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Earth System Dynamics Discussions

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

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How scaling fluctuation analyses change our view of the climate and its models

1. Introduction:

Thank you for your enthusiastic response to the paper. In brief, your questions concern the impact of the weather / macroweather / climate trichotomy on GCM's, in particular, assessing regional and decadal scale climate forecasts; whereas the present paper was primarily concerned with lower frequencies and global scales. In the discussion paper and the allied paper you cited ([Lovejoy and Schertzer, 2013a], see also [Lovejoy, 2013], and the book to appear at the end of February, [Lovejoy and Schertzer, 2013b]) the main point was the need to systematically understand GCM outputs, data and paleodata as functions of scale in both time and in space. In addition we proposed a specific tool: Haar fluctuations which were seductive because of their simplicity in

application and their simplicity in interpretation. When combined with simple statistics and scaling ideas, they have the potential to transform our view of the climate.

Before continuing, let us clear up a misunderstanding that is probably responsible for your single noted point of disagreement:

My only substantive disagreement is with the conclusion in the paper that '...we show that control runs only reproduce macroweather'.

What we implied was simply that the scaling statistics (such as spectra) of GMC control runs and real world data were actually very close to each other up to about the end of the macroweather regime, i.e. $\approx$ 30 years (for RMS fluctuations, the exponents and even the values were quite close, see e.g. figs. 4, 5 and fig. 1 below, top two curves). However, the similarity of the statistics of two series is a necessary, but not sufficient condition for them to be the same, indeed, they can clearly have identical spectral or fluctuation statistics without the existence of any statistical correlation whatsoever. However, our statement does leave the door open for climate forecasts up until the end of the macroweather regime (10 -30 yrs). The main questions raised by the paper are with respect to the even lower frequency (climate scale) statistics and concern apparent, (and likely) statistical dis-similarities: if the low frequency GCM and real world statistics disagree, then there is necessarily a problem. In point of fact, the Last Millenium reconstructions we examined were relatively weak at the low frequencies, but not hugely so. On the basis of our study of the scaling of the suggested forcings (solar, volcanic), we suspect that the deficit in low frequency variability will require the introduction of new (slow) climate processes and / or couplings (in accord with [Rial et al., 2004]). However at this stage, due to the uncertainties in the low frequency statistics of the reconstructions, this conclusion cannot be made too strongly.

To make more conclusive comments on most of the references you cite would require the results of a research program that we have only just begun in the last 2 -3 years. Nevertheless, I will try to give some specific responses based on some still provisional/

partial results. Hopefully these will soon be expanded and be the subject of full scale publications. I'll try to group these concerns around what I see to be the three key areas: a) a study of the model – model (within an ensemble) statistical characteristics of the GCM's (in both space and time), b) a study of the data – data statistical characteristics, c) a study of the data- model statistical characteristics. The first is needed in order to understand what the GCM's might be expected to deliver, while the second places interesting limits on our knowledge of the past and current state of the climate that implies limitations on our ability to evaluate GCM output fields. The third gives an idea of the current limits of the GCM accuracy.

2. The space-time characteristics of model-model, data-data and model-data differences

2.1 Temporal analyses

The use of GCM's to model the climate is based on the idea that while the weather is an initial value problem, that the climate is a boundary value problem. To paraphrase an old dictum: "for given boundary conditions, the climate is what you get". I realize that you have criticized this on the basis that the "boundaries" (such as ocean-atmosphere, ice-atmosphere etc.) are in reality interactive interfaces [Pielke, 1998], so that in reality the whole system reduces to an initial value problem. However, inasmuch as for the climate we are interested in appropriate statistics (rather than in deterministic values as for the weather), this criticism may not be as relevant as it seems since due to chaos, the effect of initial values will presumably decay over time - at least for periods longer than several times the "ocean-weather"/"macro-ocean weather" transition τmo of about a year are concerned. This is a consequence of that fact that τmo is the error doubling time of global scale ocean dynamics (which is apparently the longest (inverse) Lyapunov exponent in the weather/(near surface) ocean system and explains the results you cited from the [van Oldenborgh et al., 2012] paper. While this may explain the GCM results your comment may still be valid as concerns new, slow climate processes which are not yet incorporated in the models.

