

Interactive comment on “Tree-ring proxy based temperature reconstructions and climate model simulations: cross-comparison at the Pyrenees” by I. Dorado Liñán et al.

Anonymous Referee #3

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Tree-ring proxy based temperature reconstructions and climate model simulations: cross-comparison at the Pyrenees

By I. Dorado Liñán et al.

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Minor revisions suggested.

This paper presents a regional May–September temperature reconstruction to the year 1260AD for the Pyrenees. The reconstruction was developed from a series of tree-ring chronologies. The authors employ a suite of different techniques in order to establish

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uncertainty surrounding the methodologies used to generate the reconstruction. The reconstruction is compared with climate model data to establish the possible role of external forcing, including solar, volcanic and anthropogenic.

MAJOR COMMENTS: The value of this paper is in its presentation of multiple sources of methodological and site selection uncertainty (through the use of nests) and from its comparison with a regional scale mode. Overall, the authors have produced a paper that is sound and presents interesting results. I do think it is a touch too long and this could probably be resolved by tightening the language in the discussion, which is longer than it needs to be.

I have three main issues with the paper that I feel need to be resolved before further publishing.

1. The first is the way the authors present the uncertainties from the reconstructions developed using differing methodologies. The authors tend to discuss each reconstruction individually, where they differ etc. and the ranges of values at certain times. Perhaps it would be useful to come up with some sort of confidence interval from what is effectively an ensemble of reconstructions? This would provide an easily communicable temperature estimate for the reader.

2. In the results section I found there were several times when the authors stated that “X had a strong correlation with Y”, or “The correlations of X and Y were weak” but there was no quantification. I understand much of these values are in Table 1, but when discussed in the text it would be helpful to include a number so the reader knows what a “high” or “low” correlation is to the author (people often have very different notions of what is strong or weak for correlations).

3. The third is in the interpretation of the inconsistencies between the climate model simulations and the reconstruction. At present, I get the feeling that the authors perhaps don't grasp all the nuances of climate modeling (which is understandable if from a paleoclimate background). There is evidence of this throughout the paper but is most

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evident in the last part of the discussion (pages 3940 and 3941). Here, the authors discuss discrepancies between cooling and warming between the reconstruction and the model simulations during the 20th century. They go on to describe these discrepancies as potential caused by model deficiencies such as aerosol forcing and land use change. While these may be contributors, I think the authors give these reasons too much weight. I think other candidates for discrepancies are much more likely and as such I think much of the discussion from line 5–20 on page 3941 could be removed. I don't feel the authors recognize that climate models cannot reproduce observed year-to-year or decade-to-decade climate variations that are due to internal climate variability, which can dominate a trend – potentially even on the multi-decadal scale with external forcing. So, a climate model without external forcing could conceivably have a close to zero correlation with reality on shorter time scales (e.g. decadal). I think these differences in internal variability in the model versus reality are far more likely to be causing the discrepancies. You actually discuss this fact on page 3940, para 3, quite correctly, but you then finish with “Therefore, this particular warm episode may not necessarily appear in the simulations due to the possible shortcomings of models in regard to model internal climate dynamics”. This internal climate dynamics of the model is not a ‘shortcoming’ – it's how the model is designed to work. However, I think my above comments simply require a little re-writing surrounding the comparison between model simulations and the reconstruction and don't constitute a major change to the manuscript.

MINOR COMMENTS:

Page 3921, line 2: Please change “for the last Millennium” to “from 1260” – your reconstruction doesn't quite span the full millennium and it's better to be precise.

Page 3922, line 3: Change to “mechanisms”

Page 3923, line 14: What do you mean by “characteristic of the model” do you mean its parameterizations? Biases? Please be more explicit about what you mean here.

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Page 3923, line 16: Again, what do you mean by “main characteristics”? Circulation features? Climatic features? Please be more specific.

Page 3923, line 20-21: Change to “allows for simulation at higher resolution.” And “. . .millennium have recently become available for specific regions.”

Page 3923, line 22: Insert “data” between “This allows” (otherwise, not clear what “this” is)

Page 3923, line 24-25: Please talk about other benefits, e.g. more accurate convection schemes, better parameterization of cloud microphysics, better handles topography etc. etc.

Page 3925, line 3-4: You state an aim is to provide “an evaluation of the uncertainties related to the methodological variants employed”. As stated in the major comments above, is there a better way to consolidate these uncertainties from your ensemble of reconstructions? Perhaps create a confidence interval or similar?

Page 3930, line 23: See major comment #2, what is a “high correlation”? Please include a number in the text.

Page 3931, line 2: Change to “. . .methods do not have a significant effect. . .”

Page 3932, line 8: What is a “high” RE? Anything over 0.0? Please give a value.

Page 3932, line 12-13: Change to “using scaling rather than regression”

Page 3934, line 2: Change to “better simulate”

Page 3934, line 26: Avoid using the word “tiny” – it's a little informal. Change “very tiny ones” to “small”

Page 3935, line 5: Change “tiniest” to “smallest”

Page 3935, line 6: Change to “changes in the amplitude”

Page 3935, line 21-21: Change to “. . .local influences while also preserving as much

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climate information. . .”

Page 3936, line 12: Change “have” to “has”

Page 3937, line 8: Change “of” to “from”

Page 3938, line 11: Avoid exaggerating language – remove “extremely”. Rigorous is rigorous.

Page 3940, lines 21-23: The sentence “Therefore, this particular warm episode may not necessarily appear in the simulations due to the possible shortcomings of models in regard to model internal climate dynamics” is not correct – see major comment. The remainder of the discussion associated with model comparison needs to be thought through a little more and some rewritten or removed.

Page 3941, lines 2-20: Given all the other shortcomings in models and the influence of internal dynamics (see major comments section), I think that although land use change might be a contributing factor it is probably only a small contributor (as has been shown in other regions). As such, I don’t think it warrants such a lengthy discussion and I think much of this paragraph can be removed from the manuscript. I would simply mention it but not go into detail. As the discussion is already too long, this would be a good part to strip out.

Page 3941, line 29: Change “wider” to “larger”.

Page 3942, line 13: The word “odd” seems like an odd choice – please use a different word here.

Page 3943, line 7: Add to the end of the sentence that larger variability at the regional scale is what is seen in reality – this is what you expect (which is true, at the regional scale you should get more noise and less forcing signal).

Page 3943, line 14-16: Again, I think you’re focusing on aerosols and vegetation change while ignoring other potentially very important model shortcomings and the

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role of internal model variability.

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