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Interactive Comment

Interactive comment on "Methodological aspects of a pattern-scaling approach to produce global fields of monthly means of daily maximum and minimum temperature" *by* S. Kremser et al.

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We would like to thank all three reviewers for their positive and constructive comments and feedback. The reviewer comments are answered in detail in the following, where the original reviewer comments are in italics:

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Anonymous reviewer $\#1$

Overall Assessment

This manuscript is a valuable contribution towards constructing large computationally cheap climate ensembles as a function of external forcing on a regional to local scale for climate assessment and impact studies. The fundamental approach and several enhancements used to downscale the global simulations (or observations) and inflate the ensemble, based primarily on linear regression, are well presented and the major limitations of the procedure are discussed.

2 **Comments**

The assumption in the CPSM approach of linearity across scenarios in the response of the regression predictor to the predictand(s) may limit its applicability for most variables strongly influenced by large scale circulation, for example as in case for regional precipitation. This is even true for surface temperature response to forcing as discussed in Section 3 and Fig. 5 for the Arctic ocean. This climate sensitive region is subject to enhanced feedback mechanisms due major changes sea ice distribution. It is anticipated that similar nonlinear effects may occur for example in regions subject to strong orographic forcing.

We agree with the reviewer on the above points.

3 **Minor revisions**

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Section 4.2: Since the basis time series are orthogonalized, direct interpretations with the original basis is no longer valid. Thus the orthogonalized basis may not be labeled as northern/southern hemispheric land/ocean temperature in Fig 6.

The reviewer has a good point here and we now introduced a new subscript ($\perp$). This subscript symbolizes orthogonal basis functions. We also added a few lines to the text to explain the new subscript.

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Anonymous reviewer $\#2$

This is a very helpful and well written paper. Thank you very much for this exploration of the pattern scaling approach! I only have some minor comments:

1. The highlights of the paper (what is new in comparison to earlier approaches) should be clearly stated in the abstract: (i) test of hemispheric ocean land temperatures as additional predictors, (ii) selection of the training data set for a given global mean temperature pathway, (iii) test of time dependence of scaling coefficients). It is somehow hidden in the description of the results.

We followed the suggestion of the reviewer and added the following to the abstract: "The methodological aspects of the CPSM explored in this study include: (1) investigation of the advantage gained in having five predictor time series over having only one predictor time series, (2) investigation of the time dependence of the fit-coefficients and (3) investigation of the dependence of the fit-coefficients on GHG emissions scenario. Key conclusions are:. . ."

2. p 4840, l 15 and 16: Maybe it is clearer to call it "forced change" instead of "forced variability"

We agree with the reviewer and replaced "forced variability" with "forced change".

3. Figure 2: Explanation of black and blue dots should be added to the caption. It is only given in the text.

We added the explanation to the caption of Fig. 2.

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4. p 4842, l 18-19: The CPSM does not emulate the "variability". I would delete the term here. Or do you mean the seasonal cycle?

No, we are not referring to the seasonal cycle. We agree with the reviewer that to use the term 'variability' might be misleading and we replaced that here with:

". . . i.e. that the magnitude of the temperature change in the Arctic in response to a change in global climate forcing tends to be significantly larger than the magnitude of the global mean temperature change."

5. Figure 5: Would be good to add the a column of the differences between column 1 and column 2

We have given this suggestion of the reviewer careful consideration. The purpose of Fig. 5 is to qualitatively compare the temperature changes simulated using the CPSM and those simulated using the HadGEM2-ES model. A quantitative comparison between the two is unlikely to provide data of use to any reader and would increase the complexity of Fig. 5. We have therefore decided not to add a third column to this figure.

6. p 4845, l 12-13: I think it is the difference between the decadal mean (2090-2099) and the average over the reference period (1961-1990)

We clarified what is shown in Figure 5 and re-worded the sentence to:

"To further compare the results of the HadGEM2-ES RCP45 simulation with the

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generated fields by the CPSM, global maps of the decadal mean (2090 to 2099) of T'_{max} and T'_{min} for January and July are displayed in Fig. 5, i.e. the change in T_{max} and T_{min} by the 2090s with respect to the baseline period."

In addition, we corrected the titles of Fig. 5 from T_{min} and T_{max} to T'_{min} and T'_{max} which might have been misleading.

7. Figure 6: Hatching and thin black line are really hard to see

We tried to make the hatching and the thin black more visible in the new version of this figure.

8. p 4847, l 24: 'applying the regression model to $T'max$' instead of 'applying the regression model to changes in $T'max$'

We implemented the suggested change.

9. Is the "Assessment of the value of using multiple basis functions" done in cross-validation setting, i.e. the training is based on e.g. RCP8.5 while the test of the performances is based on the other scenarios? It should be done in this way. Otherwise it is not surprising that the performance it better for the model including more predictors.

