

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

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Don’t underestimate the power of scaling fluctuation analysis!

We thank Huber for his reactions about the originality of the paper. Since only two out of eight figures are reproduced from other publications, his claim that “more than half the figures are directly reproduced from those publications” is inaccurate (we examine his other claims at the end), and may stem from a misunderstanding of the original fluctuation analysis method that was used to establish the key results about low GCM multacentennial variability.

Consider the key original figures 4, 5, 6, 7 showing fluctuation/structure function analyses. These graphs show how the RMS fluctuations vary as functions of scale, but their real power is precisely that they enable us to compare the statistical properties of different series, and this systematically as functions of scale. While certain reference

curves (from instrumental, and multiproxy data) recur in each of the figures, this is necessary for judging GCM performance. These figures are statistical analogues of the standard procedure of comparing GCM outputs with empirical estimates - for example of global temperatures. Would it be fair to accuse a paper evaluating a GCM of lack of originality simply because every model output is compared with the same (unoriginal) 20C temperature time series? Nevertheless, Huber opines: “Figure 4 also appears to be nearly identical to a figure in L&S2012d. Many of the remaining figures appear to be replotting data from L&S 2012d with the addition of one or two additional model cases”.

The repeated use of the empirical data as a constant reference is thus the precondition for establishing the original point of the paper i.e. that the forced runs lack low frequency variability – especially multicentennial variability, and that this is likely because they essentially predict macroweather rather than the climate. Indeed the nuanced conclusion of the paper – the degree to which these conclusions are convincing - crucially depends on these detailed comparisons. These comparisons are not only of the actual values of the fluctuations at different scales for the different data and GCM curves but also of their tendencies as functions of scales (i.e. their logarithmic slopes, the exponents). Finally the discussion of the forcings as functions of scale – while only a summary of a referenced paper - are indeed helpful in understanding the observed behaviours.

We believe that Huber will be well rewarded if –rather than dismissing them as unoriginal - he makes the effort to follow the detailed comparisons between the various curves in figs 1, 4, 5, 6, 7.

Minor comments:

a) Huber is disingenuous about “original scholarhip”. He says: “Figure 1 is simply a plot of other people’s data, so that’s not original scholarship.” While it is true that – following the majority of climate authors - we did not actually collect any data for this paper, this does not obviate the need for original (and nontrivial) data processing and analysis

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techniques. Each curve in fig. 1 required a fair bit of data processing. Consider the bottom 1 hour resolution curve. The main problem here was to first remove the diurnal cycle and subharmonics which would otherwise have seriously obscured the basic behavior. Nor was the middle curve taken “off the shelf”. It was necessary to make a preselection of the appropriate data from the vast 20CR data base and then to average it a 20 day resolution, then remove the annual cycle (and subharmonics). Finally, the top curve (Vostok) data had to be interpolated to a regular time resolution. This was only possible after performing statistical checks confirming that the temporal resolution of the raw data (which was variable as the depth increased) was adequate to justify the interpolation and resampling without the introduction of artefacts. Surely this effort – combined with the theoretically appropriate choice of averaging scales - is original enough to warrant publication?

b) The final comment is about the text “echoing” arguments from elsewhere. To the extent that there are indeed echoes – these are essentially confined to a) the introduction (in which echoing is mandatory), b) to section 2 which explains the technique and reviews the little known basic application to establishing the weather- macroweather – climate trichotomy (and without which the heart of the paper would be incomprehensible), and c) the conclusion - which by reviewing echoes - the other literature needed to understand the primary findings.

c) Hubers states: “Figure 2 and 3 are taken directly from L&S 2012d.” Fair enough, this was stated. These could be removed but in our opinion, they are helpful.

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