More, More comments on: “Are there multiple scaling regimes in Holocene temperature records?”

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My attitude was that – as long as the authors frankly acknowledged our difference in opinion - that their paper would be a welcome contribution to a necessary debate. However, their responses are unnecessarily aggressive and are not very frank, ignoring key issues rather than discussing them openly. The main points of the debate are:

a) There must be transition scale to a regime with fluctuations increasing with scale - the ice ages require it. Whether or not this regime is truly scaling is a secondary issue.
b) There is evidence that the transition scale is highly variable from location to location so that no global conclusions can be reached with analysis of Greenland Holocene ice cores.
c) I have nothing against statistical hypothesis testing (in 2014 I published a paper doing exactly that with the multiproxy data… but only at time scales up to 125 years a duration over which they agree with each other quite well!). In the present case - at multicentennial, multimillenial scales - the disagreements between the various proxies is so large that it makes clear the point that the question is a scientific one (i.e. what is the true nature of the variability?), not one that is sufficiently well circumscribed so as to be reducible to a purely technical, statistical one.

Surveying the current paper, responses and evolution of their paper since the original submission, the key point that must be addressed is point a) that I made back in in [*Lovejoy and Schertzer*, 1986] and re-iterated in my first response: that the existence of the ice ages requires a transition to a regime with fluctuations increasing with scale. This is obvious because at about 100 years the typical fluctuation in temperatures over local regions (e.g. 2^{o}X2^o) is of the order ±0.4^{o}C whereas at ice age scales (60 – 100 kyrs), it is ±2 to ±4^{o}C (the “glacial-interglacial window”). In [*Lovejoy and Schertzer*, 1986] - a paper that they systematically refuse to cite here or elsewhere- it was already estimated to have an exponent H = 0.4, but whether or not it is scaling is secondary, the main thing is that there must exist an increasing regime of some kind.

Table 11.4 A comparison of various estimates of the spectral exponents β_c of the climate regime and series lengths and resolutions. The last four rows are for the (anomalous) Holocene only (see Section 11.1.2, Fig. 11.2, and Section 11.3 for GCMs).

Series	Authors	Series length (kyr)	Resolution (yr)	β_c
Composite ice cores, instrumental	Lovejoy and Schertzer, 1986	Composite: minutes to 10^6 years	1000	1.8
$\delta\ ^{18}O$ from GRIP, Greenland	Schmitt *et al.*, 1995	123	200	1.4
$\delta^{18}O$ from GRIP, Greenland	Ditlevsen *et al.*, 1996	91	5	1.6
Composite, Vostok, Antarctica (ice core, instrumental)	Pelletier, 1998	10^{-5} to 1000	0.1 to 500	2
$\delta^{18}O$ from GRIP, Greenland	Wunsch, 2003	100	100	1.8
Planktonic $d^{18}O$ ODP677, Panama basin	Wunsch, 2003	1000	300	2.3
CO_2, Vostok, Antarctica	Wunsch, 2003	420	300	1.5
$\delta^{18}O$ from GISP, Greenland	Ashkenazy *et al.*, 2003	110	100	1.3
$\delta^{18}O$ from GRIP, Greenland	Ashkenazy *et al.*, 2003	225	100	1.4
$\delta^{18}O$ from Taylor, Antarctica	Ashkenazy *et al.*, 2003	103	100	1.8
$\delta^{18}O$ from Vostok	Ashkenazy *et al.*, 2003	420	100	2.1
Composite, mid-latitude	Huybers and Curry, 2006	10^{-4} to 1000	0.1 to 10^3	1.6
Composite tropics	Huybers and Curry, 2006	10^{-4} to 1000	0.1 to 10^3	1.3
$\delta^{18}O$ from GRIP, Greenland	This book	91	5	1.4
$\delta^{18}O$ from Vostok, Antarctica	This book	420	300	1.7
$\delta^{18}O$ from GRIP, Greenland	Blender *et al.*, 2006	3	3	0.4
$\delta^{18}O$ from GISP2, Greenland	Blender *et al.*, 2006	3	3	0.7
$\delta^{18}O$ from GRIP, Greenland (last 10 kyr only)	This book	10	5	0.2
Paleo-SST near Greenland (last 11 kyr only)	Berner *et al.*, 2008, analysed in this book	10.6	40	1.4

Table, 11.4, p.399 of [*Lovejoy and Schertzer*, 2013]. Ignoring intermittency corrections, exponents with $\beta_c>1$ have fluctuations increasing with scale.

In this paper, in order to avoid this glaringly obvious fact (confirmed by many authors who found regimes with spectral exponent $\beta>1$, see the above table for various references), the authors subjectively break the data into sections that conveniently exclude the transitions that dominate the low frequencies. This subjective filtering is a somewhat different here than in their accompanying paper on 1/f noise. Here, it is the low frequencies that are filtered so as to eliminate the key breaks in the scaling conveniently eliminating the "glacial-interglacial window". In the 1/f paper, they rather eliminate "spikes" that are the signatures of strongly non-Gaussian intermittency (and that occur over a range of frequencies), in that case, in order to justify the approximate Gaussianity of the subjectively filtered and hence intermittency - free result.

