

Interactive comment on “Assessing bias-corrections of oceanic surface conditions for atmospheric models” by Julien Beaumet et al.

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The authors thank the referee for accepting to review the paper and for the generally constructive remarks aiming at the improvement of the quality of the paper. The responses to the different comments are below :

General comments This paper proposes a way to evaluate bias-correction methods for SST and SIC for future climate projections, using a perfect model approach and a real-case application. There has clearly been a large amount of work in this study and this is clear when reading the paper. The analysis is thorough and the discussion honest, with the main caveats being highlighted and explained (at least, an explanation is proposed). The conclusion is clear and includes potential other methods to investigate.

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However, the presentation of methods and results might be a little bit confusing, given the amount of data.

1. Some extra introductory sentences explaining the point of using a perfect model approach would be welcome, as it might not be obvious to a reader that is not a specialist but wants to learn more. This could be done in section 2.4, where the description of the evaluation method (which is a main point of the paper) is a bit short. More generally, it would be interesting to provide some examples of the use of a perfect model approach in the literature.

Authors' response: "The reviewer is right to state that some reader might not be familiar with the perfect model approach. We therefore added the following sentences that should explain the approach in a nutshell and refer to some examples: "A perfect model approach usually consists of using model data as a substitute for observations, and trying to predict projected model data from that model; this prediction can then be evaluated against the available model projections (e.g., Hawkins et al., 2011). In the real world, as observations of future climate are obviously not yet available, an equivalent approach is impossible if one cannot wait long enough for the future to become reality. Another type of perfect model approach are "Big Brother" experiments for evaluating downscaling techniques. In such studies, high-resolution model output is degraded in resolution and downscaling methods are then applied to these low-resolution data. The resulting synthetic high-resolution fields are then compared to the original high-resolution output (e.g. de Elía et al., 2002). Here, we consider SST and SIC..."

2. If the language is usually clear and understandable, the wording can be unusual and the authors are encouraged to have (another?) correction by a native speaker (which the reviewer is not. . .)

Authors' response: "We had a native speaker correct the revised version."

3. On a more specific point, one might wonder why were the GCMs CNRM-CM5 and

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IPSL-CM5A-LR chosen? Was this a choice based on availability or were these models selected based on their respective performance for representing SST and SIC? It would be nice to have information on this point.

Authors response: "The search for suitable bias-corrections methods and their use was first motivated by the need to drive future scenarios climate experiments with atmosphere-only GCMs ARPEGE and LMDZ. Therefore, the work was started with SST and SIC coming from the corresponding coupled model of these two atmosphere-only GCMs. HadGEM-ES was added later in order to verify if the results obtained were reproduced with this model, but we acknowledge that no criterion based on their respective performances has been used to select these models rather than another one. However, the fact that the results are very close for the three models investigated gives us confidence in the fact that they are robust and independent from the AOGCM chosen as initial material."

Finally some caveats and issues are treated too lightly and would require a more thorough description and explanation (see specific comments). Overall, the proposed paper describes an interesting and detailed work that should be of interest to many users in the climate modelling community. I therefore propose this manuscript to be accepted after the minor changes described in this review document.

Specific comments.

P1 L12: the part about RCPs is not needed, isn't it? The sentence is a bit long

Authors response: "Comment taken into account , the RCP acronym and the corresponding reference is now introduced in the Data section. "Only the first ensemble members of the historical, and of the Representative Concentration Pathways (RCPs, Moss et al., 2010) 4.5 and 8.5 simulations have been considered".

L17 bracket missing somewhere

Authors response: "Ok, comment taken into account."

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L19: Would it be possible to have some other examples from the literature? Surely a list of 4 or 5 references should be easy to find

Authors responses: "Some additional references were add as examples of the considerable literature on the bias of CMIP5 models, especially on SST. References demonstrating the added value of bias-corrected SST have been included as well: "The absence of the Pacific cold tongue bias and the reduction of the double ITCZ problem in AMIP experiments with respect to the CMIP5 model experiments (Li et al., 2014) shows the importance of forcing atmospheric model by SST close to the observations. For instance, improvements in the modelling of the tropical cyclone activity in the Gulf of Mexico (Holland et al., 2010) and of summer precipitation in Mongolia (Sato et al., 2007) were obtained by bias-correcting SST and other AOGCM outputs before using them as forcing for RCMs."

L19 "For example, it has. . ." Authors response: "Ok, comment taken into account."

L20 the seasonal cycle and the trend Authors response: "Ok, comment taken into account."

P2 L24 describe "AMIP" as "Atmospheric Model Intercomparison Project" if it is not done any- where else Authors response: "Ok, comment taken into account."

P3 L25 Is there a reference for the Hann box filter? Why did you choose this filter?

