The Cryosphere Discuss., 9, C1440–C1448, 2015
www.the-cryosphere-discuss.net/9/C1440/2015/

The Cryosphere
Discussions
Open Access

Interactive comment on "Improved Arctic sea ice thickness projections using bias corrected CMIP5 simulations" *by* N. Melia et al.

F. Massonnet (Referee)

francois.massonnet@uclouvain.be

Received and published: 19 August 2015

Review of "Improved Arctic sea ice thickness projections using bias corrected CMIP5 simulations" by N. Melia, K. Haines and E. Hawkins

François Massonnet

N. Melia and his colleagues present an original study of CMIP5 Arctic sea ice projections. Starting from the observation that the spread in CMIP5 modelled sea ice thickness (SIT) is large over the contemporary period and even larger for the decades to come, they introduce several recalibration methods to adjust sea ice thickness simulations over the 1979-2014 period to match some reference (the PIOMAS data). They finally achieve a detailed analysis of the relative sources of uncertainty in SIT pro-

jections before and after their correction has been applied. The results show a clear reduction of spread after the bias-correction method has been applied. According to the authors, this comforts the robustness of the recalibration approach, increases the confidence in the projections, and makes these projections more informative for stakeholders.

This paper should be definitely published pending minor modifications. I have to underline the high quality of the text, its logical articulation and the high quality of figures that make this article extremely easy to follow. I hope my comments are constructive and can contribute to make the manuscript even better.

MAIN COMMENTS

This article has at least three original aspects that make it novel, and support its publication in The Cryosphere.

1) It addresses projections of sea ice thickness and treats this variable locally. As underlined by the authors, SIT is a highly relevant variable for stakeholders and the ship industry. It is also much more informative than sea ice concentration (SIC) especially in the central Arctic, where local thinning can occur without major changes in the local area in the future.

2) The method of recalibration Mean And VaRIance Correction (MAVRIC) is at the same time simple and statistically robust. By attempting to correct for the first AND second order statistical moments, the authors bring CMIP5 models in a state that matches closely the reference. Correcting the mean only wouldn't be enough, as shown nicely in the study and in earlier studies (e.g., Blanchard-Wrigglesworth and Bitz, 2014, cited in the manuscript). Another essential point of the MAVRIC method is that it preserves internal variability by rescaling each member on the ensemble mean biases.

3) The authors attempt to decompose the total spread in SIT projections and understand how each of the three sources of uncertainty (model, internal variability, scenario)

contribute to the total uncertainty. This analysis is very informative because it shows that there is room for improvement in our models at least for 50-yr projections, where the scenario is hardly important.

These three aspects have also their drawbacks.

1) One of the drawbacks of using SIT instead of SIC is that SIT is much less constrained by observations. In fact, there are no long-term and spatially homogenous observations of SIT. The authors work around this by using PIOMAS. PIOMAS is the best we have for this type of study, but we shouldn't forget that PIOMAS is primarily a model output where some observations (no SIT observations) are assimilated following a very simple scheme (nudging). The paper by Lindsay et al. (2014, doi: 10.1175/JCLI-D-13-00014.1) and/or Zygmuntowska et al. (2014, doi:10.5194/tc-8-705-2014) could be cited in addition to the others in the manuscript to reflect how uncertain PIOMAS is with respect to observational products.

2) I am more doubtful about the physical validity of the recalibration. When recalibrating for the mean and for the variance (but not the trend in SIT), the evolution of SIT might be physically incompatible with the mean state over the calibration and future periods. In other words, the recalibration would be physically robust if the trends in SIT wouldn't depend on the mean state, but just on the external forcing. There is evidence from the observational record that the September sea ice extent (SIE) is following a quadratic rather than a linear evolution. There is also evidence from CMIP5 models (Fig. 4 of Massonnet et al., 2012, cited in the manuscript) that SIE trends are nonlinearly related to the mean SIE. I don't know whether this is the case with SIT, too. If so, the rate of SIT loss might be biased after recalibration and this could affect the conclusions.

3) The link "lower spread in projections –> more confidence in these projections" is not as straightforward as the authors suggest. It is undeniable that the spread in projections shrinks after the bias-correction method is applied (Fig. 9 of the manuscript). As a matter of fact, models that are forced to look alike in the present will also look

alike in the future. The question is whether this recalibration method does not itself introduce systematic biases in the updated projections. This would be the case if PIOMAS was overly thick/thin in some regions (point 1) above) or if the response of SIT would be mean-state dependent in CMIP5 models (point 2) above). In other words, it is "easy" to narrow uncertainties in projections by recalibration, selection or many other methods; but it should be kept in mind that another source of uncertainty (related to the recalibration/selection method itself) is introduced but does not appear on the final plots.

