Two comments on “Naturally trendy?” by Rasmus E. Benestad on *Real Climate*

Demetris Koutsoyiannis

Department of Water Resources, Faculty of Civil Engineering, National Technical University of Athens, dk@itia.ntua.gr - http://www.itia.ntua.gr/dk

Publication of the article Cohn T.A., and H.F. Lins, Nature's style: Naturally trendy, *Geophysical Research Letters*, 32(23), art. no. L23402, 2005, triggered stormy discussions on the internet on climatic blogs. In these two comments it is maintained that standard climatic statistics are incorrect and that the work by Cohn and Lins and its discussion will lead to more correct statistical methods and more consistent statistical thinking.

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Comment 1*

Even though I do not concur with the views of this article, I must congratulate the author for discussing and disseminating to climatologists the recent work of Cohn and Lins and, indirectly, the consequences of the related natural behaviour (that this work examines) to statistical inferences and modelling.

In fact this “trendy” behaviour has been known for at least 55 years since Hurst reported it as a geophysical behaviour or for 65 years since Kolmogorov introduced a mathematical model for this. It is known under several, more or less successful, names such as: Long Term Persistence, Long Range Dependence, Long Term Memory, Multi-Scale Fluctuation, the Hurst Phenomenon, the Joseph Effect and Scaling Behaviour; other names have been used for mathematical models describing it such as: Wiener Spiral (the first term used by Kolmogorov), Semi-Stable Process, Fractional Brownian Noise, Self-Similar Process with Stationary Intervals, or Simple Scaling Stochastic Process.

I think that this behaviour relates to climatology far more than to any other discipline and I wonder why it has not been generally accepted so far in climatological studies (or am I wrong?). In contrast in many engineering studies, for example of reservoir designs, the

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consequences of this behaviour are analysed. Also, the same behaviour has been studied by economists and computer and communication scientists in their own time series.

Of course, this behaviour is not only met in the record analyzed in Cohn and Lins work. In contrast, a lot of studies have provided evidence that it is probably omnipresent at all series and at all times i.e., not only in the 20th century – but it can be seen only on long records. To mention a single example, the Nilometer data series (maximum and minimum levels of the Nile River), which clearly exhibits this behaviour, extends from the 7th to at least the 13th century AD. Cohn and Lins' article contains a lot of references to works that have provided this evidence. For those who may be interested on more recent references, here is a list of three contributions of mine, trying to reply to some questions related to the present discussion:

What is a "trend"? *What is the meaning of a "nonstationary time series"? How are these related to the scaling behaviour?* See Koutsoyiannis (2006a).

Can simple dynamics (which do not change in time) *produce scaling* ("trendy", if you wish) *behaviour*? See Koutsoyiannis (2006b).

Why the scaling behaviour (rather than more familiar ones described by classical statistics) *seems to be so common in nature?* See: Koutsoyiannis (2005).

Comment 2†

1. "Statistical questions demand, essentially, statistical answers". (Here I have quoted Karl Popper's second thesis on quantum physics interpretation - from his book "Quantum Theory and the Schism in Physics"). The question whether "The GCMs [...] give a good description of our climate's main features" (quoted from the Rasmus's response) or not is, in my opinion, a statistical question as it implies comparisons of real data with model simulations. A lot of similar questions (e.g., Which of GCMs perform better? Are GCMs future predictions good enough? Do GCM simulations reproduce important natural behaviours?) are all statistical questions. Most of all, the "attribution" questions (to quote again Rasmus, "how much of the trend is natural and how much is anthropogenic" and "to which degree are the variations 'natural") are statistical questions as they imply statistical testing. And undoubtedly, questions related to the uncertainty of future climate are clearly statistical questions. Even if one

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believes that the climate system is perfectly understood (which I do not believe, thus not concurring with Rasmus), its complex dynamics entail uncertainty (this has been well documented nowadays). Thus, I doubt if one can avoid statistics in climatic research.

2. Correct statistical answers demand correct statistics, appropriate for the statistical behaviours exhibited in the phenomena under study. So, if it is “well known” that there is long term persistence (I was really happy to read this in Rasmus’s response) then the classical statistical methods, which are essentially based on an Independent Identically Distributed (IID) paradigm are not appropriate. This I regard as a very simple, almost obvious, truth and I wonder why climatic studies are still based on the IID statistical methods. This query as well as my own answer, which is very similar to Cohn and Lins one, I have expressed publicly three years ago (Koutsoyiannis, 2003). In this respect, I am happy for the discussion of Cohn and Lins (2005) work hoping that this discussion will lead to more correct statistical methods and more consistent statistical thinking.

