Is consistency a limitation? — Reply to “Further (monofractal) limitations of climactograms” by Lovejoy et al.

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We appreciate the discussion comment by S. Lovejoy, D. Schertzer and I. Tchigirinskaya (hereinafter Lovejoy et al. 2013) on our paper (Lombardo et al., 2013a). Comparing the terminology, notation, semantics and mathematical content in Lombardo et al. (2013a), on the one hand, and Lovejoy et al. (2013), on the other hand, one may notice big differences, which make communication difficult. Therefore, we wish to commend the discussers for their comment, which hopefully helps to remove the communication gap due, perhaps, to the different scientific origins of the two groups of authors. In response to their comment, we will try to make our points clearer, although we must warn from the outset that we adhere to the language of stochastics, as exemplified in Papoulis’s (1991) book.

A note about terminology

We appreciate the discussers’ comments about our re-baptizing of certain things and we wish to give a couple of clarifications. To start with, what we call the *Hurst-Kolmogorov process* is not exactly the *fractional Brownian motion* as the discussers state, but the so-called *fractional Gaussian noise* (for the former we would use the term *cumulative Hurst-Kolmogorov process*). The reasons we avoid using the term *fractional Gaussian noise* are three. First, we dislike the term *fractional*, which may be popular (and suggestive of the other popular term, *fractal*) but in our view is also misleading (see the last paragraph of the next section, related to the difference between fractal and long-term stochastic properties). Second, while we recognize that *Gaussian* is accurate for the original formulation of that process, we think that it overemphasizes the Gaussian aspect, while non-Gaussian transformations are possible—and actually we use a lognormal version of it. Third, we strongly disagree to call *noise* a stochastic process which is used to describe natural processes. Generally, *noise* is used (e.g. in electronics, information and communication) in contrast to *signal*. The distinction implies that there is some signal that contains information, which is contaminated by a (random) noise. Noise should be identified and removed from the signal to recover the maximum of information. Such a distinction may not have a meaning in geophysics as Nature’s signs are *signals* in their entirety even though they may look like *noise*.

Furthermore, the reason we prefer the term *Hurst-Kolmogorov process* is simple. We wish to associate the process on the one hand to Hurst, who was the first to observe and analyse the behaviour signified by this process in Nature, and on the other hand, to Kolmogorov, who was the first to point out the existence of this mathematical process.

In addition, we wish to clarify that we do not use the term *climactogram* as the discussers say but the term *climacogram*. The latter term was introduced and justified etymologically by Koutsoyiannis (2010). We do not know whether the authors want to contribute in re-baptizing by adding a ‘t’ (we would not agree, though, as this would not be etymologically justifiable) , they want to indicate the way they read our paper, or they just want to make the tone whimsical.

On “limitations” of the climacogram

According to Lovejoy et al. (2013):

> “the ‘climactogram’ is only related to the autocorrelation and the spectrum for a narrow range of scaling exponents 0< H_{clim} <1 (the authors’ exponent ‘H’ that we denote ‘H_{clim}’ […]). For scaling, Gaussian processes (e.g. in 1-D, a Gaussian white noise filtered by $\omega^{-\beta/2}$ where ω is the frequency), when for $-1 < \beta < 1$ this corresponds to spectral exponent $\beta = 2H_{\mathrm{clim}} - 1$. When β is outside this range, then the relationship between β and H_{clim} breaks down.”

We are afraid that we have to disagree with this statement and we have strong reasons for this disagreement:

1. The climacogram and the power spectrum are fully equivalent to each other, as well as to the autocorrelation function. Each of these three functions is theoretically derived by any of the other two (see Koutsoyiannis, 2010, for the relationships for discrete time processes and Koutsoyiannis, 2013a, for those for the continuous time processes; see also equations (1)-(3) below). Therefore, a property in the spectral representation should have a one-to-one correspondence with a property in the climacogram representation. Thus, it is not meaningful, to speak about “break down” of relationships.
2. Exponents in the spectral representation, even those out of the interval (−1, 1), will be captured by a climacogram representation too. For low frequencies these can be captured by the climacogram per se, while for high frequencies this may require simple algebraic transformations of the climacogram, such as the climacogram-based pseudospectrum (CBPS) shown in Koutsoyiannis (2013b). Thus, in the example 8 of Koutsoyiannis (2013b) the slope of the spectral density for high frequencies equals 2 and this is fully (and much better than in the spectral representation) captured in the climacogram representation (the CBPS).
3. However, we stress that, according to our calculations (see below), an exponent in the spectral representation out of the range $-1 < \beta < 1$, in particular an exponent $\beta > 1$, even though, as stated in point 2, it can appear for high and intermediate frequencies, is not a valid one for low frequencies (tending to 0). We are aware of several publications showing slopes violating this (e.g. Lovejoy et al., 2012 and Lovejoy and Schertzer, 2013, report a slope $\beta = 1.4$ for low frequencies), but we believe these are spurious and theoretically inconsistent.* In our opinion they just manifest estimation errors due to

