

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

### **Anonymous Referee #1**

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#### General comments

This manuscript presents an evaluation of the analogue method as a means to reconstruct high-resolution atmospheric conditions for historical times. By selecting analogues from a reanalysis-driven simulation with a regional climate model the authors demonstrate in a clear way the potential skills and shortcomings of the method and present the robustness of the method by a number of sensitivity tests. I think that the paper is well-structured and the results are mostly presented in a clear way although the presentation is relatively lengthy in some sections. I have a few questions/suggestions that I think should be addressed before the paper can be considered

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for publication.

\* 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. 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?

\* 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 . . .”).

\* 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 ENSEMBLES project (see <http://ensemblesrt3.dmi.dk/>).

\* 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

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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.

\* 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.

## Detailed comments

1) Last sentence of abstract should be removed. It is not relevant for the abstract where the preceding sentence is the important one.

2) Page 820, Line 23, add “climate change impact related studies including” before and “, hydrological” after “ocean”.

3) Page 821, line 15. It says “or few centuries” but the references does not include any work related to such simulations. Please include one or a few (Graham et al., 2009 is one example relevant for the Baltic Sea area).

4) Page 824, lines 6-10. Here, the performance of the AM in previous studies is discussed. An interesting question that I think needs to be touched upon is whether these statements are of general nature or if they are related to the specific data sets that have been used in the cited studies?

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5) Page 825, lines 21-22. Consider changing “more than 100 years” to “more than 150 years” in case the observations date back until 1850 as stated.

6) Page 825, line 25. Yes, it is true that the reanalysis-driven RCM is to some degree correlated with station data. But, it should be noted already here in the introduction that RCMs can deviate from the global driving model in the interior of the model domain. This is true in particular in summer when the forcing from the lateral boundaries is relatively small (see also my general comment on spectral nudging above).

7) It is a bit confusing with the use of data from two climate models (RCA/RCA3 and RCAO). The naming convention is not strict and it not easy to understand why you sometimes use one and sometimes another version. The cited works by Kjellström et al and Samuelsson et al do not deal with any 25 km simulations but with corresponding 50 km simulations. A better reference for the RCA3 ERA40-driven simulations at 25 km would be Christensen et al., 2010. Samuelsson et al, however, is the main RCA3 reference.

8) I fail to see why the output of the simulations needs to be interpolated into a regular geographical grid (Page 828, lie 2-3). Wouldn't it be better to retain the original model grid in this case in order not to remove any information due to interpolation? Also, what resolution of the output grid?

9) Is the search for days restricted to a calendar month (e.g. April), or, is it restricted to a month +/- 15 days from the day in question? Page 830, line 25-26.

10) A good reference on page 835, line 14-15 is Déqué et al., 2007.

11) Page 835, line 16-17. This is wrong. RCMs are not designed to explicitly resolve small scale processes like convection (in this particular case a grid spacing of a few hundred meters would be needed).

12) Page 835, line 18-19. Change this sentence into “The RCMs have been fed with reanalysis data only at the domain boundaries and SSTs within the model domain.”

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13) Page 836, line 4. Van den Dool et al is missing in the reference list.

14) Consider removing (or at least shorten) Chapter 3.3.4. This chapter states that the analogue method produces roughly the correct frequency distributions which it is supposed to do as it is only reordering the data..

15) Page 850, Line 18. RCA3 do include a gustiness parameterization (see, Samuelsson et al. 2011).

16) Figure caption of Fig. 9. Spelling of Haparanda is not correct. It is not clear what the dashed and full lines represent.

\* Christensen, J.H., Kjellström, E., Giorgi, F., Lenderink, G., Rummukainen, M., 2010. Weight assignment in regional climate models. *Climate Research*, 44(2-3), 179-194.

\* Déqué, M., Rowell, D.P., Lüthi, D., Giorgi, F., Christensen, J.H., Rockel, B., Jacob, D., Kjellström, E., de Castro, M. and van den Hurk, B., 2007: An intercomparison of regional climate simulations for Europe: assessing uncertainties in model projections. *Climatic Change*. 81 (Suppl. 1), 53-70. doi:10007/s10584-006-9228-x.

\* Graham, L. P., Olsson, J., Kjellström, E., Rosberg, J., Hellström, S.-S. & Berndtsson, R. 2009: Simulating river flow to the Baltic Sea from climate simulations over the past millennium. *Boreal Env. Res.* 14: 173–182.

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