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From: grlonline@agu.org [mailto:grlonline@agu.org]
Sent: Friday, May 19, 2006 5:22 PM
To: dk@itia.ntua.gr
Subject: 2006GL026709 Decision Letter

Dear Dr. Koutsoyiannis:

We have had your manuscript, 2006GL026709, "Long-term persistence and uncertainty on the long term," reviewed for both scientific content and GRL-specific criteria. Based on this evaluation, I cannot consider your manuscript further for publication in Geophysical Research Letters. Although one of the reviews is supportive (reveiwer #2), I am in full agreement with the comments of reviewer #1. Attached below are the review comments, which you may find helpful if you decide to revise the paper and submit it to another journal. I am sorry I cannot be more encouraging at this time.

Formal Review of GRL Submission #2006GL026709 by D. Koutsoyianis and A. Montanari

I was disappointed by this manuscript. Like many papers attempting to attribute "long-term persistence" ("LTP") to climate time series, the authors naively apply statistical tools to data series without a proper recognition of the physics and dynamics underlying the systems which these series represent. A more circumspect analysis would first analyze the behavior of appropriate synthetic time series whose properties are *a priori* known (e.g. output from a theoretical climate model), and test the results of the analysis procedure in such cases, before attempting to argue that actual time series conform to the implausible statistical model of LTP. As discussed below in my review, the claims of existence of LTP in this and related previous studies is likely simply an artifact of long-term deterministic variability that is incorrectly diagnosed as long-term stochastic behavior by appropriately specified statistical model. The fact that GRL has published other poor papers in this subject area e.g. Cohn and Lins (2005) and Rybski et al (2006) is not a justification for the continued publication of such papers. These papers do not advance the field forward, but simply clutter the peer-reviewed literature in this field with half-baked analyses and false conclusions which, as this present submission makes clear, become self-perpetuating.

There is a history in recent decades of scientists from other fields naively and inappropriately applying certain statistical methods to climatic data. The methods may be appropriate in the context of other problems in the fields these scientists come from, but they are not appropriate in general for the analysis of climate data. Most notable are the various attempts to calculate fractal dimensions, scaling laws, Hurst coefficients, and various measures of long-range or 'long-term' persistence ("LTP") from climate time series. Invariably, there is in such analyses no clear recognition of the extensive body of existing literature on statistical modeling of climate behavior based that is based on principles embracing the underlying physics and dynamics. It is of some concern that the citations of past work the authors here use to motivate their application of LTP to climate data come entirely from the hydrology and biological literature. There does not in fact, to my knowledge, exist a body of literature providing an *ab initio* physical motivation for LTP to the long-term behavior of surface temperature or other climate variables.

There are numerous studies going back decades that demonstrate that the natural, stochastic ("noise") variability in surface temperature on interannual and longer timescales is governed primarily short-term autocorrelation (so-called "red noise") associated with the interaction of high-frequency stochastic forcing (i.e. 'weather') with the long timescale response components of the system (e.g. the oceans, cryosphere, land surface processes and biosphere). See e.g. Gilman et al, 1963; Hasselmann, 1976; Wigley and Raper, 1990; Mann and Lees, 1996).

The authors appear to obliquely recognize this. For example, they do discuss Markovian processes and ARMA models, but they fail to recognize firstly that neither MA models nor high-order AR models are faithful to the actual noise processes that impact climate variables, which are best interpreted as a simple short-term autocorrelated "red noise" process which can be represented statistically by an AR(1) model. Such a simple model for climatic noise has the advantages that it is (i) motivated by the actual underlying physics (see e.g. Hasselmann, 1976 and Wigley and Raper, 1990), (ii) parsimonious (characterized by only 1 statistical parameter) and yet (iii) provides a null hypothesis for climatic "noise" that is in fact extremely difficult to in general reject.

However, there is an even deeper problem with the present analysis. The analysis presented by the authors assumnes that all variability is stochastic in nature. Yet the stochastic ("noise") component is only part of the behavior of the system. Much of the variability in surface