If the CPSM were trained on RCP 8.5 and the fit-coefficients obtained were applied to a different GHG scenario there would be two components of the sum of the squares of the residuals (SSR), a component arising from the regression model fit and a

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component arising from the differences in the scenario. Because we want our metric of the improvement following the use of 5 basis functions over 1 basis function to not be confounded by differences in scenarios we have calculated the SSR from the same scenario used to do the fit. This provides a purer metric of the results of the improvement of the fit.

10. p 4849: I would call the paragraph "Scenario dependence" instead of "Linearity" as this is what you actually check for the scaling coefficients.

We agree and made the change.

Figure 8 shows that there is some scenario dependence of the scaling coefficients that is not necessarily monotonous with the forcing. Therefore your test for a linear dependence of the scaling coefficients on the forcing may not find these dependencies.

We agree that Fig. 8 will not show that dependence. However, where the dependence is not clearly linear Fig. 9 would show values in the trend in alpha across the three RCPs that are not statistically significantly different from zero.

I would recommend showing a map of the ranges of the scaling coefficients at each grid point (devided by their mean values).

We understand what the reviewer is requesting, however such a metric would not differentiate between a linear dependence of the fit-coefficients on the forcing and a non-linear dependence since both the range and the mean value can be the same for linear and non-linear dependence. Rather we feel that our Fig. 9 as it stands indicates

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that information more robustly.

11. p 4853: T' global AOGCM has to get an index "i" although there is not much space

Yes that is correct and we added the index "i" to equation 4.

12. p 4856: The improvement by allowing for a time dependence of the fit coefficients should also be tested in a cross validation setting (e.g. training on RCP8.5 and application to RCP4.5, see point 9)

Same arguments as for comment 9 apply here.

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Anonymous Reviewer $\#3$

This paper applies pattern-scaling to mean monthly maximum and minimum temperatures. Pattern-scaling has been adopted by many authors now, as a way to interpolate between existing compute-intensive GCM simulations for alternative pathways in atmospheric gas concentrations. However most of these have concentrated on monthly means. This paper is novel because it applies to monthly maximum and minimum temperatures.

The Introduction suggests that one way to train pattern-scaling is against measurements, which of course removes any model biases. I particularly liked Figure 2 in that regard, but this only gets the shortest of mentions before moving on to using a GCM instead. Possibly for a future paper, but Figure 2 could be readily extended to many other areas where CRU data exist and there are sufficient weather stations. Or possibly even use ECMWF re-analysis data?

The reviewer is correct in saying that the CPSM model can be trained on observations; however, for the main purpose of this paper, i.e. to demonstrate/investigate the methodological aspects of pattern-scaling, we trained the model on AOGCM data and not on any observations. The issue is not so much lack of availability of observations but more that the climate signal over the period for which observations are available is less robust than over the period 1961 to 2100 where AOGCM data are available. Figure 2 was included in this paper to demonstrate the advantages/disadvantages of using data from a shorter-term period (1961-2012) over using data from a longer-term period (1961-2100). The data for the short-term period could be obtained from observational data, such as CRU, however this was not done in this study. We would like to clarify that no data in this figure were obtained from observations but rather were obtained from an AOGCM simulation (HadGEM2-ES simulation under RCP 4.5).

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This is made clear in the text. That said, we agree with the reviewer that the regression model described in this paper could be trained on CRU data or ECMWF re-analysis data to reduce the influence of any model biases coming from an AOGCM.

I realize this paper is a more a description of technical developments, as appropriate for this journal. However a little more background would be appropriate in the Introduction. Why are we interested in max / min daily temperatures (risk of heat stress, changes in overnight ice conditions, impactions for crop resilience)?

Following the suggestion from the reviewer, we added a few lines to the introduction: "This CPSM provides a tool to conduct regional-scale climate projections for a range of climate variables such as daily maximum and minimum temperatures for a wide range of GHG emissions scenarios. Using maximum and minimum temperatures as opposed to monthly mean temperature is more useful for some specific applications e.g., for agriculture and for public health officials tasked with providing warnings of extreme climate events. Crop growth, and therefore crop yields are more sensitive to maximum and minimum temperature than to daily mean values. Furthermore, probabilistic projections of T_{max} and T_{min} provide means to estimate future energy demand, e.g. increase/decrease in the usage of air conditioning or heating. As a result, this study uses monthly means of daily maximum and minimum temperature rather than monthly means of daily mean temperature to explore the methodological aspects of climate pattern scaling."

Extremes are of major interest now, and how these may change in to the future. Given the data analyzed is daily, would the authors be prepared to speculate as to whether patterns could be built to capture maximum and minimum daily temperatures for each month?

