

## ***Interactive comment on “Do GCM’s predict the climate... or macroweather?” by S. Lovejoy et al.***

**S. Lovejoy et al.**

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Betts: 1. I’m not sure why it is useful to compare either a control (unforced) run or pre-1900 forced run with observational / reanalysis data for the 20th Century (figures 3, 4 and 6), since we know the net forcing increased substantially over that time. This is therefore not a fair comparison of the models (without anthropogenic forcing) with the real world (with anthropogenic forcing). I suggest that this comparison be done against models run over the same period of the observations / reanalysis, with all forcings included.

Response: The aim of the paper was to focus on natural variability and to avoid getting into the complexities of rapidly increasing 20thC anthropogenic forcings. The problem is that if we stick to the pre-1900 period then the instrumental series are too short (the common period of the NASA-GISS, NOAA NCDG, HadCRUT3 series was 1880-2008,

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so that we would be left with only 20 years of data). Therefore, the observational and reanalysis data were included. However, in order to partially answer your question, we have performed the analysis you suggested with the GISS-E model (used in fig. 7), and have included the results in the attachment.

The main points to note from this graph are:

- a) The different models are almost identical to each over this period (presumably CO<sub>2</sub> effects are much more important than solar, volcanic effects),
- b) At scales of  $\approx 100$  years, the models are about twice as variable as the data. At these scales the variability is dominated by the global warming signal so that presumably it isn't right to attribute too much of this to natural variability.

The overall conclusion is that in the recent period the models (1880-2005) are bit too sensitive to CO<sub>2</sub>, but in the pre-1900 period they are missing a little natural variability. Overall they do quite well, but the discrepancies are detectable. We are currently using the same type of fluctuation analysis to investigate various historical 1850- present runs and data but at the moment this is work in progress.

Betts: 2. I think there is also a difficulty in comparing the multi-proxy paleo data against models arising from the fact that proxy data is from a number of discrete points whereas the model gives complete spatial coverage. Although a variety of different proxies are used, in many cases the original source data will be representative of local conditions at the specific site in question. As I understand it, when the proxies are used together to estimate hemispheric means, it is implicitly assumed that the local-scale patterns of variability at individual sites cancel each other out and that that the mean of the individual sites is therefore a good representation of the hemisphere as a whole. This may be a reasonable assumption when the focus is on long time periods, which tended to be the case when the multi-proxy reconstructions were published, but I'd expect it to be more problematic at shorter time periods including those of particular interest here (30+ years). If the spatial coverage of the proxy records is too sparse then I would

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expect this to result in an overestimation of the true hemispheric mean, so the discrepancy between the models and multi-proxy curves may be larger than the discrepancy between the models and the true hemispheric mean. Figures 4 and 5 seem to back this up, as the observed / reanalysis curves (which have better global coverage) turn upwards at smaller values on the x-axis than the multi-proxy curves.

Response: The key point about the low frequency variability can be made independently of the multiproxies: at some time scale, the variability has to start increasing in order to be compatible with glacial/interglacial transitions (and the paleo0data). Fig. 3 shows that the transition scale is quite plausibly close to the scales indicated by the multiproxies. Of course you are right that the multiproxies involve numerous statistical assumptions several of which may (and should) be questioned. Indeed, the multicentennial variability of the three used in the figures are all post 2003: five other pre-2003 multiproxies that we analyzed were found to have much lower multicentennial variability; we interpret this as being the result of various improvements that were introduced. Indeed, our analyses suggest that multiproxies could benefit by incorporating both and spatial and temporal scaling constraints to improve calibrations; but this is work for the future.

Betts: This could be investigated by comparing the multi-proxy curves against obs / reanalysis and models only using information from the points where the proxy studies are relevant. ie: take the model data set of individual model gridpoints which contain the sites of the original proxy studies, and use these to construct a hemispheric mean in the same way that the multi-proxy studies did. The same could be done with instrumental data, although this would be subject to the difficulty in my point (1) above at longer timescales.

Response: The multiproxies are based on principle component analyses. Trying to take them apart and go back to the original data isn't worth the effort. All that needs to be done is to analyze the statistical fluctuation characteristics of the raw paleo data directly (before they are massaged into multiproxies). Indeed, this is an important step

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that should be done before multiproxies are developed! More work for the future!

