

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 warmly 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 D. Notz (Referee 1)'s comments

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

In this manuscript, F. Massonnet and co-authors aim at constraining the time period during which the Arctic will become almost ice free in summer time for a given emission scenario in CMIP5 model simulations. They do so by selecting a subset of six models from the 29 models they analyzed. The selection is based on three observational parameters for the period 1979-2010 which the authors show to be of relevance for the future evolution of the ice pack.

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General comments

I very much like the analysis of why the parameters describing sea-ice coverage during the period 1979-2010 do not directly correlate with the future evolution of sea ice in the time domain. This, in my opinion, is currently the scientifically strongest part of this paper. Regarding the resulting selection of a subset of model simulations, some additional work could possibly improve the impact of this publication, as outlined below. I also recommend to put some more work into better structuring the paper. Once the list of rather minor items outlined below have been addressed, this paper will be suitable for publication.

Specific comments

*(Comments marked with a * are suggestions that could be addressed, but the paper would be publishable even if the authors choose not to address those in a revised version)*

1. *p.2932, l.25ff.: Please give references for the differences between first-year and multi year ice. At the moment, the implications of more first-year ice are summarized somewhat too vaguely for my taste*

Answer: We understand that the current writing is vague.

Action: We have now re-directed the reader to the works of Armour et al. (2011) and Maslanik et al. (2007) as well as the Arctic Monitoring and Assessment Programme report that provides a large and thorough view on the consequences of the transition of Arctic sea ice to a first-year regime.

2. *p.2935, l.14: It is mentioned that care must be taken when analyzing the output. How was such care taken in the present study? Did you normalize the sea-ice extent by the ocean area, for example?*

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Answer: We mean here that inter-model differences in sea ice extent or volume are not entirely due to differences in the models physics, but also to the differences in the land-sea mask definitions. If a model is not selected following our criteria, this may be the result of an inaccurate representation of coastlines, for example. We therefore consider that the land-sea mask is an important property of the CMIP5 simulations. We did not normalize sea ice extent by ocean area : in this case, information about the model land-sea mask would be lost.

Action: We now have continued the sentence: "[...] care must be taken when the output is analyzed: **for example, a model may misrepresent the observed sea ice extent due to too coarse a grid resolution, or to an inaccurate representation of coastlines and land distribution. We therefore consider the land-sea mask as an important property of the CMIP5 simulations.**"

3. *p.2935, l.23: When you average across all models, do you consider the mean of all ensemble members of a single model as "one model"? If yes, which implications does this have given your argument on p.2945 l. 14ff that a multi-simulation mean has different properties than individual simulations?*

Answer: Yes. For each CMIP5 model, we first consider the mean of all ensemble members. Then, we average with equal weight these means to get the multi-model average. In this sense, there is no biased weighting towards models with a large number of members.

On p. 2945 l. 14ff of the original manuscript, we show that the multi-model mean does not reproduce the marked minimum in September sea ice extent trend that is present in individual model simulations, just because these trends occur at different times in the 21st century in all CMIP5 models. The multi-model mean has different properties than individual simulations because the individual CMIP5 models are so scattered that they do not reach the most negative trend at the same time in the 21st century. By contrast, the inter-member scatter among a single CMIP5 model is much lower, so that the mean of the members from one

CMIP5 model has the same trend evolution as the individual members (see for example Fig. 3 of the original manuscript).

Action: As we understand, the current writing of p.2935 l.23. is not clear. We changed the sentence by: "The multi-model mean is obtained in two steps. First, the members are averaged for each CMIP5 model. If a model only comprises one member, then this single member is considered. Then the average is taken with equal weight over all the models. In this sense, the multi-model mean is not biased towards models with more members".

4. * p.2937, footnote: *It might suffice to say in l.15 "However, this might be a misleading result since for a mean state X and any variable Y, correlation can exist between X and Y/X even if no such correlation exists between X and Y." and to then drop the footnote*

Answer: The footnote is indeed too long, and the readers don't probably need it to understand that such spurious correlations can occur in this case.

Action: We have deleted the footnote.

5. p.2939, l.21: *Why does "the rapid loss at some point in time" cause more uniform trends over the longer period? Even if the loss wouldn't be rapid, the long-term trends for the transition from a large ice cover to almost ice free would be rather uniform, I believe.*

Answer: One could imagine the CMIP5 models reaching sea ice free conditions at different paces, i.e. diverging from their baseline 1979-2010 state. Here, because the CMIP5 models achieve very large negative trends when approaching ice-free conditions, the long-term average from now to ice-free conditions is more uniform than today's sea ice extent trends.

Action: We have decided anyway to delete this paragraph as it does not serve the flow of ideas.