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* For the completeness of this discussion, it is footnoted that the discussion has started within a guest post by Shaun Lovejoy entitled “Macroweather, not climate, is what you expect” (21 January 2013) in Judith Curry’s blog, judithcurry.com/2013/01/21/macroweather-not-climate-is-what-you-expect/. In reply to a blogger’s comment, Lovejoy diagnosed the “key limitation” of the climacogram reiterated in the present discussion. Koutsoyiannis then intervened and his replies included the following statement (judithcurry.com/2013/01/21/macroweather-not-climate-is-what-you-expect/#comment-288742).

“Meanwhile, if you can give me a rigorous example to include in my study, which you believe supports your claim, I will try to include it. I do not mean an algorithmic procedure (do this, do that). I mean, give me the analytical equation of a power spectrum (covering all frequency domain, from zero to infinity) or, if you prefer, the autocorrelation function (covering lags from zero to infinity) that supports your claim.”

Unfortunately, no such (counter)example is contained in the discussion (Lovejoy et al., 2013) or was otherwise provided to us.

inappropriate algorithms, allowed by the fact that the empirical power spectrum has a rough shape; Koutsoyiannis (2013b) and Lombardo et al. (2013b) have proposed an alternative estimation based on the climacogram (the CBPS) to avoid such spurious results.

The proof for our claim in point 3 and several other details can be found in Koutsoyiannis (2013a). However, for the completeness of our reply we re-derive here one of the results indicating the inconsistency of the case $\beta > 1$. We start the proof recalling that the power spectrum (or spectral density) $s(w)$ of a continuous-time stochastic process is:

$$s(w) = 4\int_0^\infty c(\tau)\cos(2\pi w\tau)\,\mathrm{d}\tau \tag{1}$$

where w stands for frequency and $c(\tau)$ is the autocovariance function of the process for time lag τ. The inverse transformation is:

$$c(\tau) = \int_0^\infty s(w)\cos(2\pi w\tau)\,\mathrm{d}w \tag{2}$$

At the same time, the climacogram (i.e. the variance $\gamma(\Delta)$ for time scale Δ) is:

$$\gamma(\Delta) = \frac{2}{\Delta^2}\int_0^\Delta (\Delta-\tau)c(\tau)\mathrm{d}\tau = 2\int_0^1 (1-\xi)c(\xi\Delta)\mathrm{d}\xi \tag{3}$$

As both the climacogram and the power spectrum are transformations of the autocovariance function, the two are also related to each other by simple transformations. Specifically, combining (3) and (2) we find:

$$\gamma(\Delta) = 2\int_0^1 (1-\xi)\int_0^\infty s(w)\cos(2\pi w\xi\Delta)\,\mathrm{d}w\,\mathrm{d}\xi = 2\int_0^\infty s(w)\int_0^1 (1-\xi)\cos(2\pi w\xi\Delta)\,\mathrm{d}\xi\,\mathrm{d}w \tag{4}$$

After algebraic manipulations, we obtain the following equation giving directly the climacogram from the power spectrum:

$$\gamma(\Delta) = \int_0^\infty s(w)\,\frac{\sin^2(\pi w\Delta)}{(\pi w\Delta)^2}\,\mathrm{d}w \tag{5}$$

Let us denote $s^\#(w)$ the slope of the power spectrum $s(w)$ plotted on logarithmic axis vs. the logarithm of the frequency w, i.e.,

$$s^\#(w) \coloneqq \frac{\mathrm{d}(\ln s(w))}{\mathrm{d}\,(\ln w)} = \frac{w\,s'(w)}{s\,(w)} \tag{6}$$

where $s'(w)$ is the derivative of $s(w)$.

