

Prehistory of:

Climatic variability over time scales spanning nine orders of magnitude: Connecting Milankovitch cycles with Hurst-Kolmogorov dynamics

by Yannis Markonis and Demetris Koutsoyiannis

Editor's decision on the submission to *Geophysical Research Letters*

Subject: 2011GL049194 (Editor - Noah Diffenbaugh): Decision Letter

Date: Thu, 8 Sep 2011 14:47:01 -0400

Thank you for submitting the manuscript "Climatic variability over time scales spanning nine orders of magnitude: connecting Milankovitch cycles with Hurst-Kolmogorov dynamics" (2011GL049194) to Geophysical Research Letters, which we have now had reviewed for both scientific content and GRL-specific criteria. Based on this evaluation, my conclusion is that the paper does not represent a sufficiently significant advance to meet the GRL standards of immediate impact requiring rapid publication. Therefore, I regret to inform you that I cannot consider the manuscript further for publication in Geophysical Research Letters.

Attached below are the review comments, which you may find helpful if you decide to revise the paper and submit it to another journal. You will see that the review raised important concerns about the robustness of the results and some of the methods used, and suggests that the work requires clarification beyond the space constraints of a GRL paper.

GRL has heavy demand for limited publication space, which means that we must unfortunately reject many potentially good papers, including some for which the specific issues raised by the reviewers could eventually be addressed by the authors. In this case, although I expect that the manuscript will eventually be publishable in a more narrowly focused journal, my ultimate conclusion is that the work does not represent a sufficiently significant advance to meet the GRL criteria.

I am sorry that I cannot be more encouraging at this time, but I hope that you and your co-authors will continue to submit your best work to GRL.

Sincerely,

Noah Diffenbaugh
Editor
Geophysical Research Letters

Review comments on the submission to *Geophysical Research Letters* and author responses

Authors' note: these review comments and author responses have been attached in both following submissions, initially to *Nature Geosciences* and finally to *Surveys in Geophysics*.

Reviewer #1

This study focuses on the characterization of climate variability over a wide range of temporal scales. Results are based on two instrumental series and eight proxy time series. According to the authors, the data support the presence of long-term persistence. This is an interesting topic, with possibly large implications for the way we interpret the current increase of global temperature. After reading this manuscript, I have several questions that would potentially require additional analyses and more text and figures. It is therefore my opinion that a journal without page limits may be more appropriate.

Line 57: *'The ice-core and sediment reconstructions had varying time step and were converted by linear interpolation to constant time step, not greater than the varying raw time step.'*

What is the impact of the linear interpolation on the results? Would the authors get the same results using a different interpolator? Please discuss.

A supporting figure (in Supplement, Section B) shows that linear interpolation does not affect the estimation of H .

Line 90: *'if treated stochastically will have marginal density function $f(x) = 1/(\pi\sqrt{2-x^2})$, mean 0, variance 1, and autocovariance $c(\tau) = \text{cov}[x(t), x(t + \tau)] = \cos(2\pi\tau/T)$, which does not depend on t . It is then readily showed that its climacogram is $\sigma^{(k)} = [T/(\pi k)] |\sin(\pi k/T)|$ (3)'*

I am always a bit worried when I read something like "and by simple algebraic manipulations we show that..." Based on my experience, it is rarely "simple" or "straightforward." Would it be possible to show the derivation of equation 3 in an appendix or supplemental material?

We thought that an average reviewer/reader should be able to understand this and reproduce the calculation with ease. In any case, the calculation is included in the Supplement, Section A.

By reading this manuscript, it is not clear to me what type of estimator was used to estimate H . How was the line in Figure 2 fitted? As discussed in the recent work by Serinaldi (2010), it is important to examine the power spectrum to select the right type of estimator.

As we state in the paper, *'The climacogram is simpler and more robust than other commonly used stochastic tools, the power spectrum and the autocorrelogram, although it is related to them by simple transformations (Koutsoyiannis, 2010)'*. The power spectrum is a useful tool, as discussed by Serinaldi (2010). The two are theoretical equivalent but the power spectrum gives

much less accurate and uncertain estimates (see also Tyrallis and Koutsoyiannis, Simultaneous estimation of the parameters of the Hurst-Kolmogorov stochastic process, Stochastic Environmental Research & Risk Assessment, 25 (1), 21–33, 2011). Of course the reviewer may feel free to construct power spectra (the data are available on-line) to verify that they will give roughly the same result with less accuracy.