Authors response: "The first reference to Hann function is in "Particular pairs of windows" in "The measurement of power spectra, from the point of view of communications engineering" by R.B. Blackman and J. Tukey, 1959. We have chosen Haan filter because it is the lightest filter amongst the commonly used box filter."

P5 L13 Any information on the number / proportion of GCMs that were dismissed? What does "poorly" mean for the selection process?

Authors response: "We built our library by selecting AOGCMs who have a reasonable representation of the sea-ice extent annual cycle, its maximum and minimum, in

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present climate following the literature (e.g. Turner et al., 2013, Stroeve et al., 2012). For instance, our list for the "real-case" application of the method contains historical simulation and future scenarios of the following AOGCMs: MIROC-ESM, EC-EARTH, NorESM1-M, CCSM4 and IPSL-CM5A."

L13 AOGCMs and remove "overly"

Authors response: Ok, comment taken into account.

P7 L12 "We assume that an ideal bias correction method should reproduce the same change in mean and variance between the observations and the estimated future SST and SIC as between the used coupled GCM historical simulation and the climate change experiment." That seems obvious but is there any reference regarding this issue? Is there any discussion among the scientific community?

Authors response: There is indeed debate about this issue and so far, probably no consensus. For the bias-correction of future scenarios, one usually makes the hypothesis that one can rely on the climate signal coming from a model even if this model has bias in the reproduction of present climate. There are indeed reasons to believe that model biases are time invariant (e.g. Maurer et al. ,2013 (www.hydrol-earth-syst-sci.net/17/2147/2013) although whether we should correct the climate change signal remains an open question (see Ehret et al, 2012 (<https://www.hydrol-earth-syst-sci.net/16/3391/2012/>)).

L23 What is the point of applying the perfect model approach for SST, as we use only "regular" bias correction? You highlight this issue, but you might want to shrink this section a bit.

Authors response: Indeed, the part on bias correction of SST is less novel. Following this remark and remarks of the second reviewer, the part on the methodology for the bias-correction of SST as well as the part on the perfect model test have been shrunk.

P8 Fig4 Are you sure about the color? There seems to be a very large initial bias be-

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tween the obs and the historical simulation for North Atlantic, is that expected? Moreover the RCP4.5 looks quite cold compared to the corrected values. If this is correct, can you highlight and explain that in the text?

Author response "The colors are right. The North Atlantic is a region where coupled GCMs often exhibit large biases (usually cold biases) because of their poor skills in representing correctly the Atlantic Meridional Overturning Circulation (AMOC). This example is indeed another argument for the bias correction of SSTs.

L11 "methods" Authors response: Ok, comment taken into account.

L12 delete "in" Authors response: Ok, comment taken into account.

P9 L9 This comment is valid for the whole paper, but is the use of "biases" valid when describing the results of the perfect model experiment? It is a bit confusing with the original bias that we are trying to correct. Again, if it has been used previously in the literature in that context, I'm ok, but maybe "difference" or "error" would be clearer, as it is a bias created by the method, and not a bias originally in the data

Authors response: Comment taken into account, the term "error" or mean "error" is now used throughout the text in order to make it less confusing.

P10 L12 "more or less" – can we find a more scientific term please? Authors response: Ok, comment taken into account.

L14 "is easy to explain" – Is it? Can you develop, please? Authors response: Ok, comment taken into account, some explanations are given in the text : "The presence of such peaks is easy to explain by taking into account the structure of the LUT as i) for a given month, the SIC does not always increase monotonically with decreasing SST, ii) the discrete nature of LUT is not in favour of a continuous SIC frequency distribution."

L29 Should an ideal method apply the same statistical changes? It sounds right, but what about skewed distribution (precipitation) where the BC would change the distribution, therefore changing the distribution of changes? I think there is quite a discussion

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about that topic, so, if I agree with you, I would change to “We consider here that an ideal method. . .” Authors response: Ok, the sentence has been modified following your recommendation.

P11 All text – Would it be possible to have some correlation value in order to quantify the error among the different methods? Maybe a correlation coefficient, or the value of the minimum, maximum and mean error for each graph?

Authors response: Mean errors and root mean square errors for each graph are added on the plots. In the text, we now discuss the average mean error or average RMSE for every scenarios and for the Arctic and Antarctic combined in order to quantify and compare more objectively the errors between the three methods.

Fig8 and 9 It is difficult to see which point correspond to what – Maybe adding a letter to each of them to point to the region would help – Please try but it might make the figure impossible to read. It would be nice to be able to navigate alone within the points

Author response: We changed the legend of the figure so that we can now distinguish the different regions with the help of different colors. Different signs (crosses and circles) are used to distinguish scenarios from CNRM-CM5 and IPSL-CM5A-LR. The more important for these figures is first to distinguish the regions, then the models. The distinction between rcp4.5 and rcp8.5 is less essential for the interpretation of the results and the connections with the text.

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