Clim. Past Discuss., 10, 2627-2683, 2014
www.clim-past-discuss.net/10/2627/2014/
doi:10.5194/cpd-10-2627-2014

Answers to Referee #2

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?

As we have explained in our main overarching answer, we have discovered that the so-called inside averaging method, described in Appendix A of SUN12, needs a modification. When we applied this modification, we found that the new results are in better agreement with the findings obtained by Hind & Moberg 2013; in particular suggesting that a weaker role for solar forcing is more likely. This holds for all four time units (3, 5, 8 and 12 years) when the inside averaging method is applied, although the U_T values for regionally combined data in a corrected version of Figure 8 only exceed the 5% significance threshold when using the 3-year unit. We have now also calculated statistics for the 20-year (as in Hind & Moberg) and 30-year time units and this provides results similar to those for 5, 8 and 12 years. We will base our revised discussion on the results obtained with the correct implementation of the inside averaging method.

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.

We agree, this aspect can be added to the final discussion.

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?

We can add a short discussion on what importance the interactive carbon cycle can possibly have on our results. However, with an correct implementation of the inside averaging method the results are no longer 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 "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 that 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.

It follows from our first answer above that this result will change if the "inside" method is applied correctly. The inside method is more precise (but biased). Provided a forcing effect exists, the

higher precision is likely to generate stronger significances (larger values of the test statistic). We plan to add an extra Appendix in Part 3 to explain more of the differences between inside and outside averaging, both as concerns bias and precision.

Other comments:

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

This will be changed, in line with the suggestion by Referee #1 to reduce the first part of the Abstract.

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

Yes, indeed, it is highly relevant to refer to the findings by Schurer et al 2014 (we assume the Referee means the paper in Nature Geoscience with title: "Small influence of solar variability on climate over the past millennium)". They concluded that "solar forcing probably had a minor effect on Northern Hemisphere climate over the past 1,000 years, while, volcanic eruptions and changes in greenhouse gas concentrations seem to be the most important influence over this period". We can add a comment on these findings at relevant places both in our introduction and our discussion.

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.

One example is the often temporally varying quality and statistical precision of proxy data, which to our knowledge is not explicitly dealt with in the methods mentioned. We can mention this as an example.

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).

Thank you for pointing this out. We are happy to mention this.

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

We will add "a" here.

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

You are right. μ was defined in our Part 1, but we forgot it here. μ is the expected mean value of each sequence. In our calculations, we always subtract the mean value computed over the entire period being analysed, separately for each data sequence. Thus, in practice, we consider only the variations around the long-term mean but this mean value itself does not contribute to our calculated statistical measures. We will make this clear when we revise the paper.

Stockholm, November 2, 2014
Anders Moberg, Rolf Sundberg, Håkan Grudd, Alistair Hind