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Interactive comment on "The regional MiKlip decadal forecast ensemble for Europe" *by* S. Mieruch et al.

Anonymous Referee #1

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This paper describes one of the first attempts to my knowledge to dynamically downscale decadal climate predictions using a regional climate model, and the results it contains are clearly and appealingly presented. However, the absence of any comparisons with the coarser-resolution global simulations used to drive the regional model or attempt to attribute the sources of the biases and trend errors shown in the figures are crucial omissions in my opinion, as addressed under item (1) below.

Main issues:

1) While this paper commendably breaks new ground by being among the first to describe dynamically downscaled decadal forecasts, nearly all readers will want to know how the downscaled regional forecasts compare to the set of global MPI-ESM-LR based forecasts, described in Müller et al. (2012) that are used to drive COSMO-CLM

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(CCLM). It should be straightforward to apply the same analysis to the MPI-ESM-LR simulations that has been applied to CCLM, and to show or at least describe the comparative results, and it seems strange that this not been done. (Such comparisons including assessment of "added value" are standard and essential in analyses of regional climate model simulations as for example in Berg et al. 2013 which describes downscaling of CMIP-type historical simulations, and while d and e below are noted as “open questions” in the concluding paragraph here, that does not justify in my opinion the authors not performing at a minimum the straightforward comparisons a-d.)

Questions of this nature that should be addressed include a) To what extent is the predominantly cold bias in Figs. 3 and S1 (upper right) attributable to biases in MPI-ESM-LR vs biases in CCLM? (Compare with MPI-ESM-LR and ERA40-forced CCLM biases.) b) To what extent are the weak CCLM trends (Figs. 3 and S1 second row) attributable to inaccurate trends in MPI-ESM-LR vs inaccuracies originating in CCLM? (Compare with MPI-ESM-LR and ERA40-forced CCLM trends.) c) Supposing the too-weak trends originate in CCLM, could this be due to CCLM not including anthropogenic forcing changes? (Assuming the latter to be true, please indicate.) d) How do the CCLM skills presented compare to those obtained from the set of MPI-ESM-LR forecasts used to drive CCLM? e) Please say something about the value added by CCLM to the MPI-ESM-LR forecasts. f) Likewise for precipitation In such a discussion, Berg et al. (2013) can be referenced as appropriate.

2) It’s stated on p. 5716 lines 20-22 that, aside from the moving average described in Table 1, the data pre-processing consists simply of subtracting the long term (1961-2010) means and trends. What is not done, apparently, is to take into account model drift, which can cause the forecast mean values and trends for the set of forecasts to be lead-time dependent, even when anomaly initialization is employed, and is accounted for by defining anomalies with respect to the climatology for a given lead time as described for example in Müller et al. (2012). If the authors have in fact taken that step then this should be made clearer, and if they have not then that choice should be more

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thoroughly justified.

3) At the top of p. 5717 it's claimed that the signal that remains after applying a 9 yr moving average as in Fig. 1 is the "potentially predictable signal". That this is not necessarily true is easy to see: consider a case where the original time series of annual values is white noise, which by nature has zero predictability. A 9 yr moving average will have non-zero values, much as in Fig. 1, but that does not imply predictability in any meaningful sense. To detect "true" predictability well-known statistical tests involving lagged autocorrelations, etc. can be applied as discussed for example in sections 5 of Boer, Clim. Dyn. 2004, 29-44. (Granted that Fig. 1 and the accompanying discussion is intended simply as an illustration, but the identification of the filtered values as a predictable signal should contain appropriate qualifications nonetheless.)

Minor comments:

p. 5712 line 6: This sentence gives one the impression that the primary focus of MiKlip is specifically prediction on time scales of "decades" (evidently meaning 20-30 years minimum), whereas there is little evidence at present that useful predictions of natural climate variability on such time scales can be made. On the other hand, fona-miklip.de uses terminology such as "time frames of years to decades" and decadal (rather than multi-decadal) predictions. Therefore I would recommend changing "time scales of decades" to "time scales of years to decades" or "time scales of years to a decade or longer".

p. 5712 line 25: Should inform the reader here how projections differ from predictions, e.g. "as opposed to projections, which do not take into account the influence of initial conditions".

p. 5713 lines 9-10: The authors seem to be asserting here that decadal predictions based on statistical methods cannot be useful. This sounds overstated, but if that is indeed what the authors mean then they need to provide one or more supporting references.

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p. 5713 line 11: Off hand I am not aware of any evidence for multi-year predictability specific to soil. On the other hand, there is some such evidence for sea ice (Blanchard-Wrigglesworth et al., GRL 2011). Therefore, suggest rewording e.g. to "i.e. the oceans and possibly sea ice and soil" perhaps including also "slow phenomena in the atmosphere such as the quasi-biennial oscillation" to be in accordance with the subsequent discussion.

p. 5715 line 24: Should add that the 5 starting dates are separated by 10 year intervals

p. 5715 lines 25-28: Please describe here how the reanalysis forcing is used, e.g. is it applied only at the boundaries of the regional domain, or in the interior of the domain as well? What spatial and temporal scales are constrained by the forcing, and how and on what variables are the constraints applied?

p. 5717 line 2: Can the authors say a priori that the high frequency fluctuations are unpredictable, without having performed a quantitative analysis? Suggest changing "cannot be predicted" to "are unlikely to be predictable".

p. 5717 line 20: Suggest changing "This following table (Table 1)..." to simply "Table 1..."

p. 5719 eq (3): there should be some indication here or elsewhere of what are typical values of N_eff

p. 5722 line 20: The predictions are of temporal means, not tendencies. Suggest removing the words "the tendency on"

p. 5722 line 25: "unsatisfactory" is something of a human judgement, suggest replacing with "unskillful" (a forecast without skill could be correct and presumably satisfactory if there is no potential predictability)

p. 5723 line 26: should revise to "smaller trends in northern Europe (excluding Scandinavia)"

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p. 5729 line 11: potential predictability is usually considered to be a mathematical property of observed or forecast time series, including elsewhere in the paper, e.g. Fig. 1. Therefore should replace "potential predictability" here with e.g. "predictive skill" as on p. 5730 line 8.

p. 5731: Does "a.o." mean something in English?

Fig. 2: the correlation values are so small that they are difficult to read

Fig. 2: in addition to correlation values include also N_eff or p values?

Fig. 4: the stippling is very difficult to see; one suggestion would be to include a high resolution version of the warm season temperature results, comparable to current Figs. S1-S3, as a new Fig. S1 in the supplementary material (renumbering the current figures to S2-S4).

Interactive comment on Geosci. Model Dev. Discuss., 6, 5711, 2013.