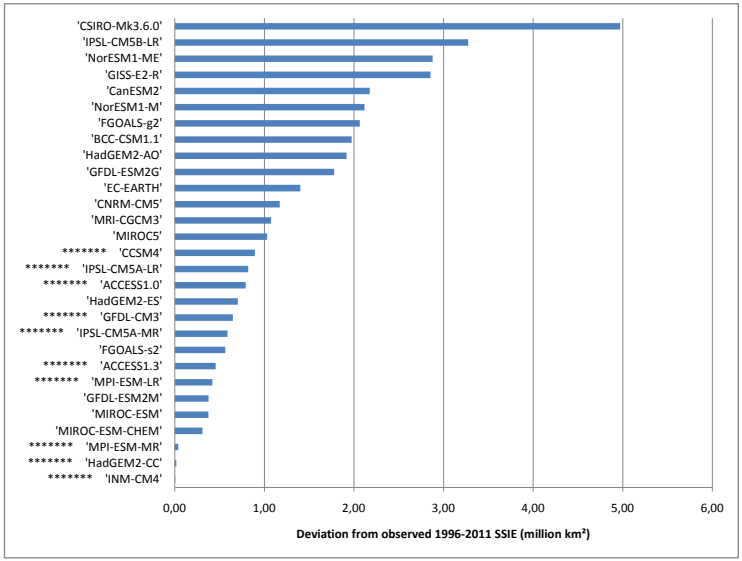

*Fig. 3. Absolute difference between simulated and observed (NSIDC) 1996-2011 mean September sea ice extent, ranked by descending order. Models preceded by ********** are models that were selected following the procedure explained in the paper, over the time period 1979-1995*

Fig. 3.

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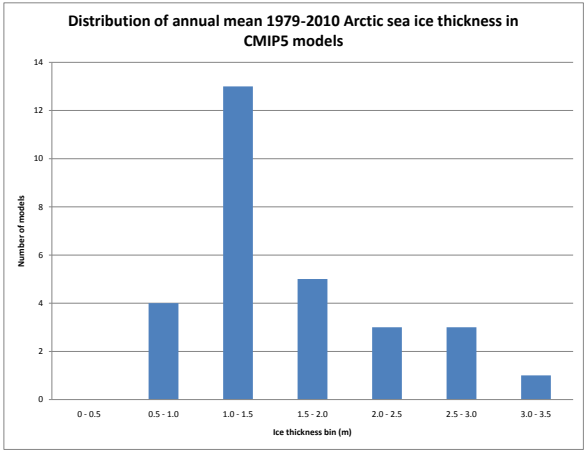

Fig. 4.

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