The effects of data inhomogeneities on the estimation of the Hurst exponent were recently discussed by Rust et al. (2008). I think that this issue should be discussed in this study.

The data we use are given as homogenized (particularly the instrumental ones). Rust et al. (2008) state *'Analyzing a set of temperature time series before and after homogenization with respect to LRD we find that the average Hurst exponent is clearly reduced for the homogenized series'*. This implies that H could actually be higher than 0.92, which is in not in disagreement with the use of this value as the lowest bound, as we clearly state.

There are large sampling uncertainties associated with the estimation of H from relatively short time series (Koutsoyiannis and Montanari 2007). What are the sampling uncertainties in this case?

More careful reading of Koutsoyiannis and Montanari (2007) will reveal that the strongest statistical effect of the presence of HK behaviour is the negative bias in the calculation of H . Therefore our conclusion that $H = 0.92$ is a lowest bound stands.

In a recent paper by Mann (2011), the author wrote that "our analysis of climate model simulations and observational data provide no support for the hypothesis of non-stationary ($H > 1$) stochastic long-term behavior in global mean surface temperature." He also wrote that "there is no obvious reason to appeal to more exotic physics for an explanation of the apparent scaling behavior in observed temperature data." I think that the authors should discuss their results in light of what presented by Mann (2011), so that a reader like me can have a better understanding of the issues at play.

In our work no *'exotic physics'* is used nor do we point to a value $H > 1$ (which, by the way, would be mathematically inconsistent—but this discussion is out of our scope). What Mann says has already been shown by Koutsoyiannis (2002) (e.g. the sum of three Markovian processes is virtually indistinguishable from a process with HK dynamics).

It is unclear to me how a deterministic component, like the Milankovitch cycles, is accounted for in the estimation of H . Please explain.

As we write in the paper *'To demonstrate that the "imperfection" is in fact the result of the Milankovitch cycles acting at time scales in the range of 10^4 - 10^5 years, we have constructed an*

explanatory toy model, which is depicted in Figure 3. This toy model represents the synthesis of the theoretical climacograms of three components, an HK process with $H = 0.92$ and two harmonics with periods 100 and 41 thousand years.'

Figure 2: I am having a bit of a hard time seeing what the authors see. If I were to analyze each series separately, it would be hard for me to come up with an estimate of H of 0.92.

The purpose of this study is not to analyze each series separately but to explore the overall behaviour of climate in different time scales. Therefore, our paper is not supposed to provide different estimates for the different data series.

Figure 3: why are the weights set to be 0.95, 0.30, and 0.15? What is their physical interpretation?

These weights can be interpreted as the contribution of each component to the entire process. This is just an illustration and is not given as a unique set of values. The purpose of the paper is exploratory and explanatory—not to provide the logistic of models. As we write in the paper, *"We note that this toy model is not unique; several other could be constructed by also including a 21 thousand years harmonic and by changing the weights of the three or four components. We selected to present the one in Figure 3 (as an explanatory tool and not as a model to be used in applications), because it is the most parsimonious among those examined."*

Reviewer #2

Markonis and Koutsoyiannis apply a climacogram approach to analyze a collection of climate records ranging from high resolution recent temperature records to low resolution records spanning the Phanerozoic. I was not familiar with the climacogram analysis, but I think it is an interesting analytical tool and am hopeful that a rewritten manuscript can be published. I am not convinced by the present manuscript that the results presented are either robust or useful.

The authors are probably not aware of the work of Shackleton and Imbrie, who used frequency analysis to address the same topic [Shackleton, N. J., and J. Imbrie (1990), The $\delta^{18}\text{O}$ spectrum of oceanic deep water over a five-decade band, *Climatic Change*, 16(2), 217-230, doi:10.1007/BF00134658]. My impression is that the conclusions of Markonis and Koutsoyiannis are not significantly different from those of Shackleton and Imbrie, although in principle the climacogram should provide different insight especially into non-periodic signals. The authors need to demonstrate that they have something new, and specifically contrast their conclusions with those of Shackleton and Imbrie.

Shackleton and Imbrie (1990) could not study the records we study because they were not available in 1990. We have now included, though, this citation in our work, when we acknowledge that '*HK dynamics with $H > 0.5$ has been already identified in some individual temperature reconstructions ...*'. In our paper we demonstrate for first time the presence of HK dynamics at different time scales spanning almost 9 orders of magnitude.

