

Interactive comment on “Statistical framework for evaluation of climate model simulations by use of climate proxy data from the last millennium – Part 3: Practical considerations, relaxed assumptions, and using tree-ring data to address the amplitude of solar forcing” by A. Moberg et al.

Anonymous Referee #2

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This paper builds on previous work by the authors in the development of a new set of test statistics for data-model inter-comparison. It is the third in a series of mainly theoretical papers and deals more with the practicality of the implementation of this method, with the addition of two new extensions: the first is to account for auto-correlation in the time series and the second to allow for the direct comparison of two differently forced scenarios. Both of these are very useful, and offer a clear improvement of the method. The paper does a good job of explaining the general outline of the method, resulting in

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a comprehensible paper which contains all the essential details from previous papers so a reader can gain a good understanding without the continual need to refer back to earlier work. The main bulk of the theoretical advancements is contained in the appendices which allow the rest of the paper to focus on the application of the method and the choices required in its implementation. This is handled in a clear and comprehensive manner resulting in an interesting paper with clear and useful figures, which I think should be published after first making some changes.

I feel that the discussion section which describes the implications of the results is slightly lacking. For example I would be interested in seeing more discussion of the difference between these results and those obtained by Hind & Moberg 2013, which suggests that a weaker role for solar forcing is more likely. Is the difference likely due to the change in statistical method which allows for a decrease in the time unit or from the difference in reconstructions used? Have you carried out this new analysis with a 30-year time unit?

The model used in this paper is a low top model without interactive ozone and with the solar variation only modulating TSI it is therefore possible that it may be lacking possible dynamical responses (see eg Gray et al 2010). Given that this paper looks at different regions in the world it is therefore possible that the regional results reflect more the deficiencies of the model not necessarily the strength in forcing. This should be discussed.

It is also worth noting that these model simulations have an interactive carbon cycle and not prescribed CO₂. Given that this leads to a discrepancy in CO₂ concentration during the LIA (see fig 6 Jungclaus et al 2010) between the different ensembles and also the observed CO₂ concentrations is it possible that this is potentially biasing your results slightly in favour of the high solar forcing?

My other major worry is contained in the results in figure 8. Earlier in the paper it was suggested that without care the difference in the two averaging methods (“inside” and

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“outside”) could lead to possible biases in the “inside” method (p2634). Looking at this figure it appears that the inside method gives systematically larger (more positive) values than the outside values. This difference can have an impact on the conclusions. Indeed if only the outside values were used I would find it hard to say which forcing was more likely. I therefore feel that this detail needs to be addressed.

Other comments:

Abstract line 7: “But we argue to study” should be changed

Introduction and elsewhere: Give that there are several citations to Hegerl et al papers, and one to Schurer et al 2013 who have all used a detection and attribution framework to look at this period, some mention should also (or instead) be made to Schurer et al 2014 who specifically looked at the question of trying to constrain solar variability and would therefore seem more relevant to this particular study

Introduction line 16.: ” So far, the available methods can however not account for the full complexity of the situation.” It would be useful to expand this a bit to say why.

P2644, line 13. Might be worth mentioning that the fourth option is similar to optimisation in other detection and attribution studies (see e.g. Allen and Tett 1999).

Appendix p. 2654 line 12 As a^* hypothesis model

Appendix p. 2654 line 18 μ is not defined here nor in the set of equations in section B

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