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Earth System Dynamics Discussions

Interactive comment on "Do GCM's predict the climate... or macroweather?" *by* S. Lovejoy et al.

Anonymous Referee #1

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This paper compares spectra of proxy reconstructions with those of model simulations of the last millennium. The main point is that the spectra of the proxies and model simulations yield large differences at various temporal scales.

In principle, this analysis is interesting and should provide a useful metric for model and data comparison. But the manuscript is extremely unclear and difficult to follow.

1. The manuscript uses a lot of material from other publications (and acknowledges this fact). The added value (including the conclusions) of the present manuscript is not clear to me. The introduction should state how far further the authors wish to go, and the conclusions should say what they did and was not done before.

2. The methodology discussion (section 2) is too obscure to be useful. For example: "The Haar fluctuation (which is useful for -1<H<1) is *particularly easy to understand* since (with proper "calibration") in regions where H>0, it can be made very close to the

difference fluctuation (the differencing dominates over the averaging) and in regions where H<0, it can be made close to another simple to interpret "tendency fluctuation" (the averaging dominates over the differencing)." (p. 1262, with my outline of "particularly easy to understand"). This sentence might be grammatically correct, it is not an epitome of clarity. The rest of the section is from the same barrel.

3. What is the difference between statistical and ensemble averaging (p. 1262)? Does this mean that this is done on all grid points? I would be interested to see how the S or H vary with space in the model simulations and the proxy reconstructions. The addition of a spatial dimension certainly alters the temporal diagnostic: for example, the properties of multi-variate autoregressive processes cannot be estimated as simply as for univariate ones, due to cross-correlations.

4. There is no canonical definition of what is called macroweather in the text. Since this word does not appear in standard textbooks on meteorology or physics of climate, I doubt that its meaning can be understood by a general audience of climatologists. The implicit definition seems to stem from a typical temporal scale, but this rather contradicts the general statement of Bryson (1997), cited in the introduction, although it can indeed be criticized or refined.

5. I actually do not see the point of showing analyses for Vostok and GRIP ice core data: the variance of such time series is mostly dominated by orbital forcing and can hardly account for weather scales (less than a few decades or centuries). To be fair with climate models, it would be necessary to do the same analysis with intermediate complexity climate models, which have runs covering several ice ages... or to do the analysis on polar points of the GCM simulations (but those are generally bad).

6. In passing, I notice that Figure 2 analyses 240 kyr of data from GRIP: are the authors aware that only the 0-110 kyr BP of this ice core are valid, and the rest is disturbed by ice foldings? Hence Figure 2 should be redone.

7. I find the figures 3âĂŤ8 very hard to read and understand (with axes in the middle

of the figures). The figure captions are not very intelligible. The authors should refrain from commenting the figures in their captions (e.g. Figure 6).

Conclusion: the manuscript needs a lot of rewriting for clarification, and figure improvements and possibly simplifications.

Interactive comment on Earth Syst. Dynam. Discuss., 3, 1259, 2012.