Here's how I would suggest to address these three main points.

1) To ensure a balanced and more objective introduction to PIOMAS in section 2.1, consider citing the two papers listed above and briefly discuss how current estimates of SIT, including PIOMAS, are uncertain. Everyone knows that PIOMAS is the best we have, but no one should forget that it is not free of errors. I stress that PIOMAS is first and foremost a model output!

2) The second point is touched in the conclusion (p. 3838, ll 13-17), but it'd be good to know how the trend in SIT relates to the mean SIT in different grid points of CMIP5 models. If there is no dependence (constant trend), then a simple recalibration of the trend would be enough - although large uncertainties exist. If the link is nonlinear, then even recalibration of the trend over the historical period wouldn't be sufficient. I'm not asking to change the recalibration method, but simply to investigate how valid the additional recalibration of trends would be for projections.

3) For the last point, I have a suggestion. The authors did train their recalibration method by splitting the PIOMAS period in two parts; while the results are satisfactory, the problem is that the training and testing periods are very short and close to each other. My suggestion is the following: apply the MAVRIC correction on 5 GCMs by taking as reference one of the member of the 6th one (i.e., replace PIOMAS by one member of one GCM). This "sister" experiment could allow to verify that the 5 GCMs

are properly constrained to track the evolution of SIT of the 6th one, and in particular the dates of sea ice disappearance. I know that this requires some (technical) work, but I think that a positive result would strengthen the validity of this method a lot!

I also have a set of shorter comments

OTHER COMMENTS

Listed as Page Number / line

3822/5 Drop "spatial and temporal": biases is enough.

3822/12 Replace "sea ice internal variability" by "climate internal variability on SIT uncertainty"

3823/1 Replace "SIT" by "SIT evolution"

3823/15 "[In the case of SIT], Model bias makes a contribution to model uncertainty". Even for well-behaved (statistically speaking) variables like SST, model bias contributes to uncertainty: working with anomalies does not guarantee that other quantities such as sea water density, or air-sea fluxes, will be consistent after the bias has been removed. I would drop this last sentence.

3823/19 "BC has not previously been applied to projections". See the papers of Boé et al. (2009, doi: 10.1038/NGEO467), Wang and Overland (2009, doi:10.1029/2009GL037820; 2012, doi:10.1029/2012GL052868), Zhang (2010, doi:10.1111/j.1600-0870.2010.00441.x), Mahlsteing and Knutti (2012, doi:10.1029/2011JD016709). The present manuscript is novel in that it recalibrates SIT, and does it locally.

3824/10 As I wrote above, PIOMAS is a model based estimate of SIT constrained by some observations. Consider changing "observationally based" by "model based".

3824/19 Same as previous comment.

3825/1 Sea ice thickness has two usual definitions: sea ice volume divided by sea ice area ("in-situ thickness") or sea ice volume divided by grid cell area ("mean thickness"). In CMIP5 models, mean thickness is reported. Did the authors check that PIOMAS also reports mean thickness and not in-situ thickness? This is to ensure consistence when recalibrating CMIP5 models.

3825/17 Delete sentence "The thickest ice is located north...". This is more descriptive than informative.

3826/1 The criteria chosen to screen the full CMIP5 ensemble are rather subjective ("have a reasonable spatial resolution", "comprise at least one ocean channel in the Canadian archipelago"). Is there a particular reason why these criteria were applied? Other criteria based for instance on sea ice extent would directly eliminate the CSIRO model. Did the authors also apply the MAVRIC method on rejected CMIP5 models? There is no fundamental reason why models without a channel in the Canadian Archipelago would give worse bias corrected SIT in the central Arctic, for instance. How are the results sensitive to the initial choice of CMIP5 models?

3826/5 I suspect that CMIP5 models were interpolated onto a common grid to make the grid-point recalibration feasible. The authors should indicate which reference grid was used (PIOMAS's? A regular 1°x1°?).