If the authors frankly acknowledged the existence of the "glacial-interglacial window" with fluctuation variance substantially larger than the fluctuation variance at century scales, then there would be no need to focus on fancy – and unconvincing - statistical tests. It would be clear that was is needed is the resolution of a scientific issue:

at what scale do decreasing fluctuations give way to increasing ones? And how does this scale vary from region to region and from epoch to epoch? This is why the authors' use of fGn models and statistical testing applied to data that has been subjectively filtered precisely to eliminate the central large low frequency events is not so interesting or relevant. This is even more true since they fall into the classical statistical testing error: the failure to reject the hypothesis that the data has two scaling regimes in no way forces us to accept that there is a single regime!

Therefore, much of the thrust of their paper is irrelevant to the key issue of determining the scale at which the decreasing fluctuation regime changes to an increasing one.

If their paper is to be useful contribution to debate, it should discuss the basic issues raised – even if only to argue against them. By simply ignoring inconvenient but critical points, the paper does not make a very useful contribution.

Detailed comments:

1) Returning to point b): there is evidence that the transition scale is highly variable from location to location so that no global conclusions can be reached with analysis of Greenland Holocene ice cores. I therefore stated that "the Greenland data are exceptional".

In their response, the authors react to this statement saying:

"In our opinion this is an inaccurate statement. We have to bear in mind that these ice cores are local measurements and as such their scaling characteristics are quite representative for high latitude continental interiors as seen e.g., in instrumental station data."

My opinion and the above stated author opinion are thus very close! However, the authors' new opinion still does not accord with their statements in the new version of the paper. For example, they nevertheless state:

"From our analysis we conclude that the two-regime model is not sufficiently justified for the Holocene to be used for temperature prediction on centennial time scales."

This statement (and others throughout the paper) implies that their conclusions apply over the whole globe, not just for "for high latitude continental interiors". If the authors' clearly stated their new view that the Greenland results are only "representative for high latitude continental interiors" and delete the sentences that imply more global consequences, then it would be acceptable.

2) I have nothing against statistical hypothesis testing (in 2014 just published a paper doing exactly that with the multiproxies…. but only at time scales up to 125 years at which scales they agree with each other quite well!), but in this case (at multicentennial, multimillenial scales) the disagreements between the various proxies at the indicated scales is so large that it makes clear the point that the question is a science one, not a technical, statistical one.

3) Line 23:
"The notion of "scaling" in climatic time series is based on the observation that the natural variability of the Earth's surface temperature can be modeled as a persistent stochastic process, with superposed trends and quasi-periodic modes representing variability which is not included in the noise background."

The sentence states what in fact has to be demonstrated! The reference to "persistent stochastic process" is misleading since persistence is a Gaussian notion (hence of limited applicability) which at best will apply to the integral of the temperature (not to the temperature itself), and this only over certain ranges.

4) Line 26: "The standard continuous-time stochastic LRM processes are the fractional Gaussian noise (fGn) and fractional Brownian motion (fBm)."

The authors' statement was arguably correct at the end of the 1970's, just before the discovery of multifractals. Since then, it has become clear that the generic scaling process is multifractal. Since 1983 the authors could at most say "for non- intermittent, non- multifractal processes, the standard continuous-time stochastic LRM processes are the fractional Gaussian noise (fGn) and fractional Brownian motion (fBm)." (Even in 1983 one could easily have added Levy processes to the list). Curiously, they admit (line 55) that "the records from the last glacial period exhibits strong intermittency" so that by their own admission, their restriction to fGn and fBm does not fully apply to paleo temperature series!

5) Line 35: This whole discussion is subjective, misleading. In the context of stochastic processes and when used correctly, the term "scaling" is a property of a *process* i.e. it is a property of the *infinite* ensemble of realizations generated by a process. It cannot be stressed enough that *each individual realization breaks the scaling*, even if the process itself is scaling over an infinite range! Scaling is a *statistical* symmetry principle holding exactly only over a statistical ensemble. That is why the scientific problem is – as always – to infer a process i.e. a model – from limited data. That is a fundamental disagreement that I have with the authors: the only circumstances in which one can reduce this nontrivial inference process to statistical testing is by *a priori* assuming a well defined stochastic process.

It is therefore simply obfuscation to talk about the scaling of a single realization over a single order of magnitude etc. The problem is simply one of trying to infer a physical process from evidence of perhaps a single series. It is misleading to dignify this inference process with the false appearance of mathematical rigour!