Now, let us assume that for a frequency range $0 \le w \le \varepsilon$, with ε however small, the logarithmic slope of the power spectrum is $s^\#(w) = -\beta$, or else $s(w) = \alpha w^{-\beta}$ where α and β are constants, with $\beta > 1$. We notice in (5) that the fraction within the integral takes significant values only for $w < 1/\Delta$ (cf. Papoulis, 1991, p. 433). Hence, assuming a scale $\Delta \gg 1/\varepsilon$, and with reference to (5) we may write:

$$\gamma(\Delta) = \int_0^\infty s(w)\frac{\sin^2(\pi w\Delta)}{(\pi w\Delta)^2}\,\mathrm{d}w \approx \int_0^\varepsilon \alpha w^{-\beta}\,\frac{\sin^2(\pi w\Delta)}{(\pi w\Delta)^2}\,\mathrm{d}w \tag{7}$$

On the other hand, it is easy to verify that, for $0 < w < 1/\Delta$,

$$\frac{\sin(\pi w\Delta)}{\pi w\Delta} \geq 1 - w\Delta \geq 0 \tag{8}$$

and since $\varepsilon \gg 1/\Delta$, while the function in the integral (7) is nonnegative,

$$\gamma(\Delta) \approx \int_0^\varepsilon \alpha w^{-\beta}\,\frac{\sin^2(\pi w\Delta)}{(\pi w\Delta)^2}\,\mathrm{d}w \geq \int_0^{1/\Delta} \alpha w^{-\beta}\,\frac{\sin^2(\pi w\Delta)}{(\pi w\Delta)^2}\,\mathrm{d}w \geq \int_0^{1/\Delta} \alpha w^{-\beta}(1 - w\Delta)^2\,\mathrm{d}w \tag{9}$$

Substituting $\xi = w\Delta$ in (9), we find:

$$\gamma(\Delta) \geq a\Delta^{\beta-1}\int_0^1 \xi^{-\beta}(1-\xi)^2\,\mathrm{d}\xi \tag{10}$$

To evaluate the integral in (10) we take the limit for $q \to 0$ of the integral:

$$B(q) \coloneqq \int_q^1 \xi^{-\beta}(1-\xi)^2\,\mathrm{d}\xi \;= \frac{q^{1-\beta}-1}{\beta-1} - 2\,\frac{q^{2-\beta}-1}{\beta-2} + \frac{q^{3-\beta}-1}{\beta-3} \tag{11}$$

Clearly, for $\beta > 1$ the first term of the latter integral diverges for $q \to 0$, i.e., $B(0) = \infty$, and thus, by virtue of the inequality (10), $\gamma(\Delta) = \infty$. Therefore, the process is non-ergodic (see Papoulis, 1991, p. 429).* This analysis generalizes a result by Papoulis (1991, p. 434) who shows that an impulse at $w = 0$ corresponds to a non-ergodic process.

In a non-ergodic process there is no possibility to infer statistical properties from the samples (as temporal averages do not represent true statistical properties). In any statistical analysis based on time series, ergodicity is necessary for the analysis to be valid. Otherwise the analysis is in vain and hence empirical results of this type are not meaningful because they contradict the basic condition on which they are based. Actually, such contradiction, when emerging from processing of data, does not suggest that a process is non-ergodic. Usually it only suggests that the algorithm used is inconsistent.

Therefore, we believe that the above comment by Lovejoy et al. (2013) is not valid and perhaps is affected by an older trend in the literature (e.g., Harris et al., 2001, Fig. 4) to mix up the fractal dimension (or a transformation thereof) of a process with the Hurst coefficient. In fact the two are distinct concepts, generally independent to each other. Fractal dimension is a local property applying to small time scales or high frequencies, whereas the Hurst coefficient (and the implied long-range dependence) is a global characteristic applying to large scales or low frequencies. This has been made clear by Gneiting and Schlather (2004).

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* It is interesting to note that, if $|\beta| < 1$, the integral in (7) can be evaluated to give:

$$\gamma(\Delta) \approx \alpha\int_0^\infty w^{-\beta}\,\frac{\sin^2(\pi w\Delta)}{(\pi w\Delta)^2}\,\mathrm{d}w = \frac{\sin(\pi\beta/2)}{(\pi\beta/2)}\,\frac{(2\pi)^\beta\alpha\Gamma(1-\beta)}{2(\beta+1)\Delta^{1-\beta}}$$

Clearly, for $\Delta \to \infty$, the last expression gives $\gamma(\Delta) \to 0$ and thus for $|\beta| < 1$ the process is mean ergodic.