3. Consequently, to incorporate the scaling behaviour in the null hypothesis is not a matter of “circular reasoning”. Simply, it is a matter of doing correct statistics. But if one worries too much about “circular reasoning” there is a very simple technique to avoid it, proposed ten years ago in this very important paper by von Storch (1995). This technique is to split the available record into two parts and formulate the null hypothesis based on the first part.

4. Using probabilistic and statistical methods should not be confused with admitting that things “happen spontaneously and randomly” or “without a cause” (again I quoted here Rasmus's response). Rather, it is an efficient way to describe uncertainty and even to make good predictions under uncertainty. Take the simple example of the movement of a die and eventually its outcome. We use probabilistic laws (in this case the Principle of Insufficient Reason or equivalently The Principle of Maximum Entropy) to produce that the probability of a certain outcome is 1/6 because we cannot arrive at a better prediction using a deterministic (causative) model. This is not a denial of causal mechanisms. If we had perfectly measured the position and momentum of the die at a certain moment and the problem at hand was to predict the position one millisecond after, then the causal mechanisms would undoubtedly help us to derive a good prediction. But if the lead time of one millisecond needs to be a few seconds (i.e. if we are interested about the eventual outcome), then the causal mechanisms do not help and the probabilistic answers become better. May I add here my opinion that the climate system is perhaps more complex than the movement of a die. And may I endorse this thesis saying that statistical thermophysics, which is based on probabilistic considerations, is not at all a denial of causative mechanisms. Here, I must admit that I am ignorant of the detailed structure of GCMs but I cannot imagine that they are not based on statistical thermophysics.

5. I have difficulties to understand Rasmus's point “A change in the global mean temperature is different to, say the flow of the Nile, since the former implies a vast shift in heat (energy), and there has to be physical explanations for this.” Is it meant that there should not be physical explanations for the flow of the Nile river? Or is it meant that the changes in this flow do not reflect changes in rainfall or temperature? I used the example of Nile for three reasons. Firstly, because its basin is huge and its flow manifests an integration of climate over an even more extended area. Secondly, because it is the only case in history that we have an instrumental record of a length of so many centuries (note that the measurements are taken in a solid construction known as the Nilometer), and the record is also validated by historical evidence, which for example witness that there were long periods with consecutive (or very frequent) droughts and others with much higher water levels. And thirdly, because this record clearly manifests a natural behaviour (it is totally free of anthropogenic influences because it covers a period starting at the 6th century AD).

6. I hope that my above points should not be given a “political” interpretation. The problem I try to address is not related to the political debate about the reduction of CO_2 emissions. Simply I believe that scientific views have to be as correct and sincere as possible; I also believe that the more correct and sincere these views are the more powerful and influencing will be.

References

Cohn T.A., and H.F. Lins, Nature's style: Naturally trendy, *Geophysical Research Letters*, 32(23), art. no. L23402, 2005.

Koutsoyiannis, D., Climate change, the Hurst phenomenon, and hydrological statistics, *Hydrological Sciences Journal*, 48(1), 3-24, 2003 – http://www.extenza-eps.com/IAHS/doi/abs/10.1623/hysj.48.1.3.43481.

Koutsoyiannis, D., Uncertainty, entropy, scaling and hydrological stochastics, 2, Time dependence of hydrological processes and time scaling, *Hydrological Sciences Journal*, 50(3), 405-426, 2005 (http://www.extenza-eps.com/IAHS/doi/abs/10.1623/hysj.50.3.405.65028; jsessionid= noyCMpKB1OFcDF7U5H?cookieSet=1&journalCode=hysj).

Koutsoyiannis, D., Nonstationarity versus scaling in hydrology, *Journal of Hydrology*, 2006a (article in press; http://dx.doi.org/10.1016/j.jhydrol.2005.09.022).

Koutsoyiannis, D., A toy model of climatic variability with scaling behaviour, *Journal of Hydrology*, 2006b (article in press; http://dx.doi.org/10.1016/j.jhydrol.2005.02.030).

von Storch, H., Misuses of statistical analysis in climate research. In H. von Storch and A. Navarra (eds.): *Analysis of Climate Variability Applications of Statistical Techniques*. Springer Verlag, 11-26, 1995 (http://w3g.gkss.de/staff/storch/pdf/misuses.pdf).