There is also a lack of demonstrated understanding by the authors of the paleoceanographic and paleoclimate literature for 10-100 Myr records. Orbital forcing is modeled as two components with periods of 100 and 41 kyr. The 41 kyr component is appropriate for obliquity, but the 100 kyr component is not an expected strong orbital forcing periodicity. Precession (~21 kyr) would be a better choice, or the stronger 400 kyr eccentricity modulation of precession. It would make sense to me for the authors to use the orbital solution of Laskar as input for their model [Laskar, J., P. Robutel, F. Joutel, M. Gastineau, A. C. M. Correia, and B. Levrard (2004), A long-term numerical solution for the insolation quantities of the Earth, *Astronomy and Astrophysics*, 428, 261-285, doi:10.1051/0004-6361:20041335].

The strong 100 kyr beat in the Pleistocene has always been a problem for the orbital theory of climate; it is not typical of the Cenozoic and is not expected to be typical of the Phanerozoic.

Whether we like it or not, the 100 kyear period is stronger than any other periodicity in the last 1 million years. The 100 kyear beat, has indeed been one of the most significant deviations between data and orbital theory. Many possible explanations have been proposed such as the existence of glacial modulated threshold (Paillard 1998), frequency modulation (Rial 1999), possible CO₂ correlation (Berger et al. 1999) or obliquity modulated threshold (Huybers, 2006), but, as yet, these are speculations and out of the scope of our paper.

In his recent work Paillard (2010) has considered using integrated summer insolation as more physically correct, as it was suggested by Huybers (2006). He underlines that "*the relative weight of 23 000 and 41 000 years periodicities is quite different with such a definition of the astronomical forcing, pleading for a more prominent role of obliquity on climate than usually assumed*". Li and Chen (2007) have reached the same conclusion using an energy index model with insolation as a forcing factor, while a new, more accurate, chronology of the Antarctic ice sheet extend (Kawamura et al, 2007) showed that the last four deglaciations occurred within the rising phase of insolation in the Northern hemisphere, three of which occurred within the rising phase of obliquity as well.

There is also an obvious gap in discussion of theories of climate change on timescales longer than orbital forcing. There is no mention of the Wilson cycle [Worsley, T. R., R. D. Nance, and J. B. Moody (1986), Tectonic cycles and the history of the earth's biogeochemical and paleoceanographic record, *Paleoceanography*, 1(3), 233-263, doi:10.1029/PA001i003p00233].

There is no mention of long-term CO₂ changes and the Berner models [Royer, D. L., R. A. Berner, I. P. Montañez, N. J. Tabor, and D. J. Beerling (2004), CO₂ as a primary driver of Phanerozoic climate, GSA Today, 14(3), 4-10, doi:10.1130/1052-5173(2004)014<4:CAAPDO>2.0.CO;2]. They might also mention superplumes [Larson, R. L. (1991), Geological consequences of superplumes, Geology, 19(10), 963-966, doi:10.1130/0091-7613(1991)019<0963:GCOS>2.3.CO;2] or cosmic rays [Shaviv, N. J., and J. Veizer (2003), Celestial driver of Phanerozoic climate?, GSA Today, 13(7), 4-10]. I doubt the authors will be able to convince readers of the usefulness of their analysis without any mention of proposed mechanisms for climate forcing on timescales longer than orbital forcing.

As we state in our paper, 'This uncertainty is magnified by the fact that, as already mentioned, the orbital forcing cycles are not apparent all the time and are not strictly periodic'. These cycles proposed by the reviewer are even less periodic so that they are actually engulfed in the HK scaling behavior. On the contrary the oscillatory nature of the 41 kyear and 100 kyear cycles is responsible for the emergence of spurious anti-persistence in the 10-100 thousand-year time horizons. Otherwise, the works invoked by the reviewer are irrelevant with our paper.

A crucial problem with the analysis as it is currently presented is the adjustment needed to match the climacograms for the various records (Lines 128-133). Having only Figure 2 to refer to, I am unable to evaluate whether the slope they show is inherent in the data, or simply a function of the adjustment applied to match the climacograms.

As explained in the paper, the normalization has obviously no effect to the value of slope (multiplication by a constant shifts all the data points uniformly). The normalization weights were estimated objectively by minimizing the matching error, as described in Methods. The graphic provides easy means to visualize the fact that the optimized weights worked properly. Besides, the data are available on line, our method and mathematics are extremely simple, and the reviewer can easily reproduce and try to falsify our results.

