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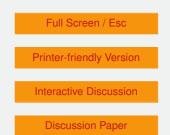# *Interactive comment on* "The regional MiKlip decadal forecast ensemble for Europe" *by* S. Mieruch et al.

Anonymous Referee #2

Received and published: 28 January 2014

Review of "The regional MiKlip decadal forecast ensemble for Europe" by S. Mieruch et al.

This study describes the regional hindcast ensemble for Europe generated in the framework of the MiKlip program. As underlined in the introduction, the question of the decadal predictability on regional scales remains very open: skills, added value of initialization, relevant variables, space and timescales which are addressable, mechanisms: many issues are on the table. In this sense, the topic of the paper is very promising and such studies are needed in the community and should be encouraged.

However, the way it is presented here is very disappointing and insufficient. Technically first:

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- No details are given regarding the downscaling method, hypothesis, results. After reading several times the experimental design, it remains very obscure to me how the outputs from the MPI-ESM decadal system were used to construct the regional forecast system. Which fields were exactly used for the forcing? How are the lateral boundary conditions dealt with?

- It is not even clear which dates were chosen (p. 5715 l. 22). I can only guess this from Fig. 2. (or wait p. 5722). This example reflects the lousiness of the description of the experimental protocol. See other questions below in minor points.

- Two regional models are announced in the introduction and in the experimental design, but the REMO is never discussed again in the rest of the paper. I am not even sure what was the aim of the authors in using two models. Was the REMO model used at all? Where?

After an interesting section on metrics, the results are then presented as a catalogue of statistics. No interpretation is ever given, either in terms of regional climate dynamics (why does the filtering have no effect over the Iberian Peninsula, Italy and the Balkan? I would guess it has something to do with the different dynamics over northern and southern Europe? Is there really no explanation?) nor with respect to the performance of the full MPI-ESM decadal system over similar regions. From my understanding, predictability of the RCM is expected only if and where there is predictability already in the initial system. Is that true? I would expect at least a discussion of this point. Finally, significance (which are a crucial point for the evaluations of decadal predictions and signals are often very weak) is not convincingly addressed (see below).

Because of these weaknesses which prevent a proper interpretation and any learning from the results presented, I have to recommend a rejection of the paper. Nevertheless, as I underlined above, the topic is of course very promising, and the experiments most certainly valuable. Therefore, I strongly encourage the authors to revise the work by addressing, at least, the points above and resubmit.

I add below some minor points which the authors might want to consider:

P5713 l 13 (or line 20) initialization: consider citing the more recent Garcia Serrano et al 2012 GRL for example rather? Or Doblas Reyes 2013 Nature Communications

L14 who -> which

L17 so therefore : this is not English

P 5714 – l.14: "1 day time lagged initialization for the atmosphere?" I don't understand this statement. I have the impression there is a confusion between what is usually called "initialization" and "perturbation" in forecasting systems. Please clarify.

P5715 l 25: How is the transition between ERA 40 and ERA interim treated?

P5716 l 4: I don't understand why "this can be regarded as anomaly initialization". And again, I don't see how this relates to the MPI-ESM decadal system.

p. 5716 section 3: What procedure is used for the detrend?? Linear? Is it applied grid point wise?? What are the justifications?

P 5717 l 3: "these high frequency fluctuations cannot be predicted using decadal model initializations": this is part of what we would like to be shown here. Or else, give a reference (do you refer to works from Boer et al for example?)

P5717 l 21: add "spatial"?

P5718 l 20 bias adjustment: this has not be discussed before: how is the bias removed? (and which bias?)

P5718 l 21 What do you mean "the interpretation of the value of the correlation coefficient is very individual"?? You mean it depends on the scientist himself? But there are some statistical tools (significance evaluation) to diminish as much as possible this subjectivity, isn't it?

P5718 l 25: "common interpretation" of the significance level? Common for who, for

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what? Please check a statistics textbook.

P 5720 l 14: How accurate is it to average p values? Please give a reference. Fig 4 legend. "here": this is not proper language for a figure legend.

p. 5723 l 21: it would also probably change the atmospheric circulation etc. I find this statement quite speculative.

On the use of moving average of 9 yrs: given that the hindcasts are 9 years long, I don't really understand how you do that.

P5724 l 15-20 "but simultaneously smears out the resolution": isn't that expected? Rephrase this passage. + check repetition with data/methods.

4.1.3: fidelity. Is this result surprising? Are their models in which it is NOT "fidel". How it is in the decadal system for Europe averages for example? Please discuss.

Section 5 P 5729 l 12: "predictability increases with low pass filtering". I have the impression this sentence is not fully consistent with the end of section 4.1.4, am I wrong? Here, your interpretation of predictability seems to rely only on the correlation. However, as you explained it in section 3, "predictability" is rather the result of all (or some) of metrics developed above, no?

Section 6: I disagree that this study has made "a large step in understanding" of regional predictability. In terms of feasibility, yes, perhaps, if the experiments were more clearly and precisely described.

P 571 l 21: should come much before.