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Climate of the Past Discussions

Interactive comment on "Reconstruction of high resolution atmospheric fields for Northern Europe using analog-upscaling" *by* F. Schenk and E. Zorita

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Response to Referee #1

We thank referee #1 for her/his helpful and constructive review. Our responses are as follows:

Referee:

"A central problem with the approach is the low degree of explained variance based on real data (e.g. page 835). Even if the skill of the AM is comparatively high the method may therefore not be that useful."

Response:

The referee is right that there is a loss in the explained variance caused by the differences between simulation and corresponding observations which has an important negative effect on the reconstruction. This is a principal drawback of all comparable approaches as the reconstruction usually cannot be better than the used regional model. However, this deficiency may be due to model errors and also to the internal model variability: the simulation has been driven at the boundaries by meteorological reanalysis, but in the interior of the domain the model is free to generate its own internal variability, which in general will deviate from the observations. The degree of lost explained variance will depend on the used model (and the size of the RCM domain). Spectral nudging, in which the regional model is also driven towards the observed large-scale atmospheric dynamics at each time-step, is one possibility to reduce this effect and should be used if available (see below).

Referee:

"As stated in the manuscript SLP is not a very good precursor for variables like temperature, precipitation and cloudiness. The authors also use monthly mean T2m as a predictor for reconstructing monthly mean temperatures acknowledging the fact that long-term temperature trends may not be captured by the SLP patterns. An interesting question here is to what degree the method could be extended and used in a multi-dimensional framework including not just SLP but also T2m (and/or other variables) as proxies at the same time?"

Response:

To use different variables at the same time as predictor is indeed a very interesting point to further explore the potential of the AM. Unfortunately, whereas such an approach can be easily tested within a surrogate climate of the regional model, it is hampered by the size of the analogue pool. On monthly scale, T2M and SLP and perhaps precipitation can be used as predictors in regions where such historical data is available. How-

ever, as the dimension of the space grow in which the analogue search is performed, it becomes increasingly difficult to find suitable analogs in a 50 year analog-pool. The problem becomes simply intractable. The solution pursued in our study, dealing separately with SLP and temperature as predictors is not totally realistic because it does not consider explicitly the cross-dependencies between these two predictors. This was however beyond the scope of the current manuscript given no appropriate long-term data would be available to do such a reconstruction.

Referee:

"How would the correlations for 10-year periods shown in Figure 3 differ if the comparison was not made with the full 50-year period but instead any other 10-year period within the 50 year period? To some degree such a comparison could be used to address the question of stationarity which is a main problem with the analogue method (e.g. page 821, lines 25-29 “difficult to estimate . . . valid outside the reference period. . .”)."

Response:

We think that stationarity becomes an issue when the climate evolves outside the range observed in the analog pool. On a daily base, this is very unlikely in the period considered (even if some singular days will not have appropriate analogs). Our concern related to stationarity or the need for extrapolation outside the analogue pool relates to longer time scales, where completely different conditions (e.g. spatial patterns) might have been prevailing than observed in the analog pool (e.g. an higher north-south temperature contrast due to higher sea-ice coverage in the Arctic). If the AM would have problems to find appropriate patterns for the past we would see an increase in the Euclidian distance in Eq. 1, which does not happen in our case. We will modify the sentence on page 821, lines 25-29 to “. . . valid on longer timescales.”

We previously did a cross calibration and validation with 25 vs. 25 and 10 vs. different 10 year periods. Not only the different periods showed a very similar correlation but

also the limitation to 25 or 10 years for the analog pool did not fundamentally change the level of correlation. The conclusion is thus the same as shown in Fig. 3, where for C only 10 years were used to reconstruct 50 years using a leave-one-year-out approach (see also response to referee #2). We think that section 3.2.1 and 3.2.2 show that increasing the amount of analogs does not increase the correlation much – at least not for the 50 years presented here.

Referee:

"The RCM produce an internal solution that is more or less governed by the large-scale features given from the GCM. In winter the steering from the large-scale is generally stronger while in summer the RCM is freer to develop its own solution (Déqué et al., 2007). In case of downscaling reanalysis data it is beneficial if the large-scale within the model domain is in close correspondence to the driving reanalysis data. One approach used in regional climate modeling to infer a stronger coupling between the large-scale and the internal solution is that of spectral nudging. Did you consider using that kind of RCM data here? An example of such data is the GKSS CLM-simulation in the ENSMBLES project."