In any event, it is salutary to systematically study the convergence of GCM ensemble members to their "climate". Here, we use outputs from historical reconstructions, (from 1850-2005) from the NASA GISS model discussed in the paper; we consider two members of the ensemble. Each ensemble member was initialized using control runs, but then integrated forward with identical climate forcings (solar variations, volcanic eruptions, land use and aerosol and CO2 emissions). Fig. 1 shows the Haar fluctuation analysis of the difference between the average temperatures between $\pm$60o. This region was chosen since one of the reference fields: the 20CR surface temperature field (unlike the 700 mb field discussed in the paper) was not reliable much further north of 60o due to insufficiently resolved sea ice (G. Compo private communication). The other surface fields are those cited in the discussion paper (from NASA GISS, NOAA, NCDC, HadCRUT3), and show that in any case, the $\pm$60o statistics are not very different from the full global ones.

In the figure (GISS-GISS pixel and global scale curves), we see that the differences in temperature between the two members of the ensemble do indeed converge – as expected - in the sense that the fluctuations of the difference field decrease with time scale Δt. However, this convergence comes with interesting caveats. First consider the top solid curve, the pixel scale difference between the two ensemble members averaged over the 45oN longitudes (this is close to the same scale data but averaged over the globe). We see that the differences decrease from about 2K at 1 yr to 0.3K at 100 yrs; we can also note the divergence with respect to the observations (20CR) increasing from about 10 years onwards to longer lags (Δt). This is due to both (some) disagreements in the magnitude of global warming, but also in the amplitude of the low frequency natural variabilities.

Perhaps more interesting are the differences in the global temperatures ("global GISS-GISS" in the figure). While these also converge, they do so at what might be termed an "ultra-slow" rate: (, where Δt is in years, S is the mean absolute fluctuation in K). For example, while the GISS GCM defines the global temperature to about 0.2 K at

1 yr, even for 100 year averages, it has only converged to ≈0.1K. Similar analysis of corresponding convergence of MIROC GCM ensemble members (not shown) leads to nearly identical conclusions.

We have already mentioned the pixel scale GCM-data (GISS-20CR) comparison, let us turn our attention to the corresponding global scale curves in fig. 1. We see that there is no sign of the GISS GCM converging to the 20CR data: at all scales the difference is roughly constant at about 0.2K. In as much as the model and the 20CR data have fairly close overall global warming trends, this is best interpreted as a largely due to data and model errors and their differences in natural variability. Since the result holds at decadal scales, this gives a limit on the accuracy of the residuals of the decadal scale trends once CO2 and aerosol forcings have been removed and is in reasonable quantitative agreement with fig. 4 in [van Oldenborgh et al., 2012].

To put this in context, consider the bottom two curves in fig. 1. These are from comparisons of 4 surface temperature series (the three in the discussion paper as well as the near surface 20CR field) and quantify the scale by scale accuracies with which the global temperature can be estimated. The curves are from the mean of the (six) pairwise temperature differences, and from the (four) differences between the series and the mean of the four. Again we find the intriguing result that temporal averaging does not lead to improved estimates: there seems to be an intrinsic limit of about 0.05 to 0.1K in our knowledge of global temperatures, and this, at any scale! Since this also applies to decadal scales, it means that global scale decadal trends are not known to higher accuracy than this. This is in quantitative agreement with the results you cited of [Fyfe et al., 2011].

2.2 Spatial analysis

Now consider the corresponding spatial analysis of the monthly data at 1 pixel scale (2.5ox3.75o, resampled at 4ox4o to so as to facilitate comparison with the 20CR fields), shown in fig. 2. We can immediately see that contrary to the temporal behaviour where

the ensembles converge to a common global mean state – even if slowly – that in space the ensembles diverge (with , θ is the longitudinal angle of separation of two points) until ≈ 90o (≈ 5000 km) scales. This indicates rather directly that – at least at these time scales - the GCM intrinsically cannot provide a meaningful notion of climatic region smaller than this (continental) scale. Unfortunately, as of this writing, the corresponding analyses for the spatial analysis of the longer time averages are not available, but due to the fairly accurate factorization property of space-time fluctuations in the macroweather regime ([Lovejoy and Schertzer, 2013b]), this conclusion will probably not change too much when the analysis is performed. This factorization can be understood in the sense that the spatial and temporal variability in the macroweather regimes are nearly independent of each other, so that temporal averaging will decrease the overall level of the fluctuations, but not the form of $S(\Delta\theta)$ function.

