Clim. Past Discuss., 11, C678–C680, 2015
www.clim-past-discuss.net/11/C678/2015/

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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Received and published: 12 June 2015

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.

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.

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).

Specific points:

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.

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?

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.

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)?

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.

Interactive comment on Clim. Past Discuss., 11, 307, 2015.