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Climate of the Past Discussions

Interactive comment on "A regional climate palaeosimulation for Europe in the period 1501–1990 – Part II: Comparison with gridded reconstructions" *by* J. J. Gómez-Navarro et al.

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"This paper describes an ambitious and skilful attempt to do something almost impossible. While I admired the project, I don't think the results here are presented clearly enough to justify publishing as-is."

We are very thankful to Dr. Brohan for carefully reading the manuscript and making very useful comments. We hope that the implementation of the changes suggested by all reviewers and a general clarification of the results of this manuscript will improve it and justify its publication.

"European regional climate has a large influence from unforced, natural variability. Over

most of the period 1501-1990 the external forcings on that climate were modest. So even if we had perfect knowledge of how the true climate had behaved, and a perfect GCM, we'd expect substantial differences between simulations and observations. In reality we have large uncertainties and important structural limitations in all three of the external forcings, the models used, and the reconstructions; I'd expect the agreement between simulations and reconstructions to be very poor - and it is."

We certainly agree with this argument. Still, there are indications (Gómez-Navarro, 2012) that the response of temperature is not so tightly driven by internal variability, and hence certain degree of agreement in this variable is expected a priori. We test here if this is the case. The fact that we find very little agreement is in itself result that points to an inconsistency that has to be solved either by attributing errors in the reconstructions of the forcings, in the variables, or in the response of the model to such forcings.

"To attack a very difficult, though important, problem is admirable, but it means that the processes used are likely to be messy and experimental, and the prospect of clear and strong conclusions is remote. This paper has exactly these problems - it is difficult to justify on the grounds of its valuable new conclusions - the uncertainties are such that the conclusions are limited, and the differences between simulations and reconstructions are so large that it's hard to justify any comparison methodology as optimal."

"So I liked the project, but why do we need this paper? The (admirable) work of setting up and running the simulations has already been described in part 1. To justify part 2 needs not just a 'Comparison with gridded reconstructions', but something more specific: something new and interesting, and only learnable from the long, high-resolution, regional simulation. This paper needs to be rewritten to highlight its new results, not just describe the work that has been done. (It would obviously also be OK to leave out comparisons which didn't show any new results)."

We consider that the added value of this paper relies on having put two state-of-the-art

datasets (note that high-resolution simulations for Europe for the last centuries were not available so far) at the same level and having carried out a critical comparison between them. Even if the results fit in the standard knowledge and do not produce counter-intuitive results, evaluating climate variability with new (and in principle more reliable) datasets is itself a piece of valuable information.

Still, we acknowledge that perhaps the tone of the paper has to be revised in order to emphasise the results, and make it more focused towards the implication and discussion of the results, not just a plain description of the comparison. This certainly applies to the last point brought up by the reviewer, namely the choice of a more ambitious title.

1) "The point of this analysis is that it uses a high-resolution regional model, not just the GCM that has been looked at before, so what it needs to highlight is where the RCM is making an important difference, especially where it shows signs of being usefully better. I didn't get a good general picture of this: In figure 2, for example are the timeseries from the GCM (not shown) better than those from the RCM, worse, where do they differ most interestingly. Same point applies to the EOF and CCA analysis."

We will try to emphasise once again the added value of the RCM compared to the GCM. Note however that this is the main topic of Part 1 paper, so we would like to avoid excessive overlap with the former publication.

2) "The paper identifies some areas where the reconstructions and simulations are notably different (1740s, maunder and dalton minima) - is it not worth looking at these periods in regional detail?"

Certainly it is worth. We will include figures and discussion focusing in the spatial structure of the anomalies, not just the spatial-averaged temporal series.

3) "I found figures 2 and 3 very difficult to use. They are very small, I'd rather have fewer panels and more figures, even if that means that some get relegated to the supplementary material. Also, could they have the model and reconstruction in the

same panel, in different colours, and perhaps the mean difference (in 1990) could be presented separately (on a map) and the time-series adjusted to be the same in that year - so the differences in the time-evolution was most obvious"

We will try to accommodate these comments to improve the figures, together to the comments raised by the anonymous reviewer regarding the same figures.

4) "'The simulated climate is a physically consistent dataset', The reconstructions have 'a lack of dynamic consistency'. Is this a new result - doesn't it follow necessary from their construction methods (more than from this comparison)?"

We will re-phrase this conclusion, because we believe the reviewer might have misunderstood our argument. Reconstructions are based on proxy indicators that are used as input for statistical models. Hence, each reconstruction is consistent with the proxies used as input and the data used to calibrate the statistical model. However, independent reconstructions that use different input data do not have to be necessarily consistent, although they should be if they were perfect. The fact that we identify an inconsistency between the SLP reconstructions and the SAT or precipitation reconstructions is not trivial, and indeed is an important result that indicates that they likely contain errors. Unfortunately, we can only find the trace of this error, but we can not identify which of the two reconstructions (if not both) is the origin of this mismatch.

5) "Understatment is traditional, but I thought that 'Comparison with gridded reconstructions' is too boring. The title is an important advertisement for the paper. If possible, get the main conclusion from the comparison into both the title and the first line of the abstract."

We agree. We will try to find a more attractive title for the paper.

Reference

Gómez-Navarro, J. J., Montávez, J. P., Jiménez-Guerrero, P., Jerez, S., Lorente-Plazas, R., González-Rouco, J. F., and Zorita, E.: *Internal and external variability in regional*

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