This conclusion - framed in terms of the observed futility of spatial averaging GCM ensemble members to yield accurate regional climate variations - was recently made by [Deser et al., 2012]. It is also in accord with the IPCC AR4 conclusion about "continental scale" regions being the smallest that have regional information, and it is close to the conclusions of [van Oldenborgh et al., 2012] paper about the general lack of regional skill beyond the greenhouse induced trends.

Finally, we compare the GCM regional climate convergence to itself with convergence to the 20CR data (top curve). We see that the GCM and data diverge until about 90o, although less rapidly than the divergence of the GCM ensemble members (exponent ≈0.2 rather than ≈0.4). The interpretation of this result is that the GCM's do not even start to agree with the 20CR data about regional climates until these (continental) scales (where the monthly resolution fields disagree by about 3K on average). Note that we could undoubtedly somewhat reduce the absolute level of these differences by making some of the data based GCM trend and other "adjustments" discussed in the paper you cited by [Zhongfeng and Yang, 2012], these will be unlikely to change the basic conclusions.

3. Precipitation

You also asked about GCM precipitation products ([Stephens et al., 2010]) and [Anagnostopoulos et al., 2010]. Without going into too much detail, I think that both papers are significantly flawed in that they do not address the fundamental problem of the space – time resolutions of their data. For example, ([Stephens et al., 2010]) compare essentially instantaneous CloudSat estimates at $\approx$ 1 km spatial resolution with GCM outputs at resolutions of hundreds of kilometers and 3 hours in time. While they are forced to admit that a problem exists, their solution is ad hoc and treats the problem as a purely spatial resolution issue. It avoids the central issue of space-time relations and the extraordinary intermittency of precipitation: the latter has by far the largest C1 value (characterizing the intermittency near the mean) – it is $\approx$ 0.3 -0.4 in space and in time, compared to $\approx$0.01- 0.02 for the temperature in the macroweather regime, and $\approx$0.07 for temperature in the weather regime). Therefore, while to a first approximation, intermittency is unimportant in macroweather temperatures, it is fundamental in weather regime precipitation (even macroweather precipitation has C1 $\approx$ 0.04 which already has some consequences due to the wide range of scales involved ([Lovejoy et al., 2012]), Since the 1980's we have published several dozen papers on the subject of space-time precipitation scaling (for an early review, see [Lovejoy and Schertzer, 1995], and for a recent global scale study, see [Lovejoy et al., 2012]) so that it is very frustrating to see that thirty years later, the mainstream is still ignoring the results of fundamental science!

These comments also apply to the [Anagnostopoulos et al., 2010] paper. In particular, their "point" daily gauge data are not space-time points and for comparisons to be valid, it is the space-time resolution which must be commensurate with the GCM space-time resolution, a fact that was at least intuitively grasped by their critics. I would like however to make an additional comment about their data analysis technique, the Aggregated Standard Deviation (ASD) method. As pointed out in [Lovejoy and Schertzer, 2013b], it is a special case of the tendency structure function with exponent HASG

(their "H", our subscript) given by HASG = 1+ ξ(2)/2= 1+H-K(2)/2 (see the discussion paper for definitions of the structure function exponent ξ(q) and the cascade exponent K(q)). However the key limitation of the ASG is that it only correctly estimates the fluctuations (and exponents) over the same restricted range as the tendency structure function i.e. -1<H<0 (for quasi-Gaussian processes, for 0< HASG <1). In other words, it is fine for macroweather but not for weather or climate applications. The Haar fluctuation (which is essentially the same but with a crucial extra differencing), is a vast improvement.

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Figure Captions:

Fig. 1: The problems of model-model, data-data and model-data convergence in time for monthly resolution data. GISS refers to the NASA GISS GCM, 20CR to the Twentieth Century Reanalysis project. The bottom two curves are from instrumental (NASA GISS, NOAA NCDC, HadCRUT3) and 20CR reanalyses. All curves marked "global" are in fact between ±60o latitude, the pixel scale curve is for 45oN, but is close to the corresponding global average at that spatial scale. The structure functions are first order (with the exception of the bottom two which are RMS; due to the low intermittency, the difference between RMS and first order S is about log101.25 $\approx$ 0.1 in the above).