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6. * p.2940, your discussion of the U shape: Such U shape is a very common 1st derivative for the smooth transition between two stable states, since both end points of the shape are constrained at 0 on the y axis. The most striking feature of Fig. 4 is hence possibly the fact that the rate of maximum sea-ice loss occurs at a similar sea-ice extent. While you mention this later in the paper, you might want to focus on this already here.

Answer: Since the main focus of the paper is not on this U-shape, we have decided not to emphasize too much on it and deleted parts of the paper devoted to it. We briefly mention the existence of this U-shape in the Results section, but nothing more.

7. * p.2941, 1.4-6: Is the increased variability at a sea-ice extent 2-4 million km² possibly related to the maximum decline at a similar sea-ice extent?

Answer: Goosse et al. (2009) worked with detrended time series to show that the September sea ice extent variance peaks at about 2-4 million km² of average sea ice extent. This increased variability is therefore not a consequence of the large trends that we detect. Yet, both the large trends and variance can be caused by a common process. As stated in the text, open water forms at higher rates when sea ice is thin. On year-to-year time scales, the sea ice extent variations are therefore likely to be larger than with thick ice. On decadal time scales, the response to forcing (i.e., the trend in September sea ice extent) is also likely to be higher in magnitude, for the same reasons.

8. * p.2943, 1.2: A comment on the reasons for the decrease in variability for longer time periods would be nice. Is this simply an artifact of the fact that the models become almost ice free and the variability for a trend extending from today's condition to almost ice free is very small?

Answer: You refer to Fig. 5 of the original manuscript. The time periods used for calculating the trends on this figure extend to up to 2042 (=1982+60). At this

time, only three models are at ice-free conditions in September. The decrease in variability in trends is therefore not an artifact. It is instead the indication that a trend calculated on 30 yr of data can be very misleading if no precaution is taken as to account for the large internal variability. Our goal with the figure is to show that trends computed on 30 yr time slices are very variable from one member to another, and very sensitive to the end points used for calculation. The actual reason for the decrease in variability with increased length of time periods is probably related to the increased role of external forcing (and decreased role of internal variability) as time periods become longer.

Action: However, we think this is not the scope of the paper to discuss the reasons for this decrease, so we propose to keep the text as is.

9. *p.2943: Selection of models I: I was wondering how fairly you treat models with multiple simulations as opposed to those with just one simulation. It seems easier for a model to survive your selection criteria if it either has very many simulations or just one. Those with three simulations might have a smaller chance getting through your procedure, since they are evaluated on a par with the model which provides ten simulations. It seems to me that a fairer selection procedure would be to first derive a plausible range of simulations as described for the multi-model ensemble and to then select a model if any of its simulations falls within this "plausible" range – independent of the number of simulations a certain model provides. The resulting "plausible" range is likely larger than the $\pm 20\%$ criterion that is currently applied to all simulations of models with multiple ensemble members. It would also be helpful if you gave actual numbers for the range that a model must meet in order to be selected.*

Answer: This is an excellent point. You are right, in the original manuscript, the selection is biased towards (1) models with a large number of members that sample a reasonable part of the actual model uncertainty space, and (2) models with one member, which are given the benefit of the doubt. For other models, two

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or three members are certainly not representative of the actual distribution and there is a chance that we reject such models for wrong reasons. We have also decided to enlarge the analysis to other ranges than $\pm 20\%$ used in the original manuscript, as to show how this tolerance impacts the uncertainty about year of disappearance of summer Arctic sea ice.

Action:

- The selection procedure has been changed. Suppose we are interested in a certain metric C , for example the trend in September sea ice extent over 1979-2010.
 - (a) Let C^{REF} be the reference value for that metric, obtained from observations or reanalysis.
 - (b) Let K be the interval $[(1 - \theta) \cdot C^{REF}, (1 + \theta) \cdot C^{REF}]$, where θ represents a prescribed tolerance.
 - (c) Let C^{ij} be the simulated value of C by the j^{th} member of model i . Let C^i and s^i denote the mean and standard deviation of C taken over all members of model i , respectively. Finally, let s be the average of all the s^i taken over models with more than one member.
 - (d) The model i is successful in simulating C if
 - There is at least one member (i.e., one j) with C^{ij} comprised in the interval K , OR
 - The intervals $[C^i - 2s, C^i + 2s]$ and K have a non-empty intersection.
- We do not focus on a particular value for the tolerance θ anymore. We show instead that decreasing θ has the impact to effectively reduce the length of time interval for summer Arctic sea ice disappearance (Fig. 6 in the new manuscript, also shown after Reviewer 2's comment 4). For the particular 20% threshold, we have indicated the actual range around observations or reanalysis now in the text.

