

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

### **Anonymous Referee #2**

Received and published: 30 August 2012

#### 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.

#### Specific comments

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

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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.

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.

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?

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?

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  $4 \times 10^6$  September ice extent? Again this seems to be related to the location in the trajectory.

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

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of the text, removing the parentheses.

## Technical Comments

Abstract; line 15. “For these reasons...”. This sentence doesn’t make sense to me.

Abstract; line 10. Change “compared to the 1979-2010 model SSIE” to “with respect to the 1979-2010 mean SSIE”

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.

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

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

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.”

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.

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

Section 2 would be a good place to introduce PIOMAS.

Page 2936, line 14. “The CMIP5 multi-model mean trend underestimates...”

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

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Comment

sentence needs to be better explained. I would also avoid using “exactly”. There is always going to be uncertainty.

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 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 km<sup>2</sup>) similar to the variances of model extents and change in extents? The example would be more relevant if they used actual variances.

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?

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

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

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

Page 2940, line 26. Of not if.

Page 2941, line 12. A zero trend does not imply no inter-annual variability. You could

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Interactive  
Comment

have a stationary climate (no trend) but still have inter-annual variability (e.g. ENSO, NAO).

Page 2941, line 21. What is “medium 1979-2010 SSIE”?

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

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).

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

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.

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

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.

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

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