

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

Anonymous Referee #2

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This paper presents an application of the analogue method in the reconstruction of high resolution atmospheric fields. In my opinion, the paper presents an interesting application of a method commonly used for downscaling in an alternative setting (up-scaling), something which is not common in the literature. Additionally, despite the comments that I will detail below, the paper shows a thorough analysis of the sensitivity of the method to several key parameters in the design of the algorithm/procedure. In my judgement, the authors comprehensively test the main potential pitfalls of the procedure. Therefore, I would recommend the publication of the paper after the minor issues raised below are corrected.

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MAIN POINTS

I think the authors are not being careful enough in the management of the references. For instance:

* Barnett and Preisendorfer, 1978, cited in line 20, page 822 is not listed in the bibliography list.

* van den Dool, 1994, cited in line 21, page 822 and line 9, page 823 is not listed in the bibliography list

* Fernandez and Saenz 2003 is cited in lines 14, page 823 and line 27, page 824, but it is not listed in the bibliography list.

I have only systematically checked up to this point (page 824), but I think that it is very likely that there are other cases such as the ones above in following pages. Authors must check that all the cited papers are listed in the bibliography list and that all papers in the bibliography list are cited.

Section 2.3. Perhaps I have missed it, but I do not see clear how do the authors arrange for the reconstruction of monthly data. Do they compute monthly averages of predictor (SLP) and use them to find analogs of monthly averaged predictand? Or do they use daily predictors to compute daily predictands to get after this step the monthly average of the predictand? The authors explain the method quite clearly for temperature, but it is not clear for me how do they proceed with other variables, such as the ones shown in figures 6-7.

p 832, lines 9-10. I do not see clear why the slope in the decay of the correlations with rank is the best measure of the density of suitable analogs. I think this must be justified either by a strict mathematical proof or by worked examples. I feel this point should be treated with a deeper discussion by the authors. It is probably true, but I don't think it is evident at all. I think that the authors must better explain the content of the figure. Do they show the correlation of the analog-based forecast with rank using the n-th rank

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analog? Or, are they using the correlation of the analog-based forecast with rank using the AVERAGE of the leading n-th best analogs? I think this must be clearly stated.

Pages 839 to 841. The authors show several maps where they count the amount of times a local significance test is/is not rejected. I am afraid that in some cases, global significance (Wilks, 2006 and references therein) is not warranted. Could the authors evaluate it? Wilks, D. S., 2006: On "Field Significance" and the False Discovery Rate. *J. Appl. Meteor. Climatol.*, 45, 1181–1189. doi: <http://dx.doi.org/10.1175/JAM2404.1>

Page 848, lines 20-25. The authors mention that the search of a sequence of analogs improves a better simulation of persistence. However, they also mention that it is harder to find a sequence of analogs than a good analog. However, they do not show any additional verification under this setup of the method. If it is harder to find good series of analogs, I guess that correlation or fraction of variance should probably be worse than in the previous setup. I think the authors should show some results about this, in case it is real.

MINOR POINTS.

When mentioning reanalyses (page 822, lines 6-12), I feel that ERA Interim should also be cited by now. <http://onlinelibrary.wiley.com/doi/10.1002/qj.828/abstract>

Regarding the discussion by the authors in lines 8-10 in page 827, I find this reference quite relevant: Vautard, R., and P. Yiou (2009), Control of recent European surface climate change by atmospheric flow, *Geophys. Res. Lett.*, 36, L22702, doi:10.1029/2009GL040480.

Page 828. Authors should justify/explain why do they regrid fields. Common RCO grid??

I am not completely sure what the authors mean in lines 9-10, page 830. I find easy to accept that the precipitation at days d , $d-1$ is dependent on the evolution of the SLP field at d , $d-1$, but I do not find easy to accept which other "several mechanisms"

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(using the authors' words) can explain precipitation's persistence. I think this should be reworded.

Page 832, lines 1-3. I guess that the authors use C1 to C5 decades as the analog pool and then check the ability of the analog method to reconstruct the whole period. I guess that the skill score might be a little bit (not too much) optimistic, due to the autocorrelation or decadal variability. I think that leaving out just one day is probably not enough in terms of autocorrelation, although I haven't computed the autocorrelation in the series. Perhaps checking the ability to reconstruct the 40 years not in the C1 to C5 subsets would be a tougher test. However, I don't expect the results would be very different. They already consider the autocorrelation in their significance tests (p833) but, anyway, verification and formulation of the models should ideally be carried out with independent information.

Page 834, lines 3 to 7. I think that after this paragraph the authors should discuss somehow the fact that the model, although considered as "true" is probably underestimating the real spatial variability of the field, so that, in my opinion, their estimation of the ability of the method is probably a little bit optimistic. They show some results consistent with this a little bit later in the text, I think they should explain something about it now.

Page 836, lines 20-23. Is this phrase complete? Do the authors refer to "n=1 to n=10" at the beginning, when they refer to a 6% decrease?

Page 845, line 26. Authors mention "The reconstruction CLEARLY reproduces". I would say it is far from clear in some cases, such as precipitation, for instance. I would recommend a more realistic phrase here.

Figure 4. Left label states "correlation [-1,1]" but range of plot is [0,1]. Do the authors refer to absolute value of the correlation? Correlation squared? Or is it really correlation but just plotted the [0,1] range? According to the case, the authors might have to change the label.

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Figure 6. I think the authors must rewrite the caption to clearly explain which quantities are represented in each panel. They mention RCAO/ERA40, and I guess that means RCAO (forced by ERA40), therefore, I don't see the point of mentioning here ERA40, since in the previous cases RCAO has been used. Expressions "(left columns)" and "(right columns)" should be added after the mention to daily and monthly in the caption. The authors mention the existence of white shaded lines. I have only been able to see them after using a 300% zoom when reading the PDF file. I suggest them to make the lines thicker.

Figure 7. Similar comments to the ones above.

Figure 9. The caption should mention which panel corresponds to each site. It is very hard to understand this figure as currently

I am not a native English speaker, but, still, I would recommend, in my humble opinion:

- * p 821, line 20. I find "also" quite strange in this context.
- * p 832, line 7. "analog is chosen" should read "analogs are chosen" (if they use all of them, which I do not see clear from the text)
- * p 833, line 12. "fields from RCAO is used" should read "fields from RCAO are used"
- * p 833, line 14. "of non-normal distributed" should read "of non-normally distributed"
- * p 833, lines 14 and 23. After the references to non-normal variables, I think they should be enumerated (precipitation and wind speed??)
- * p 835, line 11. "smaller archive" should read "smaller archives" (several archives)
- * p 842, line 27, "lower frequent variations" should read "lower frequency variations"
- * p 846, line 16, "large bins . . . in the wind rose makes .." should read "large bins . . . in the wind rose make .."
- * p 848, line 25, "analoga" should read "analogs"

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- * p 849, line 21 "principle" should read "principal"
- * p 851, line 20. I find hard the word "Also" in the beginning of a sentence.
- * p 853, line 24. "low frequent" should be "low frequency"

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

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