10. *p.2943: Selection of models II: More fundamentally, I was actually surprised to see that you went through the procedure of showing in a very nice way how removing time as an independent allows you to see similarities between almost all simulations – only in order to then follow earlier studies in narrowing down the uncertainty by picking a small number of models, which unsurprisingly narrows down the range of uncertainty. The beauty of your approach seems to me that you can actually retain much more models for your analysis by considering the fact that their specific time variable might not match observations, but that their overall sea-ice evolution might well do so. As such, it seems to me that you could build on the findings in Fig. 4 to nicely combine the strategies of earlier papers to re-calibrate models and to select certain models in an informed way to narrow down uncertainties of future sea-ice evolution. What you do is certainly not wrong, but it seems that it doesn't fully harvest the insight you gained from your analysis shown in table 2 and Fig. 4. Hence, you could certainly ignore this suggestions and take it up for later work, or see if it makes sense to already go down this route for this publication.*

Answer: This is a very good point, too. However, such a recalibration does not make sense as soon as the climate forcing is not steadily varying, e.g. with RCP4.5. In this case, a time-recalibration would not make sense since the sea ice response under stabilized emissions (after 2060 for RCP4.5) is not comparable to the sea ice response under increasing emissions (up to 2060). For RCP8.5, your suggestion is meaningful and we are considering looking at how we can extract information from the phase space (Fig. 3) as to interpret the CMIP5 models simulated September sea ice extent. While this is clearly not the purpose of the present paper, we may take it up for later work.

11. *p.2945, l.7: I'm not sure about robustness, in particular because of the high variability of trends. For example, for MPI-ESM-LR we find that just shifting the period of interest from 1979-2010 to, say, 1984-2014 we get very different trends for the*

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same simulation. Robustness of your method would, in my opinion, be better shown by being able to construct a similar range by inclusion of more models as just outlined.

Answer: Following Reviewer 2's comments 4 and 5, and your comment 9. and this one, we present "robustness" of the method somehow differently. First, we show that a random choice of models always provides larger ranges for year of disappearance than the actual selection of the paper (see the figure referred to as in Reviewer 2 comment 4; this figure is the Fig. 6 of the revised version of the manuscript). Second, we show that the models selected on the 1979-1995 period with the same procedure than in the paper are also the best performers over 1996-2011 (see the figure referred to as in Reviewer 2 comment n5). This is probably a better way to validate the method than just changing the end points of the period.

Action: The sentence of p. 2945, l. 7 has been deleted.

12. *p.2946, l.26: A choice of any selection criteria will reduce uncertainty. If, for example, I select all models whose name starts with C or G, I also get reduced uncertainty :-)*

Answer: That is right. By meaning "reduction of uncertainty", we mean that lower and upper bounds for the year of disappearance of summer Arctic sea ice are getting closer and closer to each other as we refine the selection, and tighter than if a random selection was applied. This is now illustrated more clearly in a new figure, Fig. 6, introduced in the text and also referred to as in the answer to Reviewer 2s comment 4.

Action: This new figure (Fig. 6) summarizes the main results of the paper and is now included in the revised version of the manuscript.

13. *Supplemental material: Figure CMIP5.gif is not clear to me. What is shown? For both figures, some description in the supp. material and figure captions would be*

helpful.

Answer: A legend is now provided along with CMIP5.gif and the supplementary figure.

Technical comments

Overall structure

14. *I found it sometimes quite difficult to follow the paper's line of reasoning, since it was not always clear what a certain part of the text was heading at. At the moment, the sections sometimes seem to exist almost independent of one another, and the reader must often infer their logical connection himself. To maybe help future readers to more quickly grasp the storyline of this contribution, the authors might want to consider to*
- *give a section outline of the paper at the end of the introduction*
 - *to more clearly follow and to actually specify a story line like*
 - (a) *We would like to constrain sea-ice projections by better understanding the divergence of model results*
 - (b) *To do so, we examine which observational parameters are related in a specific way to the future evolution of sea ice*
 - (c) *This then motivates what is being done in the current section 3.2*
 - (d) *Building on the finding that certain selection criteria are related to the future evolution of sea ice, certain models can be selected as described in the current section 4 and projections can be constrained.*
 - *change section headings: section 4 is not really a discussion, but contains many new results, section 3.2 is not really a summary of sea-ice projections*

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but more an analysis of which parameters can be selected for which reason etc.

Answer: Indeed, by reading the text again, we understand that the structure is not adequate.

Action: We have moved paragraphs around as to increase readability and the flow of reasoning. These moved paragraphs are in blue in the new manuscript. Thanks for your suggestion of structure, the new structure now resembles what you propose. We modified headings, transferred the main results in the section " Results ", resulting in a smaller " Discussion " section.