Fig. 2: The problems of model-model, model-data convergence in space for monthly

resolution GISS GCM and 20CR data, compared on a common 4ox4o grid, for the first order structure functions, averaged over the latitudes between ±60o. The equations of the straight reference lines are indicated. Model – model convergence (the decreasing part of the lower curve) only begins at scales > 100o, similarly the model begins to converge to the reanalysis only at similar (continental) scales. These define the smallest scales at which the GCM's define regional climates, they are also the smallest scales at which models begin to converge to the data.

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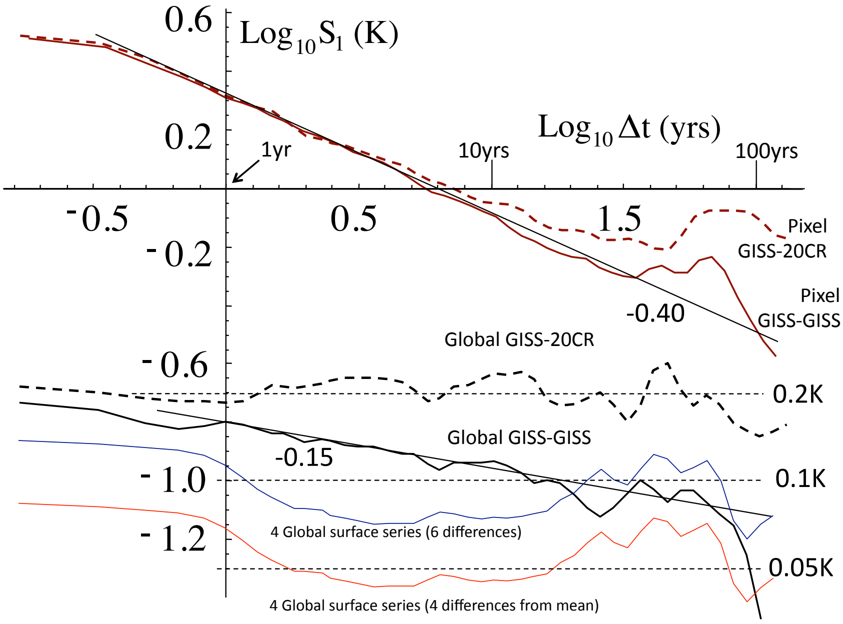

Fig. 1. Fig. 1: The problems of model-model, data-data and model-data convergence (see end of paper for full caption).

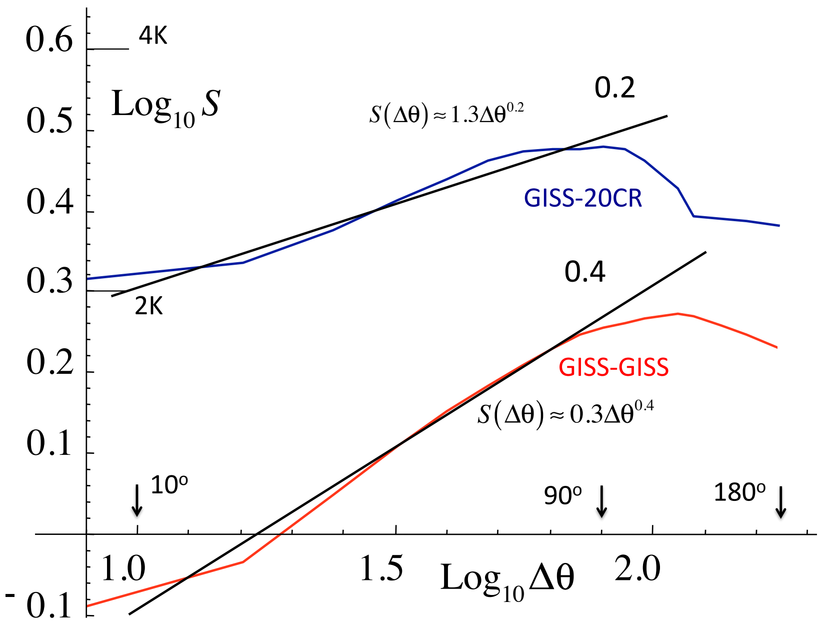

Fig. 2. Fig. 2: The problems of model-model, model-data convergence in space (see end of paper for full caption).