6) Line 60: The definition given of the scaling function only applies to functions with $H<0$ ($h<1$), so that it is *not* generally the same as the fluctuations used in the turbulence literature and which – depending on the exact definition of fluctuation - is not subject to this restriction. Another difference is that it is misleading to characterize the fluctuations of the integral of the series rather than the series itself. This departure from standard

practice (in the turbulence literature, going back nearly a century) leads to fluctuation functions that are needlessly difficult to interpret.

Ironically, it is a consequence of this needless difficulty that has lead the authors to miss the restriction. If one considers fluctuations of the series directly i.e. $\Delta T(\Delta t) = \Delta F/\Delta t$ with $\Delta T(\Delta t) \approx \Delta t^H$ – then one would see immediately that when $H>0$, fluctuations grow with scale and this is impossible for "the standard deviation of the data record after it has been filtered by a simple moving average with window width Δt": averaging obviously reduces the standard deviations. Later, in line 67-69, the authors acknowledge the problem… so why present things in this confusing way?

7) Line 64: "For a scaling process the PSD is of the power-law form with $\beta = 2\mathbf{h}+1$" . This is only a valid statement for nonintermittent, nonmultifractal processes. Otherwise – as has been known since Kolmorogov 1962 - there will be "intermittency corrections". It is too bad that the authors persist in ignoring 50 years of advances in turbulence and intermittency- especially when they admit that their process is "bursty" and "intermittent" in the same paper.

8) Line 70: The authors state: "One feature that the PSD, FA and Haar fluctuation share with many other measures of scaling is that it is sensitive to trends and large-scale oscillations, i.e., it is often not able to discriminate between such variability and true scaling behaviour." I don't understand this. If the low frequencies have a superposition of a scaling variability and a nonscaling (e.g. oscillatory variability), then no method can discriminate! In all cases, models and/or assumptions are required!

9) Line 91: Refering to Lovejoy 2012b "Common for the studies mentioned is that they don't make a distinction between glacial and Holocene spectra." This is not true, in the section of the cited paper entitled "The Holocene Exception: Climate Variability in Time and in Space" the epoch to epoch variations are investigated using pertinent data are analyzed. In the updated figure (from [*Lovejoy and Schertzer*, 2013] and reproduced in the comments to the earlier version, the geographical variations are discussed as well.

10) Line 188: I disagree with the statement: "The detrended fluctuation analysis (DFA) is an estimation technique that is commonly used for scaling analysis of climatic records, but it will not be used in this paper because it turns out to be particularly insensitive to scale breaks on scales comparable, or larger than, one tenth of the record length. This feature will be discussed in some detail in section 3.5."

As pointed out in [*Lovejoy and Schertzer*, 2012] the DFA is simply a "near" wavelet method of defining fluctuations. If it is used for the fluctuations rather than the series, and if it is multiplied by a factor of order twenty or so (depending on the data and the order of DFA), then the DFA fluctuations of appropriate order are nearly identical to the Haar fluctuations. The disadvantage of the DFA is that the theoretical basis is not as clear as for wavelets, that the fluctuations are unnecessarily difficult to interpret (hence the corresponding fluctuation function is always given without units!) and that the DFA fluctuations are more cumbersome to calculate.

The statements here and elsewhere (sections 3.4, 3.5 especially, and in the responses to the referees) about the problems of low frequency estimation for lengths >

1/10 or ¼ of the total length are *not* generally justified. As I pointed out in my first series of comments, any inference about the low frequencies – including these cautions – can only be justified by assuming that the data is a realization (or ensemble of realizations) from a specific stochastic process. Numerically testing with fGn is interesting, but is only relevant in as much as fGn is a good model for the process. It is quite misleading to give the impression that general "rules of thumb" exist applicable to general scaling processes.

11) Line 393: "Since the exact timing of the transition between the Holocene and the last glacial period is slightly different for Greenland and Antarctica, we have chosen the start and end of the time series carefully for each series, such that the transition is not contained in any of the "Holocene only" or the "glacial only" time series."

The authors present this subjective selection of the series as though it is needed for an objective analysis. On the contrary, as we have discussed, conditional sampling by avoiding large fluctuations will affect the statistics in several ways, including the artificial decrease in the low frequencies. In other words, the authors' sampling can only be justified by a model, it is not justified in some more objective manner.

This comment is pretty obvious but I mention it because the reason that a scale break must exist is precisely because the ice ages require any regime with decreasing fluctuations to give way to a regime with increasing low frequency fluctuations, the "glacial-interglacial window" discussed above.

References

Lovejoy, S., and D. Schertzer (1986), Scale invariance in climatological temperatures and the local spectral plateau, *Annales Geophysicae*, *4B*, 401-410.
Lovejoy, S., and D. Schertzer (2012), Haar wavelets, fluctuations and structure functions: convenient choices for geophysics, *Nonlinear Proc. Geophys.* , *19*, 1-14 doi: 10.5194/npg-19-1-2012.
Lovejoy, S., and D. Schertzer (2013), *The Weather and Climate: Emergent Laws and Multifractal Cascades*, 496 pp., Cambridge University Press, Cambridge.