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First it must be noted that this paper uses monthly means of daily data. The reviewer is correct it would be easy enough to model e.g. the monthly maximum of T_{max} rather than the monthly mean of T_{max}. Since the purpose of this paper is to investigate the methodological aspects of climate pattern scaling rather than changes in extreme events, this has not been done here but we will consider it in a follow up paper.

Certainly the recent SREX IPCC report could provide a steer on what might be needed, even if this is only mentioned in the paper Discussion. Figure 2 shows very noisy data, so maybe patterns are not possible for that quantity?

We do not understand this comment because the data in Fig. 2 are not noise at all. But the reviewer has a point; the CPSM approach can be applied to metrics of the probability density function (PDF) of the climate variables considered, e.g. the change in the standard deviation of the PDF can be modelled using the CPSM approach. The evolving PDF can then be used as the basis for stochastic weather generator and thereby create large ensembles of climate future of daily data including effects of changes in extremes. However, this is far beyond the scope of the paper. We are reluctant to add any material to the discussion on this given that this has not been explored in this paper.

It was slightly disappointed that only one GCM was analyzed, given the availability of models across the CMIP5 database. I do realize that the CMIP5 database isn't always as easy to work with at times, and to cover the complete set of AOGCMs would be a relatively time-consuming task.

To avoid this paper becoming overly long and convoluted, we selected a subset of the myriad methodological aspects of climate pattern scaling that could have been explored. One aspect we choose not to explore was the use of simulations from different AOGCMs. This was not because we do not have the data nor the task would be too time-consuming but rather in current pattern-scaling studies only a single AOGCM is used to train the regression model. This is because when a single AOGCM is used any bias between the AOGCM response and the response of the SCM used to provide the basis functions can be accommodated. The main purpose of this paper is to present a description of a pattern-scaling approach and to present the methodological aspects which haven't been discussed in the literature to date. To extend the analysis to additional CMIP5 simulation and to simulations extended beyond 2100 will be subject of a follow-up study as mentioned in the paper.

Additionally, I guess the methodology could also be applied to other variables?

Yes that's correct.

It might be worth mentioning that (e.g. mean maximum windspeed?). A discussion part of a paper is always more interesting when it points out additional things that need doing.

The CPSM can be applied to different variables. However, we have not explored CPSM application to variables other than T_{max} and T_{min}. Since we can't make robust statements about any other variables we are reluctant to include that into the discussion. We would suspect that a variable like maximum windspeed would be noisier than T_{max} or T_{min}, and more regionally dependent. Therefore a high-quality and high spatial resolution dataset would be required for the training of the CPSM. Furthermore, non-linearity might become more of an issue when looking at the windspeed. Those details have not been explored here and would be subject of a follow up study.

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Ultimately pattern-scaling of course requires applications. Hence if the authors want to use this for different emissions scenarios (and thus radiative forcings) to obtain $T_g'lobal$, then some sort of (1) Energy Balance Model will be needed along with (2) a carbon cycle model. Again, might be nice to at least mention this in the Discussion, even if only to point out available models (MAGICC for instance) – and to cite them. Some plant physiological responses might depend heavily on the range of daily temperatures. Hence this analysis has implications for ecosystem impacts modelling for alternative future changes to atmospheric GHG composition. This could be a suggested addition i.e. include the patterns of this paper, and in to models where pattern-scaling has been combined with land surface models (e.g. IMOGEN: Huntingford et al 2010, GMD).

We agree that these points raised by the reviewer are interesting and relevant to pattern-scaling. However, they all go well beyond the intended scope of this paper which is to explore a subset of some methodological aspects of climate pattern scaling rather than discussing and exploring specific application of the CPSM.

Figure 4 shows a property of pattern-scaling that other authors have found for precipitation at least (papers by Good, Lowe etc of the UK Met Office, Exeter). That is pattern-scaling can start to fail when moving away from business-as-usual situations. This is clearly evident in the more stabilization RCP2.6 curves of Figure 4, and is deserving of more mention – including in the Discussion.

Rather than putting this into the discussion, the issues around scenario dependence of the fit-coefficients are discussed in detail in section 5 and we see no value in repeating this material in the discussion section.

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It may be that things are even more difficult to predict with pattern-scaling when looking at "overshoot" scenarios. It is good (and honest) that Kremser et al have used one GCM simulation to train data, and then tested against other simulations for different RCPs. The analysis of patterns is comprehensive and very well presented. The statistical assessment appears robust.

In summary, this paper should be published. It is one of the first attempts to take pattern-scaling beyond just climatological mean changes, and as such will be useful – and thus cited – by future researchers.

Minor thing: I would recommend that the running title mentions this is about daily maximum and minimum temperatures.

Following the suggestion from the referee we changed the running title to: "Methodological aspects of climate pattern-scaling, applied to daily max and min temperatures"

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