I can see that some intervals of individual climacograms that have a slope of -0.5. Others show no variation with timescale at all. When the climacograms have been adjusted in an ad hoc manner, the authors must do better than "trust us".

The slope of -0.5 is extensively discussed in the text and depicted in Figure 3. Among other things we state: 'The reason for a flat tail is related to the fact that the entire time series length is located on a branch of the process with a monotonic trend (Figure 1). When a longer time series is viewed (Veizer for Zachos, Moberg and Lohle for CRU), which shows that the monotonic trend is in fact part of a longer fluctuation, the flat climacogram problem is remedied.'

One potential way around this problem would be to put all the different records on a common scale - {degree sign}C. There are records of air temperature derived from ice core

measurements that could be used instead of the d18O values, and a simplistic calculation of temperature from the marine sediment d18O would be adequate for the purposes of this paper (indeed, the Veizer record is really a d18O record, and I was unable to find a version giving temperature at the URL provided in table 1). Some consideration would still have to be made for the difference in temperature at different latitudes, but it might make it easier to reconcile the different curves than having entirely different units. In any case, the authors must make a real effort to show that the adjustment of the climacograms is not the controlling factor for their overall analysis.

Obviously, the shape of the climacogram is not affected at all after any linear transformation (see also Methods). The slope of the standard deviation vs. scale will be *precisely* the same if we convert to same units (the reviewer can readily verify this performing some calculations on a spreadsheet, or even better by making some algebraic manipulations—one or two lines of mathematical derivations suffice). Therefore the comment is not necessary to implement—we have no reason to provide derivative data when the original data will give precisely the same results.

Even accepting that the slope shown in Figure 2 is robust, I do not think that the interpretation given is convincing. They infer that, because the "real climatic variability at the scale of 100 million years equals that predicted by classical statistics for 28 months", there is some problem with differentiating weather from climate. I guess this presumes that the differentiation is based on a difference in variance at weather vs. climate timescales. I have never understood the differentiation in this way, and the authors provide no references for researchers who do differentiate weather from climate in this way. They conclude that the influence of orbital climate forcing is visible in the climacogram, but they don't seem to have considered any other hypothesis, and their interpretation is not structured in any way that would allow a useful statistical test either of the significance of the climacogram shape they seek to explain, or of the model they use to explain it.

It seems that the main conclusion they draw, and one they seem to think is very important, is that climate variability displays Hurst-Kolmogorov stochastic dynamics. As a paleoceanographer, I really can't see how this conclusion - if true - is of any practical use in interpreting the geologic climate record. Nor do I see that it provides any special insight on climate dynamics. I am willing to be convinced that the authors work does one or both of those things, but the authors need to make the case for this manuscript to be publishable.

The reviewer can perhaps allow us to keep our interpretation for the results, which we believe are significant and useful. At the same time he may feel free not to accept our interpretation, criticize our interpretation publicly and say his own interpretation publicly.

Editor's decision on the submission to *Nature Geosciences*

Dear Mr Markonis

Thank you for submitting your manuscript entitled "Climatic variability over time scales spanning nine orders of magnitude". However, we regret that we are unable to offer to publish it in Nature Geoscience.

Because we receive many more papers than we can publish, we must decline a substantial proportion of manuscripts without sending them to referees, so that they may be sent elsewhere without delay. Decisions of this kind are made by the editorial staff when it appears that papers, even when technically correct, are unlikely to succeed in the competition for limited space.

Among the considerations that arise at this stage are the likely interest of a manuscript to a broad readership of geoscientists, the pressure on space in the various fields of interest covered by Nature Geoscience and the likelihood that a manuscript would seem of great topical interest to those working in the same or related areas of the Earth sciences.

In the present case, we have no doubt that your findings will be of inherent interest to fellow specialists. But I regret that we are unable to conclude that the paper provides the sort of broad mechanistic insights into the stochastic and deterministic forcings of climate variability more generally that would be likely to excite the immediate interest of researchers in a broad range of the geosciences. We therefore feel that the present paper would find a more appropriate outlet in another journal, rather than Nature Geoscience.

I am sorry that we cannot respond more positively, and I assure you that our decision does not reflect any doubts about the quality of the work reported. I hope that you will rapidly receive a more favourable response elsewhere.

Yours sincerely

Alicia Newton
Associate Editor
Nature Geoscience