On alternative tools

According to Lovejoy et al. (2013):

> "In [Lovejoy and Schertzer, 2012] it was pointed out that a single tool – the Haar fluctuation – conveniently covers the whole range $-1 < H < 1$ yet remains simple to calculate and interpret:
>
> $$(\Delta X(t,\Delta t))_{Haar} = \frac{2}{\Delta t}\int_{t}^{t+\Delta t/2} X(t')\,\mathrm{d}t' - \frac{2}{\Delta t}\int_{t+\Delta t/2}^{t+\Delta t} X(t')\,\mathrm{d}t'$$
>
> i.e $(\Delta X(t,\,\Delta t))_{\mathrm{Haar}}$ is simply the difference between the means of the first and second halves of the interval Δt."

We thank the discussers for pointing out this tool, which we may explore in future studies. On the other hand, we wish to stress that we cannot believe without proof that it "remains simple to calculate and interpret", while we believe that expressions like "is simply the difference between the means" may be misleading. For, the definition provided (copied above) merely defines a stochastic process (assuming that $X(t)$ is also a stochastic process) and does not provide any information on the statistical behaviour of that process. In stochastics, it is always important to study the statistical properties of a random variable or of a stochastic process. Whether or not a tool is "simple to calculate", etc., does not depend on its definition per se, but on the ease of the calculation of its statistical properties as well as on whether or not the latter have specific desirable characteristics, like unbiasedness, well-behaved distribution function with small variance, etc.

To clarify better what we mean, we will use an example from our paper in discussion (Lombardo et al., 2013a). Given a random variable $\underline{x}$, the fifth moment is quite easy to define as the expected value of $\underline{x}^5$, i.e. $\mathrm{E}[\underline{x}^5]$. However, it is not this that matters. Before we try to use it, we should have in mind a few fundamental things of statistics. Assuming that we have available a time series x_i (i =1, …, n) of $\underline{x}$, first, we need to distinguish between the following totally dissimilar quantities:

(a) $\underline{x}^5$, which is a random variable;

(b) $\mathrm{E}\left[\underline{x}^5\right]$, the true expected value (defined as $\mathrm{E}\left[\underline{x}^5\right] = \int_{-\infty}^{\infty} x^5 f(x)\mathrm{d}x$, where $f(x)$ is the probability density function of $\underline{x}$) , which is a regular (not random) variable;

(c) $\sum_{i=1}^{n} x_i^5/n$, the sample average of x_i^5, which is a number, known as the estimate of $\mathrm{E}\left[\underline{x}^5\right]$; and

(d) $\sum_{i=1}^{n} \underline{x}_i^5/n$, which is a random variable known as the estimator of $\mathrm{E}\left[\underline{x}^5\right]$; this is like a temporally averaged stochastic process.

Next, we should try to study the statistical properties of the estimator $\sum_{i=1}^{n} \underline{x}_i^5/n$. In this respect, as shown in our paper (Lombardo et al., 2013a) and, in particular, illustrated in its Figures 3 and 4, even though the estimator has the property of unbiasedness, i.e., $\mathrm{E}\left[\sum_{i=1}^{n} \underline{x}_i^5/n\right] = \mathrm{E}\left[\underline{x}^5\right]$, unfortunately its density function extends (takes non-negligible values) over more than four orders of magnitude and its mode is two orders of magnitude lower than its expected value. For these reasons, it is not advisable to use it at all—and that is what our paper (Lombardo et al., 2013a) is about in simple words.

Now, we have not studied the respective properties of the Haar fluctuation (but we may do this in future studies). On the other hand, those of the climacogram have been studied

(Koutsoyiannis, 2003, 2011, 2013a,b; Koutsoyiannis and Montanari, 2007; Koutsoyiannis et al., 2011; Tyralis and Koutsoyiannis, 2011) and suggest its appropriateness to use it safely in statistical analyses of geophysical processes—and that is the reason why we use it.

Conclusion

We believe that the comment by Lovejoy et al. (2013) is extremely useful to clarify several issues and facilitate the communication between groups so far using different languages, but it does not imply that our manuscript Lombardo et al. (2013a) needs any correction or change.

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