3826/15 Change "observed" to "PIOMAS"

3826/16 Change "there is only one realization of the past" by "PIOMAS only yields one realization". In fact, PIOMAS was run with many atmospheric forcings (see Lindsay et al. (2014, doi: 10.1175/JCLI-D-13-00014.1)) but only makes one publicly available. Applying MAVRIC with other versions of PIOMAS wouldn't sample uncertainty related to internal variability, but at least to the atmospheric forcing used to generate PIOMAS.

3826/17 I'm a bit confused here, because I think two ideas have to be expressed separately. First idea: the calibration period is short, hence internal variability pollutes the

recalibration method. Second idea: even if the recalibration was done on a very long-period, it is not sure that the future evolution of SIT would be correct because of the possible dependence of SIT on the mean state.

3826/24 Change "observations" by "PIOMAS"

3826/27 I don't understand the following sentence, explaining why trends are not corrected :"Our reasoning is to keep this as prescribed by the differnet models because the response of the SIT to fugure warming is unknown and GCMs are designed to give an estimate of this". Do the authors mean that it is useless to correct the trends over the PIOMAS period because the trends might anyway be different in future periods? If so, please rephrase.

3827/7 The toy model uses an AR1 process with declining linear trend. How was this choice made? What are parameters of the AR1 model? Did the authors check the auto-correlation properties of CMIP5 SIT evolution to design this toy model? When SIT approaches zero, negative values are reset to zero? All this information would be welcome to be able to reproduce the results.

3827/22 Replace "mean" by "time-mean"

3828/12 Sections 3.1-3.3, illustrating the limitations of simple recalibration methods, could cite the paper of Blanchard-Wrigglesworth and Bitz (2014, cited elsewhere in the manuscript) where the mean-variance relationship of SIT is clearly illustrated.

3829/13 Add "thickness" between "sea ice" and "variance"

3829/15 The authors should define "ice-free" at this point of the manuscript. This concept is defined elsewhere in the manuscript, but it'd be good to have it where it is first introduced.

3830/12 CSIRO also has too much ice areal coverage, this could be added here.

3832/10 How did the authors find that the shift towards earlier ice-free dates is at-

tributed to the change in the variance rather than the mean? Is it a speculative statement or were tests done with and without mean or variance correction in MAVRIC?

3832/13 I wouldn't use the term "projections" over the historical period, rather "simulations"

3835/23 What is the asterisk in SIV*? I couldn't find where this points to.

3835/25 The assumption of 100% SIC in September is questionable. Have the authors looked at SIC in CMIP5 models in September for future periods? It is likely that models simulate values much lower than that. Did the authors try other baseline values for SIC? That is, can the sentence "this assumption should only have a relatively small effect" be supported by objective arguments?

3836/9 Magnitude is always positive. Delete "absolute", unless you want to oppose it to relative magnitude.

3841/11 Did the authors check the residuals (Tˆ2 - Mˆ2 - Iˆ2 - Sˆ2) to quantitatively verify that the independence between the three sources of uncertainty can safely be assumed?

Fig. 1 The colorbars (and colorbars of all subsequent figures) have a bin that goes below zero. This is a bit disturbing, as we know that sea ice thickness is always positive. Following the colorbar conventions, dark blue areas must be ice-free (SIT=0) grid cells, but then white areas must be grid cells with SIT>=2.25 m, following the same convention. Another person could interpret white areas as 2m<SIT<2.25m, though. There might be confusion.

Fig. 2 Same comment as for Fig. 1

Fig. 3 Please add units of SIT along the y-label.

Fig. 4 Same as for Fig. 3

Fig. 5 Same as for Fig. 3

Fig. 6 Same as for Fig. 1. Also, adding the PIOMAS SIT fields would be insightful to report the improvements.

Fig. 7 Same as for Fig. 1. Also, a map with differences (corrected minus raw) would be very helpful to interpret the benefits of the bias-correction method. In the current version of the figure, it is really difficult to see where the corrections occured. A blue-red set of maps with positive-negative changes in SIT could be added as a third row.

Fig. 8 Same as for Fig. 1. Also, make clear that you define the "sources of SIT uncertainty" as the standard deviation of the detrended SIT.

Fig. 9 I would change "Uncertainty" by "Variance" in panel (a), because "uncertainty" has been used interchangeably with "standard deviation" in the rest of the text. Alternatively, you can choose to show the standard deviation but then loose additiveness.

Interactive comment on The Cryosphere Discuss., 9, 3821, 2015.