Clim. Past Discuss., 11, C4–C8, 2015
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Climate of the Past Discussions
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Interactive comment on "New insights into the reconstructed temperature in Portugal over the last 400 years" *by* J. A. Santos et al.

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Received and published: 8 February 2015

This manuscript surely is a valuable contribution to research in paleoclimate and fits well into the scope of CPD. It is generally well written, and the figures meet the common quality standard, though some could be improved.

In this study the authors seek to show the consistence of Portuguese paleoclimate data from four different sources, (1) the Europe-wide annual reconstruction of Luterbacher et al. (2004); (2) local repeated borehole temperature observation from one site in Portugal; (3) paleoclimate simulation and their signature in these boreholes; and (4) precipitation indices from documentary sources since the late Maunder minimum. Noting that the (1) is not consistent with the other results, they propose a new reconstruction based on a two-stage calibration procedure using information from these sources, and

compare the obtained results with (4). This is an interesting approach which should be discussed in the community, though I think this manuscript should only published after major revisions.

Particular Comments:

1) Borehole data. Though the authors refer the reader to earlier papers, I think that a bit more information would be useful. This concerns mainly the estimation of the geothermal gradient (or heat flow density). The authors correctly mention that their results can only be preliminary, given the small depth of the borehole. However, it would be interesting if the authors could include a discussion of this problem. For example, a plot of HFD(z) could be helpful, in order to see whether we are reaching more or less constant values in the estimation interval. In Fig 1 there is obviously a change in thermal conductivity at about 180 m depth (why not use the interval 140-180 m?). A HFD plot could possibly better justify the assumption that the background heat flow can be approximated linearly. Surely the estimated linear profile is not an steady-state geotherm (as stated in the caption of Fig. 1), but may contain the signature of older events back to the last glacial cycle (e.g., Rath et al.2013). In addition, it would give a more direct view of the errors. It would be nice to see a graphic showing how this estimation error translates to the whole profile. Maybe a Monte Carlo study? Clearly, getting a bit more quantitative here would strengthen the study considerably.

(2) Inversions and forward models. The authors state that "...uncertainties inherent to these inversion methods (Hartmann and Rath, 2005) are avoided in the present study". I do not agree. Most of the problems mentioned in the article cites are of a physical character. If you lack reliable estimates for the subsurface properties, they also will render the forward model (forced by some simulation or reconstruction) unreliable. I have already mentioned the problem of background heat flow density above. Any model or parametrization error will be present in both approaches and thus can not be simply avoided. The only additional problems here are the procedures related to the solution of the ill-posed inverse problem, e.g. the truncation or damping of the SVD-derived

generalized inverse. However, comparable procedures are also implicitly or explicitly ingredient of many (non)linear regression codes, or, for example, in SSA procedures of different flavors. This is of course not central to this study, but if mentioned at all, it should be discussed in a fair manner.

(3) Regression approach. I found the part on the two phase calibration rather difficult to understand, and thus think that it should be expanded and improved. In particular, It should be made clearer, which assumptions have to be made, and that - as I understand it - the result is a recombination of long period information from the simulations and shorter period information from the instrumental period and the Luterbacher reconstruction. This may be meaningful, but deserves more discussion. Personally I would not call this process a calibration ("adapting uncertain parameters in order to increase agreement of models with available observations"), but a method of reconstruction. From the description of the method at the end of section 2.3 it is not fully clear to me why it "can be used to correct discrepancies between long-term trends of reconstructed and simulated temperature series". One could conclude from this study that the Luterbacher reconstruction is not "valid" in this area in the light of the borehole temperatures (and rogation ceremonies) and simulations based on best current knowledge on climate physics. What observational data are relevant for the Luterbacher reconstruction in this area? Or, more general, why does the reconstruction not capture the trends? These would be the obvious questions following this study. A discussion of these problems could improve the manuscript considerably.

Minor items:

General: too often "not shown" - better refer to other publications, or reformulate.

P4, L5: Possibly the reference is wrong - no boreholes mentioned. Christian, H. J., Blakeslee, R. J., Boccippio, D. J., Boeck, W. L., Buechler, D. E., Driscoll, K. T., Goodman, S. J., Hall, J. M., Koshak, W. J., Mach, D. M., and Stewart, M. F.: Global frequency and distribution of lightning as observed from space by the Optical Transient Detector,

J. Geophys. Res.-Atmos., 108, 4005, doi:10.1029/2002JD002347, 2003.

P9, L7ff: Please explain shortly why a difference of 2 m may explain the results. Difference in what? Smallest observation depth? What happened to the annual temperature wave, which is dominant in boreholes down to 15 -20 m?

P9 L15ff: Which ensemble? Never mentioned before. I think It should be mentioned in the section describing the simulations.

P10, line 1: Observations can support simulations, but how can simulations support observations? This is also relevant to the formulation on P12, L15.

P10 L9: what is a "2-order SSA filtering"? Reformulate or explain.

P4, L5 P13, L5 P10 L26: "cross-validation" and "2-way validation" are misleading here.

P12, L26: The absence of the trend is not unlikely, but a fact. The absence "in reality" is unlikely.

Figures:

General: It would help to have at least one sentence in the captions which tells us what to look for. This is a matter of taste, of course, because this may lead to redundancy with respect to the text.

Fig 1: I suggest showing also the estimated background gradient in the top panel, perhaps the reduced temperatures the regression equations in the figure should be in physical units - T and z. As already mentioned I suggest to complement this figure with a HFD(z) plot, but this of course depends on how the authors choose to revise their text.

Fig 2: OK if large enough in the final text.

Fig 3: I guess this is a "robust" regression? Otherwise I would have expected a larger influence of the high leverage points (at the very low & high CalT values), which could be classified as outliers. Also, the distribution of the residuals is clearly not Gaussian. You can see different behavior at CalT < 0.7 C and above. What does this mean with respect to the statistics? Is this significant?

Fig 4: I suggest to improve or leave out the (c) panels: they should be at the same horizontal scale as the others. I do not find the white "cone of influence". Maybe this refers to another version of the plot? One might also argue whether this panel is necessary, as it does not contain much information. "panels" should be "panel".

Fig 5: I do not see "0 indices" - also "black edges" instead of "outer lines"?

Interactive comment on Clim. Past Discuss., 11, 1, 2015.