temperatures and other climate variables is not stochastic at all, but deterministic, driven by long-term changes in forcing, e.g. changes in radiative balance. The simplest and most obvious example is the seasonal cycle in surface temperatures, which represents the thermal response to changes in solar insolation over the course of the year, and is characterized by a nearly sinusoidal emperature signal which dominates the variance in monthly temperature series. The seasonal cycle is the most obvious example of a completely deterministic signal whose presence would obviously compromise any attempt to estimate the parameters of a stochastic model based on direct application to actual monthly temperature time series. Indeed, this is why the first thing that climatologists typically do before analyzing temperature time series is to remove the seasonal cycle, yieldings residuals that are known as temperature "anomalies". But this, of course, does not remove all of the "signal". There is much else in the data that is almost certainly not stochastic. Much of the decadal and lower-frequency variability is known to represent a response to long-term changes in radiative forcing due both to natural causes (e.g. volcanic and solar forcing) and in the most recent centuries, anthropogenic impacts (anthropogenic increases in greenhouse gas concentrations and anthropogenic tropospheric aerosols). Stochastic models for surface temperature simply represent the "null hypotheses" for detecting these signals. Indeed, the Wigley and Raper (1990) article represented an attempt to use a simple model for the "red noise" stochastic temperature variations as a null hypothesis for detecting a forced (largely anthropogenic) 20^{th} century trend in surface temperatures.

The current paper shows a complete lack of recognition of the distinction between the stochastic and deterministic components of climate variability, both of which contribute substantially to observed records, and both of which will influence the estimation of any statistical parameters from the raw temperature data. Obviously, the presence of a substantial forced "signal" will compromise any analysis which attempts to describe the record entirely in terms of a stochastic process ("noise"). It is almost certain that any apparent "long-term persistence" in the authors analyses is simply an artifact of the presence of deterministic forced long-term variability, which compromises the naive estimation of stochastic parameters directly from the data.

The obvious test that the authors (and other researchers attempting to attribute low-frequency climate variability to "LTP" or related phenomena) must pass is to be able to demonstrate that the LTP analysis procedure works correctly when applied to a realistic synthetic example where the answer is known *a priori* not to be "LTP". In other words, the procedure has to be demonstrated not to yield unacceptably high "false positives". The obvious test would be to use a climate model simulation which contains both the "red noise" stochastic variability of the sort described by Hasselmann (1976), Wigley and Raper (1990), and many others, but also the deterministic forced variability due to long-term, including anthropogenic, radiative forcing changes (e.g. Crowley, 2000). Jones and Mann (2004) describe several such simulations which have been performed, and in several instances the data have been made publicly available. In this case, , we know the physics present in the models and it is not "LTP". Rather, it is described by a combination of AR(1) "red noise" stochastic variability combined with deterministic long-term forced variability. What would the present authors' analysis methods yield when applied to these simulation results? If the analysis indicates the presence of LTP in this case, it reinforces the interpretation that any apparent "LTP" in these series is simply an artifact of contamination of the estimated parameters of what is wrongly assumed a purely stochastic procedure by the authors' process, by deterministic long-term variability. Until the authors can demonstrate that their claims pass this sort of test, it is difficult not to conclude that the claimed LTP is simply an artifact of long-term deterministic variability that is incorrectly diagnosed as long-term stochastic correlations by their procedure.

Some additional comments:

1. The manuscript is unfocused and appears to lack any central hypothesis or conclusions. Much of the paper is devoted to detailed discussion of the concept of LTP. But certain parts, such as the section on "Observation Uncertainty" describe inappropriate specific applications of the concept. The attempt to characterize and explain various different proxy reconstructions of long-term hemispheric temperature variations using the concept of LTP, in particular, is completely nonsensical. First of all, the use of the concept of LTP to characterize hemispheric surface temperature is unjustifiable on physical grounds, as discussed above. It therefore follows that the concept is equally inappropriate for characterizing imperfect reconstructions (from proxy data) of that variable. Equally problematic, the use of LTP to attempt to classify climate reconstructions into different groups simply obscures, rather than enlightens, the true reasons for differences among different reconstructions. The true reasons likely relate to differences in the types of proxy data that have been used in the various reconstructions, the seasonal and spatial representativeness of the various reconstructions, and the differing statistical calibration approaches. See for example the review by Jones and Mann (2004). It is difficult to see how the concept of LTP can provide any possible insight into these issues. There is an entire body of literature which the authors seem to be entirely unware of (see e.g. the Jones and Mann review described above) that provides far more insight into these issues.

2. The paper is overly pedantic, with much of the manuscript used for the detailed development of mathematical formalisms which are surely already widely available in the publish literature cited. This is especially irritating since even cursory review of the existing literature discussing the underlying physical considerations for the problem at hand (understanding surface temperature variations on interannual and longer timescales), renders all of the assumptions made inappropriate anyway.

3. Five of the citations are to web pages!! This is thoroughly inappropriate, and a disturbing trend if it is indeed the case that other scientists are now doing this. The content of a webpage is even less reliable than the typical 'gray literature' (conference proceedings, technical reports, etc.). At least the latter are typically written by scientists with some expertise in the area. A webpage can be created by anybody, without any expertise whatsoever in the topic under discussion. Neither GRL nor any other journal should permit the citation of webpages as supporting material for scientific arguments made in the peer-reviewed literature.