Minor points: 3. There seems to be inconsistency in the way 20CR is described in different figure captions. In figure 3 it is labelled "Twentieth Century Re-analysis", but in figure 4 it is "instrumental data". Please clarify.

Response: To avoid overburdening the already crowded graphs, we lumped the reanalysis with instrumental data (which by the way also involves various nontrivial assumptions to put it on a grid).

Betts: 4. It's very useful to see the multi-proxy studies with and without 20th Century data. Following my point (1) above, I think that the 20th Century curve should be compared to models and obs / reanalysis separately from the pre-1900 multi-proxy curve (i.e: all in a separate figure). However it would also be useful to show a new figure which only shows the multi-proxy curves with and without the 20th Century data. They should also be shown in different colours to make it clear that they apply to different circumstances and are not just two different versions of the same thing.

Response: Thanks for the suggestions. The new figure supplied with this comment should go some way to answering this.

Betts: 5. Overall the readability of the paper would benefit from clearer figures with less information in each figures. Separating things out as I suggest in (1) and (4) above would help here.

Response: We'll try to reduce the information content of the figures. However one of the interests of the method is precisely that it allows for intensive comparisons between different data types.

Caption fig. 1:

The simulations (top three green: volcano Gao, magenta: volcano Crowley, purple: solar only) are for the northern hemisphere from 1880-2005, the bottom is the whole globe from 1880-2008, it is from the NASA GISS, NOAA NCD, HadCRUT3 surface

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temperatures, the same as in figs. 3, 4, 5. Note that the simulations are the same as in fig. 7 but with a somewhat different correction for long term drift (I thank G. Schmidt for the simulations).

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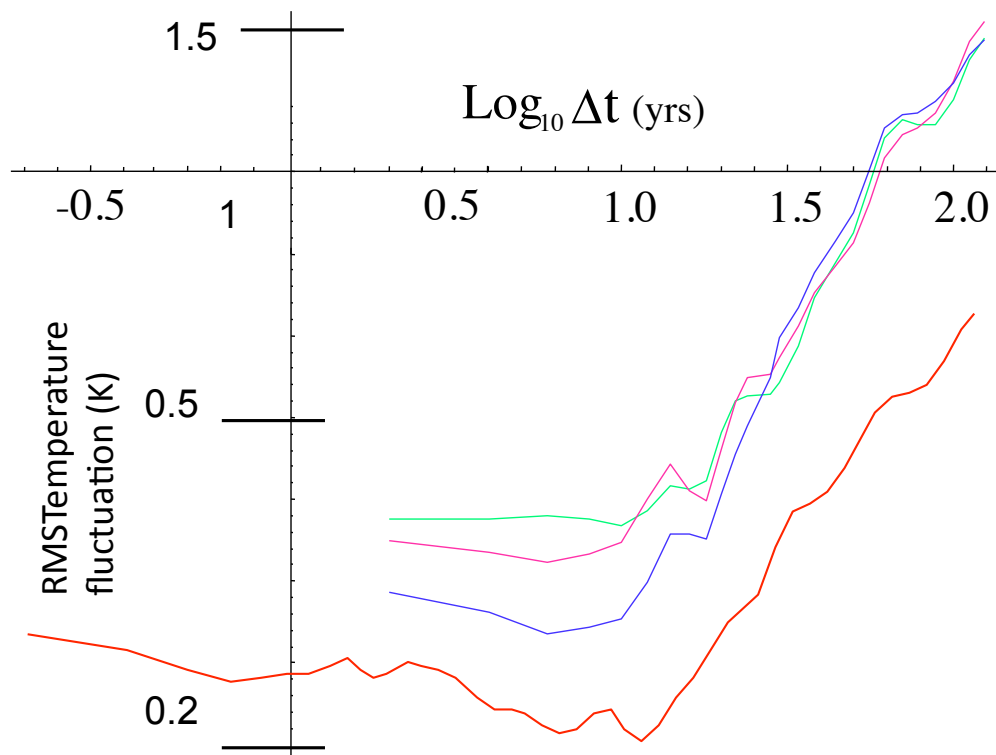
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Fig. 1. Fig. 1

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