Response:

The reviewer is right that spectral nudging would bring the RCM closer to observations also in the interior of the domain. We plan to repeat the reconstruction with fields from the coastDat (http://www.coastdat.de/) hindcast from HZG (formerly GKSS) where spectral nudging was used. This reconstruction will be identical to HiResAFF but the fields of each day are replaced by another model. For this manuscript, the use of the models RCAO /RCA3 was dictated to maintain the same RCM for the period 1850 to 2100 as used by the project partners of ECOSUPPORT (Meier et al., 2012). The output of this regional coupled atmosphere-ocean model is used as forcing for their ocean ecosystem models. Hence, lower daily to monthly skill was accepted in favour of consistency of the model fields over the whole time period (see chapter 3.4, p 850,

line 21ff., now 4.1).

We added this information to the manuscript to motivate the usage of spectrally nudged simulation if available. We therefore rewrote parts of section 3.1 (now 4.1) and added the aspect of spectral nudging to the discussion as section 4.1 adding references (von Storch et al., 2000, Yoshimura and Kanamitsu, 2008).

Referee:

"There are a large number of numbers given in the text which makes it a bit impeding to read. The numbers are often also given in the Figures where they are sometimes not so easy to read against the colored background field (for instance Fig 6 for July total cloudiness). I suggest that you collect all the numbers to a few Tables. In this way the reading of the text will be easier and you would also benefit from having all the numbers at one place easy to compare. There you could elaborate with bold/italics to stress what is significant or not."

Response:

We collected all numbers to three tables as suggested and removed the numbers from the text.

Referee:

"I suggest that the text in the result part is somewhat shortened and that there could be more focus on discussing the results. Introducing tables as suggested above would imply that you need not state all results explicitly in the text. Also, there is some mixture in the text when it comes to what are the results and how these are interpreted. Most of the subchapters in Ch 3 end with a paragraph that is really more a conclusion or discussion of the results. I suggest to move these parts into the last chapter (that should really be labeled “Summary and Conclusions” or “Discussion and Conclusions”). Furthermore, ch 3.4 is not presenting results but only discussing them and it should also be moved into the last chapter."

Response:

We shortened the results section using now tables and moving the concluding paragraphs from chapter 3 to a new chapter 4 (Discussion).

Our responses to detailed comments:

1) we removed it

2) done

3) we added Graham et al. and also Schimanke et al., 2012 for the Baltic Sea

4) Here we can only relate to the work done by Matulla et al., 2004. Whether comparable results are achieved for other regions has not been evaluated till now and might depend on the variable and e.g. topography.

5) done

6) we highlighted this aspect later in the manuscript but will add this also to the introduction

7) we will clarify more about this aspect in the text. RCAO is used in all cases with the exception of the T2M reconstruction, where daily anomalies of T2M stem from RCAO and the monthly mean of T2M from RCA3, therefore relating the T2M reconstruction to RCAX. As stated in the text, the monthly mean T2M from the RCAO displays some shortcomings in winter due to problems with the sea-ice fluxes in some areas. According to SMHI we clarify the references as:

RCA3 model = Samuelsson et al., 2011

RCA3/ERA40 simulation = Christensen et al., 2010

RCAO/ERA40 = Comparison of RCAO 25km and RCA3 25 km is Meier et al., 2011b

8) We generally agree that interpolations should be avoided when ever possible. This decision was, however, taken by the ECOSPPORT project, where generally regular lon-

lat grids were used as forcing fields. The resolution of the output is 0.25° as described for the simulation above.

9) Our search is always restricted to calendar months m (one or three), and so it is not symmetric around the calendar target day.

10) we added Déqué in this context and moved the paragraph to the discussion (Sect. 4.1)

11) the reviewer is right – we reworded this paragraph and moved it to the discussion

12) done

13) added

14) We think this section should remain as it is. The reproduction of the distribution types by the AM might be obvious for those being familiar with the method. We wanted to indicate to which extent deviations are still possible. E.g. referee #2 did not agree that the distribution types are “clearly” reproduced in all cases.

15) we only use wind from RCAO where no gustiness parameterization is used. We removed RCA3 from the sentence as it was not used for wind here.

16) corrected. As indicated in the text, the dashed line represents the model data, the solid line the reconstruction. We added this now also to the figure caption

Corrigendum:

Please note that we corrected Fig. 7j which was showing again WS10 instead of PREC. Numbers and the description in the manuscript have been correct.

Interactive comment on Clim. Past Discuss., 8, 819, 2012.