4. The written English here is quite poor, and at times nearly unintelligible. The manuscript would have benefited greatly from a critical reading by a colleague with better English writing skills.

References:

Crowley, T. J. (2000). "Causes of Climate Change Over the Past 1000 Years." Science 289(14 July): 270-277.

Gilman, D. L., F. J. Fuglister, et al. (1963). "On the Power Spectrum of "Red noise"." Journal of the Atmospheric Sciences **20**: 182-184.

Hasselmann, K. (1976). Stochastic climate models part I Theory, Tellus 28, 473-478.

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Mann, M. E. and J. M. Lees (1996). "Robust Estimation of Background Noise and SIgnal Detection in Climatic Time Series." Climate Change **33**: 409-445.

Wigley, T. M. L. and S. C. B. Raper (1990). "Natural Variability of the Climate System and Detection of the Greenhouse Effect." Nature **344**: 324-327.

Long-term persistence and uncertainty on the long term

by Demtris Koutsoyiannis and Alberto Montanari

Reviewed by Harry F. Lins, USGS

Comments:

This manuscript addresses a very old topic that has recently resurfaced and taken on enormous importance in two separate letters in GRL (Cohn and Lins ("CL05") and Rybski et al. ("R06")): How to detect trends in climate data in the context of long-term persistent errors. The underlying issue is whether the observed trend in global atmospheric temperature should be attributed to some causal factor (e.g., increasing carbon dioxide) or might be explained by natural variability. CL05 and R06 agree that long-term persistence is present in temperature (and other hydroclimatic) time series. However, they draw quite different conclusions about the impact of long-term persistence with respect to the attribution question above. The purpose of this manuscript is to sort out and reconcile the differences between CL05 and R06, and, to a great extent, the present manuscript achieves this goal. It provides a rigorous foundation for considering the differences, as well as presenting a much-needed review of the statistical problems that arise when long-term persistence is present.

The manuscript also includes some new material that is very intriguing. Table 1 contains amazing -- almost unbelievable -- results related to the equivalent sample size of the various data sets if we assume that long-term persistence is present. In particular, the 150-year instrumental northern hemisphere temperature record, which exhibits a Hurst coefficient of about H=0.93, contains the equivalent of only about 2 years of equivalent "white noise" information. That seemed incredible, so I checked: The computations are correct. That fact alone should wake people up; the world of long-term persistence -- which both papers concede we're living in -- is not easy to fathom.

One minor issue: Although R06 refers to an MM03 "reconstruction" (column 7 in Table 1), Steve McIntyre has been very clear that he never produced such a series [see http://www.climateaudit.org/?p=577] but, rather, modified the Mann proxy series to illustrate the lack of robustness in results. It would likely be best to re-label column 7 in Table 1 from “MM03” to something like "RBHS/MM03" -- and clarify the confusion in the text (i.e., lines 211-213).

In short, this is a superb manuscript and I strongly recommend that it be published in GRL. In my opinion, other than some editorial minutiae and cleaning up the wording regarding “reconstruction” as noted above, it could be published as is.

Minor Edits:

l. 1 “…on the long term” ==> “…in the long term”
l. 10 "...arrive to disagreeing conclusions." ==> "...arrive at different conclusions."
l. 12 “…uncertainty on the long term…” ==> “…uncertainty in the long term…”
l. 21 "...arrived to similar conclusions." ==> "...arrived at similar conclusions."
l. 45 “…in the climatic…” ==> “…in climatic…”
l. 48 "In this respect, with this Letter we wish contribute our thoughts, ..." ==> "In this Letter we offer several thoughts, …”

l. 49 “…also put emphasis to another closely…” ==> “…also highlight a closely…”
l. 50 “…uncertainty on the long…” ==> “…uncertainty in the long…”
l. 52 "...independent identically distributed..." ==> "...independent and identically distributed..."
l. 54 "...mislead us so as fail to..." ==> "...mislead us such that we fail to..."
l. 87 "...it should be reminded..." ==> "...it should be recalled..."
l. 185 "This point has been already done in some studies." ==> "This point has already been made in some studies."
l. 188 "...cannot not be..." ==> "...cannot be..."
l. 346 "It may have some interest..." ==> "It may be of some interest..."
l. 347 "...double sided..." ==> "...double-sided..."
l. 354 "...continue to be an attracting one in..." ==> "...continue attracting attention in..."
l. 355 “…as newer data will be accumulated.” ==> “…as newer data accumulate.”
l. 403 Date of Koutsoyiannis reference needs to be changed from “(2002)” to “(2003)” Note, however, that the corresponding citations in text appear to be correct.