Smaller comments

15. *p.2932, l.17: constrained for what by what ?*

Action: The new version now reads : " are likely to be constrained by the present-day sea ice properties "

16. *p.2934, l.18: what is "local mean sea ice"?*

Action: We changed the text : " is marked by a nonlinear relationship between the mean and trend in September sea ice extent. "

17. *p.2934, l.18: where did you mention your "initial idea" before?*

Answer: The " idea " we are referring to is that of the importance of present-day sea ice parameters for long-term projections. Indeed, " initial " is not necessary

Action: " initial " has been deleted.

18. *p.2936, l.20: improvements/tuning: these are two very different things, but the "/" suggests that they are very closely related*

Action: "or " is now replacing " / "

19. *p.2938, l.14: changes or anomalies?*

Action: " (anomalies) " has been deleted.

20. *p.2939, l.14, l.18: I do not see why this is "on the one hand" and "on the other hand". To me, these seem to be rather unrelated things?*

Answer: We wanted to stress the similarities in CMIP5 behaviour in the way they reach ice-free conditions compared to their own baseline state, opposed to the apparently scattered trends in the same models. Nonetheless, we have decided to delete this paragraph as it does not serve the flow of ideas and renders the reading difficult.

Action: The paragraph p.2939 l 11-20 (original manuscript) has been deleted.

21. *p.2939, l.28: "minimum trend" – > maybe more clear to use "most negative trend"*

Action: " minimum " has been changed to " most negative "

22. *p.2942, l.26: please specify what criteria I and V are, most readers won't remember*

Action: The sentence has been changed to " In order to be more quantitative [...] for criteria I (mean September sea ice extent) and V (trend in September sea ice extent) [...]"

23. *p.2942, l.29: please specify how you "slightly changed the end points". Or at least refer to the caption of the figure for further explanation.*

Action: We now refer to the caption of the Figure where the methodology is explained in detail.

24. *References: I don't think you have to embed the page numbers where you cite a specific paper. It is actually confusing, since this is usually not being done*

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Answer: This is one of the requirements from The Cryosphere journal.

25. *p.2953, table 1: MPI-ESM-MR uses a 0.4°x0.4° tripolar grid in the ocean*

Answer: Thanks for this information !

Action: Changed.

26. *I find that the very ample usage of bracketed parts of sentences doesn't really improve readability. For example, almost every sentence of the abstract contains some bracketed parts even to the point that the abstract closes with one of your main results in brackets. In my opinion, if something is important, it should be incorporated into the sentence, if it's not, it can be left out. If the incorporation into the sentence makes the sentence very long, the sentence should be split up. Also generally, it'd be helpful if some long sentences could be split into two. Thanks!*

Answer: Your remark is sound, and the second Reviewer also made a similar remark.

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

27. *There is sometimes some uncommon usage of English throughout the paper. It'd be nice if a re-submission could thoroughly be checked by a native speaking co-author. Examples for such (at least according to my non-native gut feeling) uncommon usage of English:*

Answer: Part of this uncommon usage of English probably arises from the differences between American and British English, as two of the co-authors are native American English speakers. Yet, we take into account most of your suggested changes.

28. *p.2932, l.24: "in large part" – > "to a large part"*

Answer: Either is OK, we keep the current version

29. *p.2933: l.6: "participating to" – > "participating in"*

Action: Changed.

30. *p.2933: l.11: usage of "underlined"*

Action: " This was already underlined " changed to " This point was already raised "

31. *p.2934, l.5: "their" refers grammatically to Wang and Overland*

Action: " their " changed in " the "

32. *p.2934, l.18: usage of "contemporary"*

Action: The sentence has already been changed earlier following one of your comments regarding the use of the word " local "

33. *p.2934, l.19: remove either "current" or "over (the) recent decades*

Action: " current " is now removed.

34. *p.2937, l.15: usage of "is a concern"*

Action: This sentence has been changed to : " However, a relationship can be found by construction [...] "

35. *p.2937, l.15: "even though" – > "even if"*

Action: Changed.

36. *p.2937, l.16: usage of "besides"*

Action: " Besides " changed in " In addition "

37. *p.2938, l.2: usage of "yet"*

Action: " yet " removed.

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38. *p.2939, l.11: remove "of them"*

Action: This paragraph is now deleted anyway

39. *p.2939, l.25: dominate – > dominating*

Answer: This sentence has already been deleted following one of your previous remarks.

40. *p.2939, l.26: usage of "manifests"*

Action: Changed to "is characterized by"

41. *p.2939, l.27: "identified with" – > "marked for*

Action: Changed.

42. *p.2949, l.26: "if the Arctic" – > "as the Arctic"*

Answer: We think D. Notz refers to p. 2940, l. 26.

Action: Changed

Please also note the supplement to this comment:

<http://www.the-cryosphere-discuss.net/6/C1803/2012/tcd-6-C1803-2012-supplement.pdf>

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

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